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Is econometrics relevant to real world economics?

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Abstract
Econometrics has become irrelevant to real world economics and a drag on the discipline. It has changed from being the means to an end to the end itself. Econometrics provides the tools that can be used to prove almost anything and support inappropriate, if not disastrous, policies. Students of economics need to learn more about the real world and the current state of the world economy, as well as economic history and the history of economic thought. Students should be introduced to different approaches to economics rather than insisting that only the current mainstream, econometrics-dependent, approach is the right (or only) way to do good economics.

Introduction

Econometrics provides the statistical and mathematical tools used for the purpose of testing theories and generating forecasts, with the objective of enhancing policy formulation and business decision making. The word "econometrics" consists of two Greek words: *oikonomia* (meaning administration or economics) and *metron*, which means measure (for example, Chait, 1949). In English, the literal meaning of "econometrics" is "economic measurement". Today, econometrics is about more than measurement, and for the sceptics it is effectively "economic-tricks", a collection of "tricks" used by economists to produce evidence supporting their views or prior beliefs.

While the term "econometrics" was used for the first time by Pawel Ciompa in 1910, it was Ragnar Frisch who established the discipline as we know it today (Bjerkholt, 1995). The precursor to econometrics was quantitative research in economics, the origins of which can be traced at least as far back as the work of the 16th-century political arithmeticians who analysed data in their studies of taxation, money and international trade. Econometrics as we know it today began to emerge in the 1930s and 1940s with the advent of the probabilistic rationalizations of regression analysis as formulated by Koopmans (1937) and Haavelmo (1944). Haavelmo (1944) defended the probability approach by arguing that the use of statistical measures for inferential purposes is justified only if the process generating the data can be cast in terms of a probability model.

Recent work in econometrics has been predominantly about the development of new estimation and testing methods without corresponding advances in empirical work on the working of the economy and financial system. Engle (1982) suggested the autoregressive conditional heteroscedasticity (ARCH) model to represent volatility clustering, which opened the flood gates for a non-stop emergence of ARCH-like models. Extravaganza in estimation and testing methods continues, with the development of fancy methods such as jackknife instrumental variable estimation, estimation with over-identifying inequality moment conditions, Bayesian estimation of dynamic discrete models, super-parametric estimation of bivariate Tobit models, quantile regression for dynamic panel data with fixed effects, nonparametric instrumental regression, local GMM estimation, and many more. As for testing, recent developments include testing models of low-frequency variability, unit root quantile regression testing, specification tests of parametric dynamic conditional quantiles, and testing

for common conditionally heteroscedastic factors. Even cointegration, which has proved to be a notion of dubious usefulness, has gone through some recent developments. And there have been more sequels to ARCH than to Jaws, Rocky, Die Hard and Rambo put together. The sequels include IGARCH, MGARCH, TS-GARCH, F-ARCH, AGARCH, LARCH, SPARCH, AARCH, QTARCH, STARCH, NAGARCH, PNP-ARCH, and so and so forth.

The objective of this paper is to argue that econometrics has not led to improvement in our understanding of the working of the economy and financial markets. It is even argued that most of the empirical work in economics and finance is useless at best and dangerous at worst, as it may lead to the confirmation of prior beliefs and consequently disastrous policies such as the wholesale deregulation of the financial system.

The alleged success of econometrics

Econometricians typically hail the evolution of econometrics as a "big success". For example, Geweke et al. (2006) argue that "econometrics has come a long way over a relatively short period". As indicators of the success of econometrics, they list the following: (i) applications of econometric methods can be found in almost every field of economics; (ii) econometric models have been used extensively by government agencies, international organizations and commercial enterprises; (iii) macroeconometric models of differing complexity and size have been constructed for almost every country in the world; and (iv) both in theory and practice econometrics has already gone well beyond what its founders envisaged. Other measures of the success of econometrics include the observation that there is now scarcely a field of applied economics into which mathematical and statistical theory has not penetrated, including economic history. Pagan (1987) describes econometrics as "outstanding success" because the work of econometric theorists has become "part of the process of economic investigation and the training of economists". Yet another indicator of the success of econometrics is the observation of excess demand for well-trained econometricians.

These claims represent no more than self-glorifying rhetoric, which at the limit considers the discovery or invention of ARCH to be as worthwhile of the Nobel Prize as the discovery or invention of Penicillin. The widespread use of econometrics is not indicative of success, just like the widespread use of drugs does not represent social success. Applications of econometric methods in almost every field of economics is not the same as saying that econometrics has enhanced our understanding of the underlying issues in every field of economics. It only shows that econometrics is no longer a means to an end but rather the end itself. The use of econometric models by government agencies has not led to improvement in policy making, as we move from one crisis to another. Constructing models for almost every country in the world has not helped alleviate poverty or solve recurring economic problems.

The observation that econometric theory has become part of the training of economists and the other observation of excess demand for well-trained econometricians are far away from being measures of success. Including more and more statistical and mathematical material in curriculums amounts to squeezing out theoretical and applied courses. As a result of the alleged success of econometrics, courses in economic history and the history of economic thought have all but disappeared from the curriculum. The alleged success of econometrics has led to the production of economics graduates who may be good at number crunching but do not know much about the various economic problems faced by humanity. It has also led to the brain drain inflicted on the society by the movement of physicists, mathematicians and

engineers to economics and finance, particularly those looking for lucrative jobs in the financial sector. At the same time, some good economists have left the field or retired early because they could not cope with the success of econometrics.

The move towards abstraction

Econometrics is no longer about measurement in economics as it has become too abstract. The word "econometrics" is typically stretched to cover mathematical economics and the word "econometrician" refers to an economist, or otherwise, who is skilled and interested in the application of mathematics, be it mathematical statistics, game theory, topology or measure theory. Baltagi (2002) argues that research in economics and econometrics has been growing more and more abstract and highly mathematical without an application in sight or a motivation for practical use. In most cases, however, mathematization is unnecessary and a simple idea that can be represented by diagrams is made much more complex and beyond the comprehension of the average economist, let alone policy makers.

Heckman (2001) argues that econometrics is useful only if it helps economists conduct and interpret empirical research on economic data. Like Baltagi, Heckman warns that the gap between econometric theory and empirical practice has grown over the past two decades. Although he finds nothing wrong with, and much potential value in, using methods and ideas from other fields to improve empirical work in economics, he does warn of the risks involved in uncritically adopting the methods and mind set of the statisticians. Econometric methods adapted from statistics are not useful in many research activities pursued by economists. A theorem-proof format is poorly suited for analyzing economic data, which requires skills of synthesis, interpretation and empirical investigation. Command of statistical methods is only a part, and sometimes a very small part, of what is required to do useful empirical research.

The trend towards more abstract work can be seen in the contents of *Econometrica*. In the 1930s and 1940s, *Econometrica* published papers on economics, dealing with microeconomic issues like the demand for boots and macroeconomic issues like the multiplier effect of a balanced budget. In the 2012 and 2013 volumes, most of the papers are too abstract, use no data and do not provide new econometric methods that can be used in empirical work. In particular there is a high frequency of papers on game theory, which is supposed to be a branch of mathematics. Recent issues of *Econometrica* are dominated by what a frustrated academic economist once called "data-free mathematical masturbation", suggesting that it was not his "source of enlightenment" (Mason et al., 1992). This is why a joke goes as follows: during the rule of Nicolai Ceausescu in Romania, the government banned all "western" economics journals – the exception was *Econometrica* because it had nothing to do with economics.

Econometrics has been a success only in the limited sense that it can be used to prove almost anything, providing a bag of tricks *à la* Felix the Cat. Econometrics is very useful for those wanting to prove a prior belief or find results that support an ideologically-driven hypothesis. Take, for example, Brexit, which had proponents and opponents. The empirical results produced by the opponents on the effect of Brexit on the British economy of leaving the EU are all over the place but ideological bias is conspicuous. For example, the Confederation of British Industry (2013), which is against Brexit, estimated the net benefit to Britain of EU membership to be in the region of 4 to 5% of GDP – that is, between £62 billion and £78 billion per year. Conversely, Congdon (2014) puts the cost of Britain's membership of

the EU at 10%, attributing this cost to regulation and resource misallocation. Congdon's estimates were prepared for the United Kingdom Independence Party (UKIP), which has a strong anti-Europe stance.

Econometrics has been used to make outrageous claims and justify draconian economic policy. Econometrics has been used to justify inequality and defend the top 1%. Econometrics has been used to justify tax cuts for the rich and support the trickle-down effect, which is used to justify the redistribution of income in a reverse-Robin-Hood manner. Econometrics has been used to support the so-called "great moderation" and justify wholesale financial deregulation, the very policies that have led to growing poverty and social misery. Econometrics has succeeded in one sense – it has succeeded as a con art, enabling anyone to prove anything.

Econometrics as a science

Econometrics looks "sciency". Once in a seminar presentation I displayed two equations, one taken from *Econometrica* and the other from the *Journal of Theoretical and Experimental Physics* and challenged the audience to tell me which is which. No one volunteered to tell me which is which, including at least one hard-core econometrician. Economics is a social science where the behaviour of decision makers is not governed purely by economic considerations but also by social and psychological factors, which are not amenable to econometric testing. This is why no economic theory holds everywhere all the time. And this is why the results of empirical testing of economic theories are typically a mixed bag. And this is why econometricians use time-varying parametric estimation to account for changes in the values of estimated parameters over time (which means that the underlying relationship does not have the universality of a law). And this is why there are so many estimation methods that can be used to produce the desired results. In physics, on the other hand, a body falling under the force of gravity travels with an acceleration of 32 feet per second per second – this is true anywhere any time. In physics also, the boiling point of water under any level of atmospheric pressure can be predicted with accuracy.

Unlike physicists, econometricians are in a position to obtain the desired results, armed with the arsenal of tools produced by econometric theory. When an econometrician fails to obtain the desired results, he or she may try different functional forms, lag structures and estimation methods, and indulge in data mining until the desired results are obtained (torture produces a confession even when applied to data). If the empirical work is conducted for the purpose of writing an academic paper, the researcher seeks results that are "interesting" enough to warrant publication or results that confirm the view of the orthodoxy. And it is typically the case that the results cannot be replicated. Physicists do not have this luxury – it is unthinkable and easily verifiable that a physicist manipulates data (by using principal components or various econometric transformations) to obtain readings that refute Boyle's law. Economists study the behaviour of consumers, firms and governments where expectations and uncertainties play key roles in the translation of economic theory into real world economics. These uncertainties mean that econometric modelling cannot produce accurate representation of the working of the economy.

Two of the characteristics of a scientific discipline are identified by von Mises (1978) and Schumpeter (1978). For von Mises, a scientific method requires the verification of a proposition by numerous sets of data pertaining to sufficiently comparable situations. For

Schumpeter (1978), correct prediction is the best or only test of whether a science has achieved its purposes, which means that correct prediction (within the bounds of what one can reasonably expect of an uncertain future) is a requisite for scientific status. Kearns (1995) argues that the two characteristics of a scientific discipline noted by von Mises and Schumpeter are found in econometrics. It is not at all clear how these characteristics are found in econometric work. The results of empirical work are typically irreproducible and contradictory, while econometric forecasting is no less than a fiasco – see, for example, Moosa (2017) for illustrations. A look at the literature on exchange rate economics gives us an idea of how bad econometric forecasting is. We can predict precisely when a falling object will hit the ground and where a projectile will land, but we cannot predict with a reasonable level of confidence whether a currency will appreciate or depreciate on the announcement of unemployment data – that is, we cannot even predict the direction of change, let alone the magnitude of change.

Hendry (1980) contends that it is possible to verify results consistently by using "rigorously tested models, which adequately described the available data, encompassed previous findings and were derived from well based theories". This makes sense but the reality of econometric testing is far away from Hendry's description. While it is possible that a proposition can be verified, the same proposition can be rejected by using a different set of data, econometric technique or model specification. I am yet to see a hypothesis in economics or finance that has been supported or rejected universally. Take any literature review on any topic in economics and you will quickly reach the conclusion that the results are a mixed bag (try purchasing power parity or the J-curve effect).

An argument that can be put forward in favour of the proposition that econometric work represents a scientific endeavour is based on the desirable properties of econometric models as identified by Koustoyannis (1977). The desirable properties are (i) theoretical plausibility, in the sense that the model must describe adequately the underlying economic phenomena; (ii) explanatory ability, in the sense that the model should be able to explain the observations of the real world; (iii) accuracy of the estimates of the model parameters, which should converge as far as possible on the true parameters of the model (that is, they should be efficient, consistent and unbiased); (iv) forecasting ability, as the model should provide satisfactory predictions of future values of the dependent variable; and (v) simplicity, as the model should represent the economic relations with maximum simplicity. Anyone who has found an econometric model that meets these criteria should be given the next Nobel Prize in economics (although this does not say much, given that the Prize has been awarded for nonsense).

Unlike the models of science, econometric models typically fail to explain what happens in the real world, let alone predict what may or can happen. Blommestein (2009) refers to the "common situation where the empirical results of different studies of a similar topic have often a very wide range of outcomes and values for structural parameters" (and without a convincing or clear explanation why this is the case), arguing that "such a situation would be unthinkable and unacceptable in the physical sciences". If a physicist obtains different outcomes when addressing a similar problem, this would be a key reason for an urgent scientific debate until the discrepancy in results has been resolved. Unlike scientists, Blommestein argues, "economists are prone to an attitude where they stick to their favourite theories and models come hell or high water and where no mountain of evidence can move them".

Econometrics is not a science because economics is not a science, at least not in the same sense as physics is a science. The desire to elevate econometrics and economic theory to the status of science may be motivated by some sort of inferiority complex. Ritholtz (2009) emphasizes this point by arguing that "economics has had a justifiable inferiority complex versus real sciences the past century". The science-like quantification of economics has created barriers to entry into the economics profession, impeded endeavours to integrate economics with other social sciences and learn from them, led some good non-quantitative economists to leave the profession, produced brain drain by attracting people from science and engineering, and led "scientific economists" to follow empirical results blindly, sometime with serious adverse consequences (it was all fine before the global financial crisis!).

Loopholes and shortcomings

Empirical work in economics is criticized on several grounds. To start with, the results of empirical work are sensitive to model specification, definitions of variables, sample period, estimation method, and data transformation. Hence econometric testing can be used to prove almost anything because the researcher (by manipulating the underlying model) is bound to find some results that support a prior belief or an agenda of some sort. The use of atheoretical models makes the task of obtaining the desired results even easier, as the researcher is not constrained by a particular theory-based specification. The search for "good" results makes it tantalizing to indulge in data mining, involving the estimation of thousands of regression equations and reporting the most appealing one or more. On the other hand, the empirical results may be insensitive to the estimation method and model specification, which casts doubt on the usefulness of "sophisticated" econometric estimation methods. For example, Moosa (2003; 2011) and Maharaj et al. (2008) demonstrate that the use of estimation methods of various degrees of sophistication does not make any difference for the estimation of the hedge ratio and hedging effectiveness, because what matters is correlation.

When empirical work involves the testing of a hypothesis on a time series basis for a large number of countries or industries, a problem arises in the form of unexplainable cross-sectional differences. For example, Bahmani-Oskooee and Alse (1994) found a mixed bag of results when they tested the J-curve effect for 19 developed and 22 less developed countries. The same thing applies to the estimation of Okun's coefficient (for example, Moosa, 1997).

Empirical work may be based on dubious tests and procedures. Econometrics provides estimation and testing methods that enable a researcher to prove almost anything and to make any model look good. A prominent example of a test that enables anyone to prove anything is the Johansen test for cointegration, which (fortunately) has gone the way of the dinosaurs. This test over-rejects the null of no cointegration and produces results that are sensitive to the specification of the underlying model, particularly the lag length. Given confirmation and publication biases – that is, the desire to produce results that do not reject the underlying hypothesis so that the results can be published – this procedure has become a useful tool for producing desirable but misleading results. As for procedures that make any model look good, try the Cochrane-Orcutt correction for serial correlation. By using this procedure to estimate a regression equation, the results change dramatically from those produced by using OLS: an R^2 of 0.99 and a DW statistic close to 2 – that is, perfect results.
A major problem associated with empirical work is deriving inference on the basis of correlation as if it were causation. Econometricians came up with an answer when Clive Granger devised a test for causality based on temporal ordering – something causing

something else because the first something occurs before the second something. Subsequently, many variants of the Granger causality test appeared, allowing economists to test the same hypotheses over and over again without reaching any conclusion. The notion of causality is ludicrous, a fallacy that is sometimes described as *post hoc ergo propter hoc*, which is Latin for "after this, therefore because of this". The development of causality testing follows from the desire to make economics physics-like. In physics we know that force causes motion, but in economics we depend on the misleading causality testing to find out whether inflation and the current account cause the exchange rate or vice versa. Of course we can prove anything we want by changing the lag structure of the underlying VAR. And in all of this, economists do not bother presenting a narrative as to why X causes Y – we simply have to trust the results of the Granger causality test or those produced by its disciples.

Then there is the problem of spurious correlation. For example, Beard et al. (2011) find that reducing the total budget of all U.S. federal regulatory agencies by 5% produces 1.2 million private sector jobs each year. They argue that firing one regulatory agency staff member creates 98 jobs in the private sector. These results sound ridiculous, most likely the product of extensive data mining motivated by an ideological anti-regulation stance. Naturally, Beard et al. do not tell us anything about the mechanism whereby the firing of a regulator leads to job creation. This is an example of spurious correlation, resulting from the interpretation of a multiple regression equation. In reality it is common sense, not econometrics, that tells us whether correlation is spurious or genuine – so, it is unfortunate that we have decided to ditch common sense in favour of econometrics.

The significance level is yet another problem associated with empirical work in economics. A regression equation containing 15 explanatory variables or so is typically estimated with a menu of stars to indicate the significance level (* for 10%, ** for 5% and *** for 1%), but we are not told what to consider to be statistically significant. How about going half a star for 20% or six stars for 0.5%? The choice of the significance level has been recognized in the finance literature. Harvey et al. (2015) suggest that studies of the so-called asset pricing models involve extensive data mining, arguing that "it does not make any economic or statistical sense to use the usual significance criteria for a newly discovered factor" (that is, a t-ratio greater than 2). Accordingly, they argue that "most claimed research findings in financial economics are likely false". Likewise, Kim (2016), Kim and Choi (2016) and Kim and Ji (2015) observe the use of conventional significance level without due consideration given to factors such as the power of the test and sample size, which makes them sceptical of "research credibility and integrity". The one-million-dollar question is the following: what hurdle should be used for current research?

Last, but not least, we have the problem of omitted and unmeasurable variables. The problem of omitted variables is particularly relevant when a model is not theory-based, particularly models estimated from cross-sectional data. In the absence of a theoretical model there is no guarantee that all of the relevant explanatory variables are included in the model. Sometimes, an explanatory variable is excluded deliberately because it cannot be measured. When a relevant explanatory variable is excluded from the model, the results will be biased in the sense that the model compensates for the missing variable by over- or underestimating the effect of one of the other variables.

Econometrics and policy prescriptions

Goertzel (2002) criticizes the use of econometric modelling to evaluate the impact of social policies, given that multiple regression cannot be used to distinguish between correlation and causation. Some of the studies that use econometric modelling to make microeconomic and policy recommendations have produced results telling us the following (all are based on U.S. data): (i) every time a prisoner is executed, eight future murders are deterred; (ii) a 1% increase in the percentage of a state's citizens carrying concealed guns causes a 3.3% decline in the murder rate; (iii) 10 to 20% of the decline in crime in the 1990s was caused by an increase in abortions in the 1970s; (iv) the murder rate would have increased by 250% since 1974 if it were not for the building of new prisons; and (v) the welfare reform of the 1990s would force 1,100,000 children into poverty. I guess that any physicist will be envious of the precision of these numerical results – they do not even come with probabilities, and this is how they are presented to policy makers.

According to Goertzel (2002), "if you were misled by any of these studies, you may have fallen for a pernicious form of junk science", the use of econometric modelling to evaluate the impact of social policies. He goes on to describe these studies as "superficially impressive", "produced by reputable social scientists from prestigious institutions", "often published in peer reviewed scientific journals", and "filled with statistical calculations too complex for anyone but another specialist to untangle". These studies are supposed to give precise numerical "facts" that are often quoted in policy debates, but the "facts" turn out to be fiction sooner or later. He goes on to say the following: "often before the ink is dry on one apparently definitive study, another appears with equally precise and imposing, but completely different, facts and that "despite their numerical precision, these facts have no more validity than the visions of soothsayers".

These studies have serious implications in the sense that the results provide justification for draconian policies. They imply that capital punishment is moral despite the possibility of a miscarriage of justice. They imply that carrying concealed guns should be encouraged despite the horrendous murder rate in the U.S. They imply that there is nothing wrong with the U.S. providing accommodation for 25% of the world prison population. And they imply that the fate of children should be left to the almighty market. Empirical studies based on multiple regression analysis have been used, or can be used, to justify evils like slavery and war as well as the right-wing obsession with deregulation.

Although the results of these studies are fragile and purpose-designed, they are believed as "facts" that can be used to formulate policies because the starting point is that they are the right policies to follow. Even if other studies produce contrasting evidence, the original results remain the basis of policy formulation. For example, Lott and Mustard (1997) reach the conclusion that carrying concealed guns is a deterrent to crime, which is music to the ears of the gun lobby. Even better, carrying concealed guns deters violent crime without causing any increase in accidental death. Recently one person, who killed and injured tens of innocent concert-goers in Las Vegas, proved (without econometrics) that more guns lead to more homicide, not the opposite. A month later another person proved the same (without econometrics) by shooting dead 26 worshipers in a church in Texas. Yet, President Trump seems to believe the empirical evidence of Lott and Mustard as he refuses to connect the killings with the ease of obtaining fire arms.

Summers (1991) has criticized econometric formalism as applied to macroeconomics, arguing that "the empirical facts of which we are most confident and which provide the most secure basis for theory are those that require the least sophisticated statistical analysis to perceive". He examines some highly praised macroeconometric studies (Hansen and Singleton, 1982, 1983; Bernanke, 1986), arguing that while these papers make a brilliant use of econometric methods, they do not prove anything that future theory can build on. Noting that in the natural sciences, "investigators rush to check out the validity of claims made by rival laboratories and then build on them", Summers points out that this rarely happens in economics, which he attributes to the fact that "the results [of econometric studies] are rarely an important input to theory creation or the evolution of professional opinion more generally". Summers criticizes the use of econometrics in macroeconomics on the grounds that it involves confusion between causation and correlation, the use of mathematical equations in preference to verbal exposition, and the use of statistics rather than experiments.

Another economist who is rather critical of the use of econometrics in macroeconomics is Donald Kling (2011) who argues that "macroeconometric models are built on astonishingly precarious grounds and yet are used by policy makers to project precision and certainty". In particular he is critical of the use of lagged dependent variables, add factors, and other techniques to make their models more "accurate" at the expense of integrity. The reason for the unscientific nature of macroeconometric models is that, unlike the objects of controlled experimentation, real-world events are often unique and non-repeatable. He also refers to the sensitivity of the results to model specification and similar factors, arguing that an almost limitless number of factors could affect key macroeconomic variables, there are several potential specifications for the variable representing that factor. He refers to linear versus nonlinear specifications, detrended versus trended data and current versus lagged data.

One has to admit that the econometrics establishment has done rather well in defending and preserving the status of their approach to econometrics. They have prevailed despite serious criticisms by the likes of Edward Leamer, J.M. Keynes and Ludwig von Mises. They have prevailed although the most important contributions to economics have been made without the use of econometrics. What would Adam Smith say if he were alive in the "econometrics age"? One thing that we know for sure is that Smith would not be able to publish even in a mediocre journal, given his limited knowledge of econometrics.

Concluding remarks

Because of the emphasis placed on econometric and quantitative analysis, modern economists cannot say anything useful about the real world, because they talk in a language that is incomprehensible to non-economists, let alone down-to-earth economists. Students and many employers feel that the typical economics graduate today receives training that is irrelevant to understanding real economies, incomprehensible to the target audiences for economic advice, and often just plain incorrect. This situation can be dealt with by following a "back to the future" approach. Students need to learn more about the real world. They need to know about the current state of the world economy, as well as economic history and the history of economic thought. Students should be introduced to different approaches to economics rather than insisting that only the current mainstream approach is the right way to do good economics because it is amenable to quantification.

Unfortunately, the true believers are adamant that econometrics is contributing to human welfare. For example, Magnus (1999) argues that "econometricians can continue to make important contributions and eventually, perhaps, become respectable scientists". What important contributions have econometricians made? Cointegration, causality and ARCH/GARCH models? The contributions of econometricians is that they have provided tools that allow anyone to prove anything. Econometrics is not a science, perhaps it is junk science, but more accurately it is an art, a con art to be specific. It has no relevance whatsoever to real world economics.

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The fiction of verifiability in economic "science"

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Abstract
Today's economic theory is unverifiable. The argument justifying the claim is as follows. Economic theory makes predictions about equilibrium positions. To verify such predictions, we need equilibrium data. Since, hitherto, we have no way of knowing if the data we use in empirical work is equilibrium data, all tests that have hitherto been conducted to verify economic theory are non sequitur.

Key words methodology, economic statistics, econometrics

JEL classification B41, C00, C01

I. Economic theory is unverifiable

The central argument of this article can be stated in a few sentences. Let me provide it sharply to begin with, and then add the requisite qualifications. The proposition is "Today's economic theory is unverifiable".[2] The argument justifying the claim is as follows. Economic theory makes predictions about equilibrium positions. To verify such predictions, we need equilibrium data. Since, hitherto, we have no way of knowing if the data we use in empirical work is equilibrium data, all tests that have hitherto been conducted to verify economic theory are non sequitur.

As the argument is short and elementary, let me break it down into its component parts.[3]

- Economic theory makes predictions about equilibrium positions. This is common knowledge and requires only a brief review.
- To verify such equilibrium predictions, we need equilibrium data. This is the central inference I draw from the above. It is both immediate and obvious.
- We have no way of knowing if the data we use in empirical work is equilibrium data. This is a practical point, only to be judged by the looking at the data generating process and the data collecting procedures.
- Hence, all tests that have hitherto been conducted are non sequitur.

There is something indecent in accusing an entire profession of engaging in a global non sequitur. Good manners require that one at least address the question: how did we get into this mess? Here is my best guess. The early empirical studies were based on agriculture, a field where there used to be only one annual major crop. Once the harvest comes in and the size of the crop is accepted, prices reach equilibrium fairly fast and stay predictable for a while. Those who reported the price had enough sense to let the market settle down before

[1] I am grateful for helpful comments to Anis Chowdhury, M G Quibria, and M A Taslim as well as the participants at several conferences: Illinois Economic Association (2014), INEM (2015) and the SEA (2018). All errors are mine.
[2] With the corollary that "economics cannot claim to be a science", in the sense of a systematic study capable of empirical verification and accurate prediction.
[3] I will make many critical comments on the profession, so let me begin by saluting two individuals who were steadfast in their intellectual integrity: Franklin M. Fisher and the late Zvi Griliches. This essay is meant to show my respect for them.

reporting the price, which was thus a plausible equilibrium price. In such a world, it makes sense to think of the market price as the equilibrium price.

Section II goes over the above claims more carefully. Logically, that ends the paper. However, it is intellectually unsatisfactory to note incompleteness in an accepted argument and place a burden upon others without providing some concrete reasons for doubt. Section III addresses this point by examining the law of one price. Section IV suggests that the doubts expressed earlier should give particular qualms to macroeconomists Section V is a light-hearted look at the burden placed upon the "optimising agents" who are supposed to ensure equilibria. Section VI summarises and concludes.[4]

II. The argument amplified

Economic theory provides predictions. Such predictions arise from comparative statics or CS. If the prediction is qualitative, then the confirmation is weak e.g. "Tighter supply leads to higher prices and lower sales". If this is all that can be said, we can join company with every milkman in producing "science". It is a very low bar, and familiarity with historical documents will show that such "science" was known to multitudes of illiterate humans, such as tribal chiefs in Africa and peasants in India. There is a school of economics, the Austrian school, who claim that only patterns can be predicted, (much as in biology) and that knowledge of such patterns suffices to make economics scientific. Hayek is probably the best known proponent of such a view. Arguing about what is *really* science is pointless and needless for my purposes. Mainstream graduate economics is not based on such views of science as patterns – so I will not engage with such a view here.

"Graduate School Science" demands that a prediction be exact. For example, "A 10% decrease in supply causes price to rise by 8%." This claim is obtained through Comparative Statics or CS. Predictions relate to *equilibrium* values. The prediction quoted above, "A 10% decrease in supply causes price to rise by 8%" is loose. A more exact version of the prediction will state that "If the market is initially in equilibrium, and if it reaches a new equilibrium, then a 10% decrease in supply causes price to rise by 8%." The method is called "comparative statics" because it compares positions of equilibrium, so the claim that predictions in economics refer to equilibria should not require elaboration.

To test such a prediction, we need equilibrium data. i.e. *The numbers used for testing **must** be equilibrium values.* If we do not provide assurance that the data used for empirical tests are actually equilibrium values, our tests are sub judice or non sequitur. Are the data currently used for testing at all relevant? Strictly speaking, NO – unless we have devised some tests for data to be equilibrium data, and the data to be used for testing have passed those tests. To claim that economic data have equilibrium properties, we need to show that data collection is based on an understanding of what will constitute "equilibrium" for each type of data collected. No one has engaged directly with this question, though many have expressed unease about the relationship between economists and data. Most data are collected for administrative

[4] The thoughts presented here have been in my mind since 2007 (see footnote 5 below). Thinking I must have missed something essential, I wrote to many economists. Only Ed Leamer was kind enough to acknowledge that I had a point, but he doubted its empirical significance. To my mind, we can only answer the question of empirical significance if the subject is properly studied, rather than being ignored, as at present. By 2013 I decided that no one would give me a direct reply, so I returned to the issue and started presenting my ideas at seminars. I hope this explains the patchwork manner of references given here.

purposes – imports, exports, taxes etc.; perhaps accounting needs would be a more exact characterization of the data collecting process. What bearing do such collections of numbers have on economic equilibrium?[5]

Can we *never* claim our data to be equilibrium values? There is only one case I can think of in which we can avoid the criticism of testing with data of unproven relevance. It consists of the assumption, (or, better yet, demonstration), that *all* data are equilibrium data. Such a move would "save the day" by making any and all data ever assembled to be relevant for *some* prediction. This is the *hara kiri* gambit. Let us apply the perpetual equilibrium argument to the oldest question in economics – demand and supply. Demand and supply curves are determined by given parameters for the exogenous variables.[6] Given such values, demand and supply curves become well-defined, and equilibrium price and quantity are known. Since prices and quantities are endogenously determined, and everyday observation shows us that they are visibly moving, the *hara kiri* gambit entails the claim that exogenous variables are in some form of continual motion. Those who are willing to accept the last characterization may have trouble explaining why one is interested in separating endogenous from exogenous variables.

Strictly speaking, the above is a complete, but negative, argument. The burden of proving that the economic data being used are in fact equilibrium data is not mine, but that of those who use such data. Using the perpetual equilibrium assumption solves the difficulty, but it does so by fiat. Since it is making a claim about real world data, the *hara kiri* gambit denies any virtue to the price system, which has been considered an equilibrating device par excellence for centuries. According to *hara kiri*, there is nothing to find, since equilibrium is always there. On the other hand, if equilibrium is not true everywhere and always, then those who use data are required to demonstrate why the data that they do use has any relevance for testing equilibrium positions.

III. The prevalence of disequilibria

This section provides some reasons for doubting the assumption that our data are equilibrium values. It arose from an inability to verify a simpler version of equilibrium, the law of one price, or LoP hereafter.

- The law of one price states that the same commodity cannot have two prices in the same market.
- The claim sounds trivially true.
- Now look at all the papers that have been written to empirically test LoP – at least a half, perhaps two-thirds, claim that the law is not confirmed.
- In an earlier note[7], I showed how the failure of LoP occurred at several levels of aggregation by tracing the failure of LoP though successive levels of disaggregated models.
- Whether or not the law is "true", in some exact (ontological) sense yet to be determined, is beside the point. Can LoP be verified *with the data available to us*? This is the real question.

[5] Of all the papers I have read, Fischer Black's charmingly iconoclastic presidential address comes closest in spirit to my message here.
[6] Such parameters need not be scalars, as when they represent "taste" or "technology".
[7] Rashid, 2007.

Failure to verify the LoP is sufficient for my claim, which states that the data we have at hand, which we use every day, and which we have been using, do not justify being considered "equilibrium values"[8]. In one of the most important papers written at the start of the modern quantitative revolution in economics, Trygve Haavelmo laid down some guidelines which point directly to my claims[9].

> "The economist… often has to be satisfied with rough and biased measurements. It is often his task to dig out the measurements he needs from data that were collected for some other purpose;… his task being to build models that explain what has been observed. The practical conclusion of the discussion above is… that one should study very carefully the actual series considered and the conditions under which they were produced, before identifying them with the variables of a particular theoretical model."

Once we recognize that the testing of economic theory requires comparative statics, hence *equilibrium* data, Haavelmo's caution about studying "very carefully the actual series considered and the conditions under which they were produced", leads directly to my point about the need to test our data for LoP.

Let me repeat, the responsibility for establishing the equilibrium property of the data is not mine, but of those who use the data for testing equilibrium theory. However, the widespread failure to confirm it is an intellectual curiosity worth some discussion.

Since LoP fails in a multitude of cases, we have to say that at least one of its underlying assumptions fails. But LoP is based on:

1. Greed
2. Homogeneity of goods
3. Speedy move to equilibrium

The literature to date has focused upon the fact that almost all data are aggregated to some extent, hence the culprit is point 2, the homogeneity of goods. A small number of papers have addressed point 1, which consists of showing that small optimization errors can have large effects.[10] I turn now to the questions raised when we consider the speed of convergence.[11]

LoP can fail tests if there is no equilibrium, or if equilibrium is reached in a slow or fluctuating manner. How bad is the potential failure of LoP due to such phenomenon? The difficulty here is that once one turns a skeptical eye to the literature of applied economics, examples of disequilibria that continue through periods of data collection – which is the relevant standard for this question – seem to be all around us. What empirical economists seek are markets where prices converge quickly *and* monotonically to their equilibrium values. Even if prices converge quickly, but do so with violent oscillations, we will have trouble relating the measured value to the equilibrium value. Applied economists acknowledge the importance of disequilibria and of rates of convergence and frame their research to avoid issues created by

[8] Section 3 of Rashid (2007), provides evidence for the claim that the LoP is confirmed only for a very limited class of commodities, even after disaggregating to the maximal extent we appear capable of.
[9] Haavelmo, 1944, p. 7. There are further relevant observations on pp. 15 and 16.
[10] Akerlof & Yellen, 1985.
[11] I once thought of estimating speeds of convergence in individual markets, with finely defined goods, but correspondence with some very helpful BLS staff persuaded me that such an effort would probably be inconclusive. The details are given in Appendix A.

oscillating prices or by disequilibria. But they do not seem to appreciate that their bread and butter tools of numerical evaluation are directly affected by disequilibria in the macro markets whose impact upon the market they are studying cannot be ignored. Consider the three principal prices used in applied economics, say benefit-cost analysis: wages, interest rates and exchange rates.[12]

- Wages have a large "human" component and adjust slowly in many, if not most, cases.[13]
- Exchange rates, also often "managed", can have "perverse" effects, as in the J curve, before reaching new equilibria.[14]
- Interest rates are not primarily market driven, but anchored by the Fed.

The contrariness of price convergence is compounded by looking beyond the standard models and institutions, which do not assume the necessary "stability" of our models or the force of profit maximisation.

- Chaos and catastrophe theory are two alternative ways of looking at the world, both are quite different from the neoclassical view, and each provide so many examples of non-monotonic convergence that one is at a loss to pick a "favorite" example.[15]
- We cannot rely on profit to provide speedy adjustments in those sectors that are not based on profit maximization. Few will argue that Government functions like a competitive enterprise while the Health sector has many profit seeking enterprises but is rife with the problems detailed in the pioneering paper of Arrow (1963) What is the force leading to convergence when profit is not providing the energy? Such sectors, whose combined share of GDP is about 50% in the USA, or half the economy, will confound any claim that our data are equilibrium data. Neither Government nor Health, considered as individual sectors, can be expected to provide the data we expect from profit maximization; furthermore, as these sectors are large, their general equilibrium effects upon the economy can be significant.[16]

The widely observed fact that many investors are infrequently active traders is the theme of Duffie's Presidential address. It leads to the "key implication" that supply or demand shocks must be absorbed on short notice by a limited set of investors. Since shocks have to be absorbed by a limited number of traders, prices move excessively initially. "As a result, the initial price impact is followed by a gradual price reversal".[17] In some markets, such as that for catastrophic insurance, these price reversals can occur over several months. In explaining such inattention Duffie concisely states:[18] "A simple explanation is that trading takes time away from valuable alternative activities." The ambiguity of this statement needs exploring. If

[12] The literature on both wages and exchange rates is vast, and I have given only a few references for illustration.
[13] Jardim et al., 2019. This paper provides a welcome empirical antidote to the literature suggesting rigid wages, but it does not dispute the point needed here, i.e. slow adjustment. Further evidence on this point comes from Grigsby et al., 2019 and Hall & Kudlyak, 2019.
[14] The estimated half-life to convergence, not full convergence but just half the distance, is estimated at over a year (Bergin et al., 2017).
[15] Rosser, 2000.
[16] If William Lazonick's careful arguments about corporate organizational form being a device to increase control by owners is correct, then even the profit maximizing thrust of corporations is put in doubt, and with it the force of profit maximization as validating equilibrium based on market fundamentals (Lazonick, 2017)
[17] Duffie, 2010.
[18] Ibid. p.1238.

the more valuable activities are economic then the neoclassical model holds, but if the more valuable activities are social or cultural, then a substantial concession has been made, especially since a telling example of slow trading occurs when traders took an hour off to listen to Tiger Woods' televised confession of infidelity.[19]

That the problem of quick and monotonic convergence is probably of fundamental import can be gauged by looking at one area where the LoP should be rapidly validated – financial markets. In financial markets prices move rapidly, the goods are homogenous, and traders are clearly motivated to maximise their wealth. Nonetheless, many clearly documented cases of anomalies appear. Since markets reward those who make the most money, one would expect the actions of superior managers to be particularly profitable. Daniel Kahnemann provides some striking evidence to the contrary.[20] One would also expect that profit making opportunities cannot exist much beyond the span of time needed to exploit them; Richard Thaler has been carefully documenting cases where profit seekers appear to lie waiting.[21] In conclusion, note the disturbing fact that some of the failures of speedy market convergence are the result of the very institutions that perform arbitrage and whose actions are supposed to make LoP true.[22]

A significant part of the problem with the usual defense of markets lies in the method by which economists can simply claim "if a profit can be made, someone will make it". We are never told who this someone is or how they will even know that the opportunity exists. In the language of mathematics, all such proofs are not "constructive", i.e., they do not provide a step-by-step account of the process by which profitable opportunities are realized. For a while, market believers claimed that markets "see through" accounting forms, hence different accounting schemas cannot stop market efficiency. However, many of these same market believers lobbied hard, and succeeded, in getting accounting procedures revised, so as to be more amenable to investors. This activity tells us that accounting forms do matter. But if so then need to understand their construction and use as well as in the self-interest of the actors involved. Larry Cunningham and Stephen Penman have been criticizing current accounting practice on just these grounds.[23]

For data to be relevant for tests of equilibrium theory the requirement imposed upon empirical economists is to find data which result from fast *and* monotonic convergence. We not only need to claim that prices move rapidly to equilibrium, but also that the path is rapidly damped. If prices can have large fluctuations even as they get close to equilibrium, then nothing has been solved. The convergence of prices to equilibrium has to be both fast *and* monotonic for the data to be relevant for testing economic theory. Our confidence in the data generating process arises because we believe in the force of profit maximization, in the impact of competition, and effective arbitrage. But even if we believe that competition will move us *towards* an equilibrium, nothing tells us *how fast* we will be so moved. An equilibrium reached in a decade is of minor interest while an adjustment that is quick but very volatile also makes it impossible to use observed values as equilibrium values. Franklin Fisher stated the difficulty beautifully.[24]

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[19] In itself a telling commentary on the cultural homogeneity of this group of profit seekers.
[20] Kahneman, 2011.
[21] Thaler & Ganser, 2015.
[22] Maćkowiak & Smets, 2008, pp. 7-8.
[23] Penman, 2002; Novak, 2008; Cunningham, 2005. I am grateful to Ehsan Feroz for his advice on these issues.
[24] Franklin Fisher, Disequilibrium and Stability, pp. 75-76.

> There are two fairly common mistakes that must be avoided in considering such matters. First, one must not confuse the fact that the economy will move away from positions that are not equilibria with the much deeper and unproven proposition that the economy always converges to equilibrium (let alone the proposition that it spends most of its time near equilibrium). In more specific terms, the fact that agents will seize on profitable arbitrage opportunities means that any situation in which such opportunities appear is subject to change. *It does not follow that profitable arbitrage opportunities disappear or that new opportunities do not continually arise in the process of absorbing old ones.*

In an earlier paper, I claimed that the failure of LoP is of more significance than is generally believed without clearly stating its nihilistic implications for empirical economics. Now I want to refine and strengthen the earlier argument. If LoP does not hold, then it is probable that equilibrium is not reached, and *if equilibrium is not reached, how do we relate our models to data?*

IV. "Testing" macro

Macroeconomic theory is based upon the belief that 1) aggregates can be usefully reasoned upon, and that 2) there are important instances where the properties of an aggregate cannot be deduced from the behavior of its parts. Applications of Macroeconomics to policy implicitly requires the data to be equilibrium values. This last assumption, that Macro data are equilibrium data, is one that has simply not been questioned or tested to my knowledge. The problem is particularly acute for Keynesian macro. We can build models with aggregates such as C, I, G, X and M, and then fit these models to the data, but when we make predictions, are these not about equilibria? Since almost all macro discussion has been conducted with annual data, have we not implicitly claimed that macro variables reach their equilibria within the year and that only one such equilibrium is reached annually. If several different equilibria were attained during the year, which of these should we use as our datum? Implicit in our arguments on relating macro-models to data lies an assumption about the way data is generated and how it is collected.[25]

How is one to understand Macro equilibria in terms of observable data? Macroeconomics deals with many sectors and one wonders if it needs something like uniform convergence across sectors for its empirical claims to be acceptable – what if one sector only reaches half its equilibrium in the data period?[26]

[25] Haavelmo, 1944. I found nothing on the same lines in Frisch or Tinbergen, but it needs pointing out that Haavelmo thanks Frisch for many ideas.

[26] Having taught CGE modelling for many years, I tried to modify the price algorithm we use by slowing down price adjustment in some sectors, by 80% for food and by 90% for agriculture, and then introducing an exogenous policy change. Suppose the lagged model is called B and the original model A. Of course model B took longer to converge than A. To my surprise, the distance from equilibrium prices for B, after letting the model run for the number of iterations needed to reach equilibrium in A, was less than 5%. If the real world is as simple, with equilibria as reliably unique, then speeds of convergence may not matter much. I am grateful to Hadi Esfahani for having introduced me to CGE modelling on Excel.

Services, in particular, raise many questions of delimitation and counting, known to us at least since Hill (1977).[27] Readily available data is often unusable as an equilibrium proxy – sometimes because it is unreliable, but more often because it is an aggregate or index whose "make" is not transparent. The more accurate, basic, data, the source of the index that is publicly provided, is confidential, inaccessible, and perhaps incomprehensible to ordinary economists. The problem is so prevalent that it is non-negligible, even if it may not be systemic.

Many, perhaps most, economic variables are affected by expectations. Why does an announcement by the Fed, or even a casual remark by its Chair, give rise to changes in prices? Similarly, the January announcements of profits by corporations leads to noticeable buying and selling – phenomenon that are to be expected if agents are widely seeking information. Any equilibrium has to be based on a set of expectations and the expectations are derived from projections of data. But since these projections are defensible only as bounds and not as point values, as argued forcefully by Charles Manski in his critique of the widespread but unjustifiable desire for "incredible certitude", it follows that the unique equilibria needed by theorists for comparative statics either do not exist, or, if they do exist, are normally unknowable from the data.

One of the widely agreed upon lessons of the GFC of 2008 is that finance must be integrated within macroeconomics for macro to achieve relevance. A worthwhile goal, but can it be rigorously achieved? Eugene Fama is generally respected as one of the founders of modern finance and Nobel Laureate for 2013. In an interview with Joel Stern in 2016 Fama has some very discouraging observations.[28] After dismissing the literature on anomalies and giving guarded respect to Richard Thaler behavioral critique, Fama goes on to assess the rigor of modern corporate finance. Fama's emphasis upon rigor needs to be understood and his honesty applauded. The entire quote is too long to be provided but Fama tells us he is not allowed to teach corporate finance, "because my view is that what we teach our students has very flimsy theoretical underpinnings". Fama then elaborates

> "Take DCF analysis, for example, which comes out of the perfect certainty world of Irving Fisher. My first objection is that we don't live in a perfect certainty world. But let's push past that problem. We then have the problem of estimation. So, let's suppose the sky opens and a voice tells us that CAPM is the right model, and all we have to do is to estimate beta. The problem here is that, for individual stocks, the estimate of beta is garbage. Even for an industry, there's no hope of estimating beta because it's too dynamic through time. Ken French and I wrote a paper that showed you're no better off using two years of data than ten years of data because there's so much movement. But now let's suppose that the sky opens again and the correct beta comes down. Now, all you have to do is to estimate the market premium. But all we can really say is that it's a number somewhere between 2% and 10%, and we have very little basis for settling on a particular estimate. And yet the number you choose will have a dramatic effect on your results."

A major problem is that Finance must consider asset values and this is a beast yet to be tamed. Fischer Black claimed long ago that asset values depended on several indeterminate

[27] Hill, 1977.
[28] Fama & Stern, 2016.

variables and would perhaps always be subject to estimation with a wide margin of error.[29] Vernon Smith found, to his surprise, that subjects who were able to get close to the equilibrium when participating in goods markets, were prone to produce wide swings and bubbles when dealing with asset values.[30] When we combine both points – Black telling us that even experts work in a fog when estimating asset values and Smith providing experimental evidence about the proneness of humans to miscalculate asset values – it appears that we have little hope of getting asset values "right" in some objective sense.

It is no wonder that new models seem to be continually proposed to explain the observed anomalies in asset and in financial markets.[31] The most empirically relevant is perhaps that of Fostel, Genakopoulos & Phelan,[32] which treats a question of increasing importance at a time of greater global financial integration – cross-border financial flows. They show that such flows can arise when otherwise identical countries differ in their abilities to use assets as collateral to back financial contracts. Due to a resulting gap in collateral values, financially integrated countries can have access to the same set of financial instruments without producing price convergence for assets with identical payoffs. The price divergence will produce financial flows which can amplify asset price volatility in both countries. Unless the countries adopt the same institutions and legal characteristics it is hard to see how collateral value equalization will be attained.

For empirical Macroeconomics to be plausible, we need data which are collected while parameters are stable and after equilibrium is reached. This involves more than one assumption, so let us call the joint assumptions the Fundamental Assumptions of Measurement. Once the implications of these assumptions – that measured values are taken to be the equilibrium values generated by stable systems – are explicitly spelled out, some obvious questions arise about the acceptability of such assumptions:

1. What if parameters change in the time it takes us to reach equilibrium?
2. What if data is collected in time periods too short for equilibrium to be reached?
3. What if the categories being used for data change before data collection is complete or before equilibrium is reached?

Since many useful applications of economics are occurring every day, surely the situation cannot be as chaotic as implied by the above questions about data and equilibrium?[33] Agreed, but this may be because those who engage in policy readily modify or select data to represent reality – just as those who reported crop price 200 years ago waited till the harvest was known and crop prices could be assumed to have settled.[34] This recognition of "dirty detail" and its importance is missing in neoclassical economics. Those who have participated in policy discussions generally attest to the fact that all the interesting debates occur are about institutional details which seem too petty for theory. We manage not only because we know a lot more than our theory tells us, a la Michael Polyani's views on "implicit knowledge",

[29] Black, 1986.
[30] Noussair, 2017.
[31] Guo & Wachter, 2019 consider an economy in which investors believe dividend growth is predictable, when in reality it is not. They show that a wide variety of evidence can be explained with this hypothesis and furthermore that are "rational" when confronted with evidence.
[32] Fostel, Genakopoulos & Phelan, 2019
[33] It is not directly germane to this paper, but in looking at the unexpected regularities of some forms of data, I am driven to the observation that different levels of aggregation may lead to different empirical "laws", meaning regularities which we cannot be theoretically comfortable with, but which are adequate for practical purposes (see note 28 also).
[34] As one observes Working (1925) when he is selecting the data to use for demand studies.

but also because we tacitly allow such practical knowledge to supplement or even override our theoretical framework.

V. "Cognition" for optimisation

Modern *homo economicus* is possessed of "greed" to an extent scarcely recognized in the past. They are optimization machines whose internal functions are unobservable but whose effectiveness is absolute. "Knowledge" for such people requires "knowing" of a peculiar sort. "Knowing" has two parts here. First, the agent has to know all the relevant facts, in the sense of being aware of them and being able to assimilate and store them. Thereafter, the agent has to be able to process and deduce the implications of the knowledge acquired. Thereafter, there is a third step wherein the agents actually execute the optimal plan. This requires self-control and will power that psychologists have shown to be not only demanding but infeasible for most of us. Since two Nobel prizes, to Kahnemann and to Thaler, have made the last point, among many others, very effectively, it is needless to discuss their import.

The finite limits of our cognition is an important point, which has been inadequately stressed. Our agents need to accept, store and assimilate very large amounts of information. Computer scientists can perhaps address the reasonableness of this assumption. Thereafter, "cognition" requires deduction and our rational agents are capable of very large amounts of inference. I once heard Larry Samuelson joke about chess players refusing to play because of Zorn's lemma. It seemed a most effective retort to the idea of perfectly informed agents. Chess players are highly motivated to win – I have read that Grandmasters put in so much effort that they can lose up to 10lbs in a match. If infinite cognition made any sense, why do the chess players not just refer to Zorn's lemma, claim that such deterministic games have a solution, then shake hands and award the prize?

Indeed, the whole history of mathematics has been a waste of energy. Once Euclid wrote the assumptions, all of Geometry was known to such infinitely competent inference machines. Perhaps the rules for manipulating integers were known before Euclid. In which case, all of Number theory was already common knowledge before Euclid. There was never any need to teach mathematics – only to state the axioms. And so on… *ad absurdum.*

Suppose however, in the spirit of nonchalance with which the profession accepts any and all axioms and then looks to their consequences and the observable predictions that follow, we accepted "unbounded cognition" as a research program. This would completely alter valid research questions. Now we would *assume* that everyone always knew all of Geometry, Calculus and Algebra; what needed explaining was why and how they hid this knowledge?

- Was it due to social forces?
- Was it the result of censorship?
- Did people communicate the results by some cryptic code?
- Were there indirect forms of speech which conveyed the message that "polynomials are solvable"?
- Who pressured Gauss into providing three different proofs of the solvability of polynomials?

And the *piece de resistance* of this research program,

- Why did mathematicians pretend to have such a hard time with proofs?

It is undoubtedly a fruitful program. There is much to occupy academics for decades…

VI. Summary and Conclusion

It is plausible that economics slipped into its current difficulty because all earlier theory was framed with agriculture in mind. But we are not in an agricultural world anymore. The number of available products must have expanded a 1000-fold since the 1700s. Unless one looks, it is difficult to grasp the sheer amounts of data that are generated and potentially available – but perhaps impossible to digest because of their magnitude and complexity. Below are two examples, from Trade statistics and from Price indices.

> Customs forms provide us with Trade data, one for each export shipment. There were about 22 million export shipments originating in the U.S. in 2005. This suggests that we have information on some 22 million individual decisions. However, there are 229 countries and 8,867 product codes with active trade, so a shipment can have more than 2 million possible classifications.[35]
>
> Next, consider the Consumer and Producer Price Indexes, the CPI and PPI[36]. The Producer Price Index program collects monthly price data on about 128,000 individual items from about 32,000 establishments. The CPI collects data on about 80,000 individual items. The larger number for the PPI is presumably due to the addition of many intermediate goods in the PPI.

Unless one accepts the *hara-kiri* assumption of perpetual equilibrium, the question of data relevance now revolves around speeds of convergence in each market. However, there are practically no studies of this question – the speed of convergence to equilibrium – for goods or services in *microeconomics*.[37]

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[35] Surprisingly perhaps, even such extensive data show several regularities. (1) Most product-level trade flows across countries are zero; (2) The incidence of non-zero trade flows follows a gravity equation; (3) Only a small fraction of firms' export; (4) Exporters are larger than non-exporters; (5) Most firms export a single product to a single country; (6) Most exports are done by multi-product, multi-destination exporters (Armenter & Koren, 2010).

[36] I am very grateful to Scott Sager, Ken Stewart and Amy Hobby for answering my queries. I have used their replies for this section with only the minimal editing needed for my purposes. The plethora of data obtained from POS transaction records should satisfy the quantitative economist by their volume. But the sheer volume alone does not solve questions of aggregation, or functional form, or endogeneity of explanatory variables. More interestingly, none of our usual procedures recognise how the institutions at work have adapted to their particular circumstances. Economists at the FTC, who have to argue for or against mergers of firms, urge caution in moving from the retail level POS data to inferences about wholesale market elasticities (Hosken et al., 2002, pp. 2, 3-4, 21, 24).

[37] A Google search of 200 plus items under "rates of convergence in economics" produced only one entry on micro. All the others consider growth theory, which does not bear on this issue. The one seeming exception adapts growth theory concepts to micro contexts and fails to address the concerns expressed here, i.e., the rate at which price and quantity converge to equilibrium in each individual market (Fazio & Piacentino, 2011).

Recognising the complexity of the modern world leads me to return to the principal message of this paper in three sentences.

1. Equilibrium economic theories can be tested only by equilibrium data
2. Data being used for tests have no presumption of being equilibrium data.
3. Hence, the data now used for "testing economic theories" provide no tests at all.

Are there any conditions which justify the current practice? Yes, a state of perpetual equilibrium will serve to justify current practice. However, such an assumption creates many subsequent difficulties, some of which have been described above.

Appendix A

To know about rates of convergence to equilibrium, one wants to get direct estimates of the speed with which prices of individual goods adjust to shocks. Unfortunately, the wealth of detail possessed by US agencies does little to help the ordinary inquirer who wishes to behind such well known figures as the Consumer Price Index or CPI. For several reasons, the pursuit of individual goods is not considered feasible, even by many insiders in the BLS. First, the data are almost certainly insufficient for looking at specific items such as eggs. The average sample size for a given item, such as eggs, in a given area such as Cleveland-Akron, is 10 price quotes. Because the CPI reflects consumer shopping patterns, the price data contain considerable heterogeneity. Isolating a single homogeneous item thus leads to extremely small sample sizes. For example, in the relatively homogeneous category of eggs, the most common unique item contained a monthly average of 62 price quotes for all 87 areas. Second, because of the heterogeneity, it would be burdensome for BLS staff to select specific homogeneous items for such a study, which would end up being quite expensive and probably subject to high variance because errors, either in data collection or in BLS selection, could affect the results. Third, many prices change slowly with prices changing on average only once every eight to 11 months. Because BLS samples are rotated either every two years or every four years, only a very small number of price changes in any specific item will be observed before that item is replaced. Fourth, most price changes for durable goods only occur when models change. If one wishes to observe price changes in homogenous goods, durable goods of a given model will frequently show no price changes at all. To follow the dispersion, transmission and adjustment of the prices of individual goods in a world with micro-data is beyond the range of ordinary persons and probably requires a bureaucracy in itself. If we want to model the process by which data are actually generated, a further complication lies in the use of price/revenue algorithms by many supermarket chains. If such algorithms include local idiosyncratic patterns and if there are many such idiosyncratic patterns in the sample area, then interpretations of the data become even more complex.

The fact that the difficulties which are alluded to above are real is both supported by, and hopefully will be addressed with the considerations provided in "Minding your Ps and Qs".[38]

[38] Ehrlich et al., 2019.

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Nominal science without data – the artificial Cold War content of Game Theory and Operations Research

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Abstract
Expanding upon literature on early digital computers, this paper shows the role mathematicians have undertaken in founding the academic fields of Game Theory and Operations Research, and details how they were supported by the mathematics departments of military agencies in branches of the US Armed Services. This paper claims that application is only decoration. Other than astronomy, physics and engineering, where experiments generate data analysed with the aid of models and appropriate software on computers, Game Theory and Operations Research are not data driven but method driven and remain a branch of applied mathematics. They use the method of "abstractification" in economy and society to derive their models but lack a layer of empirical research needed to generate data and to apply their methods in economics and society. Therefore, their models were only nominal mathematics without application.

Introduction

Since 1945, the United States had experienced a unique innovation push with the computer, the nuclear weapon, new air combat weapons and the transistor within just a few years. These innovations were accompanied by Game Theory and Operations Research in the academic field. Widely–held is the view that computers supplemented the mathematical concepts of Game Theory and Operations Research and gave these fields a fresh impulse. Together, they established the view of the world as a space of numbers and introduced quantitative methods in economics, political science and in sociology. A series of conferences on these subjects settled this new view. They imparted Cold War science and technology policy with a unique flavour of progress, superiority and modernity.

Whereas the history of quantitative methods has been mainly written as a history of digital computers, the history of Game Theory and Operations Research has had only a small number of contributions. In the issue 83 of Real World Economics Review Bernard Guerrien and Lars Pålsson Syll published 2018 critical contributions to the current state of Game Theory: Syll criticised the rational choice theory and Guerrien doubts whether Game Theory could be applied to real world problems.[1] My approach here is a history of science approach that reveals the artificial content of Game Theory and Operations Research in the Cold War science context. In addition as a sociology of science approach, I characterize these theories as an expert movement of mathematicians. This paper deconstructs the current success stories and shows that Game Theory and Operations Research were not only related to the Cold War scenario in the nominal sense, but lacked substantiated applications in social,

[1] Florencia Garcia-Vicente, Daniel D. Garcia-Swartz, Martin Campbell-Kelly, "The History, Geography, and Economics of America's Early Computer Clusters." *Information and Culture*, issue 4, 2016, pp. 445-478. William Aspray, *John von Neumann and the Origins of Modern Computing,* Cambridge, Mass., *1990*. Thomas Haigh, Mark Priestley and Crispin Rope, *ENIAC in Action – Making and Remaking the Modern Computer*, Cambridge, Mass., 2016. Lars Pålsson Syll, "Why Game Theory never will be anything but a footnote in the history of social science." *Real-World Economics Review*, issue no. 83, 20 March 2018, pp. 45-64. Bernard Guerrien, "On the current state of Game Theory." *Real-World Economics Review*, issue no. 83, 20 March 2018, pp. 35-44. Roy Weintraub (ed.), *Toward a History of Game Theory*, London, 1992.

political and economic fields, and remained a branch of applied mathematics. To regard Game Theory and Operations Research within the context of digital computers opens up the view that these strands of science and technology came about through the same institutions, at the same time and using the same proponents and funding agencies which have John Von Neumann at the centre.[2] Mathematicians in the branches of the Armed Services strongly supported the development of analogue and digital computers and related research in Game Theory and Operations Research. The U.S. Army's Ballistic Research Laboratory at Aberdeen, Md., was led by mathematicians and funded the development of the ENIAC computer at the Moore School of the University of Pennsylvania in Philadelphia. The Navy maintained their Office of Naval Research in Washington D.C., which included a mathematics department and supported several R&D projects.[3] The Air Force employed the RAND Corporation with the department of mathematics and the National Bureau of Standards (located in Washington D.C.) as R&D laboratories and agencies for financing research and the development of digital computers. Established in 1948 in Santa Monica, California, RAND was the think tank of the Air Force and had great influence in shaping academic debates during the Cold War. But its research on future air warfare and strategic bombing systems did not meet the expectations of the Air Force. RAND's plan to attack the Soviet–Union using a fleet of bombers, in which most of the pilots would have been put at risk, was refused by the Air Force.[4] So RAND focussed very successfully on academic attitudes toward research on Game Theory. It organized conferences and edited books. Every leading economist and mathematician held a consulting contract with RAND – these were very well-paid.[5]

The history of Cold War discourse at RAND has already been the subject of critical accounts. Stephen Johnson and Philip Mirowski covered the rise of Game Theory and Operations Research at RAND and their impact on neoclassical economics.[6] Judy Klein explored the emergence of quantitative methods in the field of time series and of the theory of Dynamic Programming in the Cold War and contributed to the critical study on the role of Game Theory in Cold War discourses. She also contributed to the book "How Reason Almost Lost Its Mind" (2013), the result of a summer seminar on Game Theory at the Max–Planck–Institute Berlin in 2010 (in the following MPI–group).[7] This book also contains a critical account of Operations

[2] William Aspray, *John von Neumann and the Origins of Modern Computing,* (cf. note 1).
[3] Mina Rees, "The Computing Program of the Office of Naval Research, 1946-1953." *Annals of the History of Computing,* issue No. 2 - April-June (1982 vol. 4), pp. 102-120. As R&D is research and development meant. Richard Vahrenkamp, "The Computing Boom in the US Aeronautical Industry, 1945–1955." *ICON – The Journal of the International Committee for the History of Technology*, volume 25, 2019, pp. 2–25.
[4] Stephen Johnson, *The United States Air Force and the culture of innovation, 1945-1965*, Washington D.C. 2002, Air Force History and Museum Program, p. 42.
[5] Charles Shrader, *History of Operations Research in the United States Army*, Washington D.C., 2006, p. 60. On the role of the RAND Corporation in decision theory see Paul Edwards, *The closed world. Computers and the Politics of Discourse on Cold War America*, Cambridge (Mass.), 1996, pp. 114-116. George Dantzig, "Impact of Linear Programming on Computer Development", Lecture at ORSA/TIMS meeting on April 30, 1985, typewriter manuscript Stanford University, Document ADA157659, 1985 (Internet source).
[6] Stephen Johnson, "Three Approaches to Big Technology: Operations Research, Systems Engineering, and Project Management." *Technology and Culture*, Vol. 38, No. 4 (Oct., 1997), pp. 891-919, here p. 898. Philip Mirowski, *Machine Dreams – Economics becomes a Cyborg Science*, Cambridge (Mass.), Cambridge Univ. Press 2002. See also Jennifer Light, *From Warfare to Welfare: Defense Intellectuals and Urban problems in Cold War America*, Baltimore 2003, on the influence of RAND on urban planning in New York.
[7] Paul Erickson, Judy Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm and Michael Gordin, *How Reason Almost Lost Its Mind*, Chicago UP, 2013. Judy Klein, *Statistical Visions in Time: A History of Time Series Analysis, 1662 – 1938*. Cambridge (Mass.), 1997. Judy Klein, "Cold War, Dynamic Programming, and the Science of Economizing: Bellman Strikes Gold in Policy Space", lecture at First Annual Conference on the History of Recent Economics (HISRECO), University of Paris X -Nanterre,

Research. Paul Erickson's book on Game Theory followed in 2015. My paper continues these studies and will introduce the new concept of "abstractification". With this approach, the results of the MPI–group will be developed further to show the artificial content of Cold War discourses on Game Theory and Operations Research.

Atsushi Akera and Brent Jesiek have already explained the leading role mathematicians assumed in the development of the digital computer.[8] I will expand this reasoning and show that mathematicians also developed Game Theory and Operations Research and introduced a particular view of society as a space of numbers. The method applied in Game Theory and Operations Research is the "abstractification" of social reality in order to get a mathematical model. In engineering, astronomy and meteorology, mathematical models serve to structure the data measured and to make better predictions. Computers are fed with data to test the models. Scholars work inside the triangle data-model-computer, making this approach data-driven.[9] The scientists had personal experience with the material which they studied, as Nathan Ensmenger showed with the example of a laboratory in biological research.[10] Another is the approach of Game Theory and Operations Research. These fields use social, economic and political relations in firms and in society to derive mathematical models for their own sake, but not to derive solutions for social or economic problems. They stripped their models of social and political relations and gained simple models as material for academic purposes. Both were not driven by data, but rather by new mathematical methods. Empirical data was not particularly interesting for the scholars, and therefore the triangle of data-model-computer remained blank. The method of abstractification leads into the space of numbers with no way back to the real world, as will be shown with the examples of mixed strategies in Game Theory and the Transportation Model of Operations Research.

To regard Game Theory and Operations Research as an expert movement of mathematicians is not extraordinary in a twentieth century that witnessed various expert movements: the efficiency movement in the US around 1910, the rationalization movement in European industry around 1925, and the automation movement in the US and Europe around 1960. All these movements were already subjects of critical studies exploring their goals and the limited extent to which they achieved them. Furthermore, the studies explored the actors, the influence of government policy and views in public debates, scientists, employers and trade unions.[11]

As primary sources, this paper relies upon material provided by the 60th anniversary edition of Morgenstern's and Von Neumann's book "Game Theory and Economic Behaviour", published by Princeton University Press in 2004. It also refers to original papers on Game Theory and Linear Programming which the RAND Corporation offers on its web site and on contemporary

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France, 21-23 June 2007. Paul Erickson: *The World the Game Theorists made*, University of Chicago Press, 2015.

[8] Atsushi Akera, *Calculating a Natural World – Scientists, Engineers, and Computing during the Rise of U.S. Cold War Research*, MIT Press 2007. Brent Jesiek, "The Origins and early History of Computer Engineering in the United States." *Annals of the History of Computing*, vol. 35, 2013, October, pp. 6-18.

[9] Gabriele Gramelsberger, *From science to computational sciences: studies in the history of computing and its influence on today's sciences*, Zurich, Diaphanes, 2011.

[10] Nathan Ensmenger, "The Digital Construction of Technology: Rethinking the History of Computers in Society." *Technology and Culture*, Volume 53, Number 4, October 2012, pp. 753-776.

[11] Samuel Haber, *Efficiency and Uplift*, Chicago 1964. Charles Maier, "Between Taylorism and Technocracy: European ideologies and the vision of industrial productivity in the 1920s." *Journal of Contemporary History*, 5 (1970), issue 2, pp. 27–61. Ronald Kline, "Cybernetics, Management Science, and Technology Policy: The Emergence of 'Information Technology' as a Keyword, 1948–1985." *Technology and Culture*, 47 (2006), issue 3, pp. 513–535.

conference proceedings. For the history of Operations Research, this paper refers to Dantzig's book on Linear Programming (1963) and to the contemporary journals which *The Society of Operations Research* and *The Institute for Management Science* have issued. The book "An Annotated Timeline of Operations Research" (2005), edited by Saul Gass and Arjange Assad, serves as a collection of references to original papers.[12]

Morgenstern's and Von Neumann's push for Game Theory

Similar to digital computers, Game Theory developed as a view of the world as perceived by mathematicians and was pushed by the same institutions as the Institute for Advanced Study (IAS) in Princeton and the RAND Corporation. This body of theory splits into two strands: mathematical and experimental. The latter conducts experiments in groups of test persons, and studies how they behave when following certain rules. Kurt Lewin founded "Group Studies" in the 1920s as part of the experimental psychology of the University of Berlin, and was later head of the research unit for group dynamics at MIT.[13] In the 1950s, behavioural psychologists and economists introduced experiments in groups to study the behaviour of test subjects in market exchange and game playing. In the 1980s, Reinhard Selten, who received the Nobel Prize in economics for Mathematical Game Theory (together with John Nash) in 1994, turned his attention to experimental Game Theory, together with his pupil Axel Ockenfels.[14] Both branches of Game Theory developed, to a large extent, independently. But mathematicians at the RAND Corporation conducted some experiments in the early 1950s.[15] The mathematical branch of Game Theory did not pick up on results from the experimental one but based on mathematical axioms.[16] In the following, the history of Mathematical Game Theory will be focussed on, in which the term Game Theory is understood to refer to Mathematical Game Theory.

Against the background of Cold War R&D, John Von Neumann was one of America's leading mathematicians and scientists. He was not only engaged in designing digital computers and atomic bombs, but also shaped Princeton and RAND into centres of Game Theory. From 1941, he gave lectures on Game Theory at the University of Princeton, where he met Oskar Morgenstern – an Austrian immigrant (and refugee) and economist.[17] Together they wrote the book *Theory of Games and Economic Behavior* that was published in 1944 by Princeton University Press, and contained more than 600 pages.[18] The book laid the ground for a new field of applied mathematics that abstractified social relations in society to develop simple

[12] Saul Gass and Arjange Assad, *An Annotated Timeline of Operations Research*, New York 2005.
[13] Anna Perlina, *Shaping the Field: Kurt Lewin and experimental psychology in the interwar period*, Berlin 2016. Philipp Ullrich, *Der Beitrag von Kurt Lewin zur Grundlegung des modernen Managements*, Kassel 2005.
[14] For the experimental Game Theory see Vernon Smith, "Game Theory and Experimental Economics: Beginnings and Early Influences", in Roy Weintraub (ed.), *Toward a History of Game Theory*, London, 1992, pp. 241-281. Anatol Rapoport and Albert Chammah, *Prisoner's Dilemma*, Univ. of Michigan Press, 1970.
[15] Paul Erickson et al., *How Reason Almost Lost Its Mind*, (cf. note 7), pp. 135-142
[16] The turn from mathematical Game Theory to experimental Game Theory in the 1980s can be studied in the journal *Game Theory and Economic Behavior*. The first volume, in 1989, was devoted to mathematical Game Theory, whereas the tenth volume, in 1995, had experimental contributions.
[17] Urs Rellstab, "New Insights into the Collaboration between John von Neumann and Oskar Morgenstern." In Roy Weintraub (ed.), *Toward a History of Game Theory*, (London, 1992), pp. 77-94. Oskar Morgenstern, "Collaboration between Oskar Morgenstern and John von Neumann on Theory of Games." *Journal of Economic Literature*, vol. 14, 1976, no. 3, pp. 805-816.
[18] *Theory of Games and Economic Behavior*, Princeton UP, 1944. John von Neumann published already in 1928 in German language "Zur Theorie der Gesellschaftsspiele", *Mathematische Annalen*, vol. 100, pp. 295-320.

models of competition between firms and social conflicts between two or more antagonistic "players" who pursue "strategies". Morgenstern and Von Neumann coined the term "Game Theory", unheard of until then. The authors did not derive their models from social life, as known from social sciences or experimental Game Theory, but their approach was based purely on axiomatic mathematics. They observed phenomena in society in order to derive axiomatic mathematical models that seemed to be of value for society only in a nominal sense. But they did not provide techniques on how to apply their models. Game Theory remained a field of academic mathematics that existed purely for its own sake.

The approach of Morgenstern and Von Neumann was as following. To make a model of competition between two players (named A and B) they assumed that players get payoffs or profits depending on the (fictive) strategies they chose. Not derived from empirical research, the authors introduced a payoff table with numeric values for each player, which they invented at their office desk. The tables have the dimensions 2x2, in which each player could choose from two strategies, then the dimensions 3x3, in which each player could choose from three strategies, etc. The payoff tables are assigned to the strategies of A and B: the lines to the strategies of A, the columns to the strategies of B. The tables therefore show all the possible combinations of payoffs for A and B, depending on the choices made by the players. The following tables show two 3x3 payoff tables, called A's Profits and B's Profits, in a piece of 1946 coverage on Game Theory by the New York Times.[19]

Table 1 A's Profit. Example of a payoff table as published by the New York Times on 10 March 1946.

	A's Profits		
	B_1	B_2	B_3
A_1	2	8	1
A_2	4	3	9
A_3	5	6	7

Table 2 B's Profit. Example of a payoff table as published by the New York Times on 10 March 1946.

	B's Profits		
	B_1	B_2	B_3
A_1	11	2	20
A_2	9	15	3
A_3	8	7	6

When, in the example of tables 1 and 2, player A chooses strategy A_1 and player B strategy B_2, then A receives amount 8 as payoff (in cell 1,2) and player B amount 2 (in cell 1,2). The

[19] On March 10, 1946.

exact meaning of the payoff is left open: it could be measured in Dollars or in subjective utility values. Positive values could be seen as gains, negative values as losses.[20] The tables show the result of abstractification: they were stripped of all social and political context and reduced the decision situation to calculate the optimal solution inside the tables. The complexity of the world was reduced to few entries of a matrix, as Paul Erickson critically observed.[21]

The payoff tables display the payoffs for when the game is played just once. The player's choice of strategies is called 'pure strategy'. This situation changes when the players take in a long sequence of repeated games, where the strategies are randomly mixed with certain but constant probabilities. Then the average payoff, evaluated by using the probability values, is considered for each player (expected payoff). The turn from pure strategies to mixed strategies has important implications. For mathematicians, it appears as a standard method of generalization, linking probabilities to strategies and leading Game Theory into the abstract space of numbers. But in the real world, players do not have such a large amount of time and money to play such a long sequence of repeated games. In politics, time can be a very scarce resource. So, the concept of mixed strategies cannot be applied in the real world. In his popular account of Game Theory, the RAND author John Williams tried, on two pages, to convince the reader that the turn from pure to mixed strategies was justified. But he did not understand that mixed strategies were a mathematical fiction that could not be applied to the real world.[22] The MPI-group indicated that the repetition of a game induced effects of learning and therefore deviations from the first results. In their empirical study on Prisoner's Dilemma games, Rapoport and Chammah saw in the concept of mixed strategies a "natural" extension of repeated board games.[23] But this assumption is misleading, as economics and politics are not board games, and repeated runnings are not possible.

The author Arthur Copeland, in the Bulletin of the American Mathematical Society in 1945, saw the book *Theory of Games and Economic Behavior* as one of the major scientific achievements of the first half of the 20th century.[24] The book, however, did not sell very well. Von Neumann saw the book as a "dead duck". But then something surprising happened, presumably because of John Von Neumann's overwhelming influence on science policy at the East Coast. On March 10, 1946, the New York Times put a sensational headline on the front page of its Sunday edition: "A new approach to economic analysis that seeks to solve hitherto insoluble problems of business strategy by developing and applying to them a new mathematical theory of games of strategy like poker, chess and solitaire has caused a sensation among professional economists". The economist Leonid Hurwicz published another article in the same issue of the New York Times, with two 3x3 payoff tables (as shown in tables 1 and 2 above), as an example of how to apply Game Theory to the duopolistic competition of two enterprises.[25] To build his payoff tables, Hurwicz did not use empirical field studies in duopol cases but invented the tables on his office desk. The New York Times coverage led to a breakthrough in Game Theory. The first edition of the book quickly sold out, and in 1947 a second edition appeared in which the authors inserted a new third chapter on

[20] Behavior psychologists measure utility values on interval scales so that by adding a constant to the values in the tables one can turn all values into the positive domain, see Rapoport and Chammah, *Prisoner's Dilemma*, (cf. note 14), p. 39.
[21] Paul Erickson: *The World the Game Theorists made*, University of Chicago Press, 2015.
[22] John Williams, *The Compleate Strategyst*, New York 1966, second edition, 206s.
[23] Paul Erickson et al., *How Reason Almost Lost Its Mind*, (cf. note 7), p. 145. Rapoport and Chammah, *Prisoner's Dilemma*, (cf. note 14), p. 23.
[24] *Bulletin of the American Mathematical Society* 51, 1945, p. 498. For further reviews see the 60th anniversary edition of *Theory of Games and Economic Behavior*, Princeton UP, 2004.
[25] For the duopoly cases in Game Theory see Guerrien (cf. note 1).

utility theory. Again, this strand of theory was purely mathematical and not derived from investigations in social contexts.[26] It is not easily accessible, and for the author of this paper, completely unintelligible. The third edition appeared in 1953. The new field of Game Theory mushroomed. The breakthrough in Game Theory represented by the *New York Times* coverage suggested that Game Theory was a media event and led to great esteem in public and academic fields. Since the 1950s, universities have published a steady stream of books on Game Theory, as an investigation in the library catalogue of the Technical University of Berlin revealed:

Table 3 Number of published books on Game Theory according to decades.
(Source: Library catalogue Technical University of Berlin)

before 1964	40 books
1964 till 1975	124 books
1976 till 1987	158 books
1988 till 2000	225 books
after 2000	199 books

The output of books reached its height in the 1990s, when John Nash won the Noble Prize 1994. The Noble committee awarded its Prize in economics for research in Game Theory also in the years 2005, 2007 and 2012.

Surprisingly, the New York Times coverage refers to poker, chess and solitaire, but not to a genuine example of duopolistic competition such as, for example, Shell versus British Petroleum in the petroleum industry. Game Theory, then, had an image of being for entertainment, and only promised applications "to social, political and economic phenomenon(s)", as Rudolf Henn and Otto Moeschlin proposed in their retrospective in honour of Oskar Morgenstern's 75th birthday in1977. Game Theory achieved an extraordinary level of success, with more than 6000 publications by 1977.[27] Mathematicians exported the field of Game Theory, together with Operations Research, into economics departments in universities.[28] The proposition that mathematicians in the field of Game Theory had filled positions in economic faculties can be substantiated by the careers of prominent Game Theory scholars such as Robert Aumann, Reinhard Selten and Joachim Rosenmüller. All three obtained a doctorate in mathematics before becoming game theoreticians. Robert Aumann founded the Center for Game Theory in Economics at Stony Brook University on Long Island, New York, in 1989. He was awarded the Nobel Prize in Economics in 2005. Reinhard Selten became full professor at the Faculty of Economics of Freie Universität Berlin in 1969 and joined the newly founded Center for Mathematical Economics at Bielefeld University (Germany) in 1972. The Center became part of the newly founded Faculty of Economics at Bielefeld University in 1974. Selten received the Nobel Prize in Economics in 1994. Joachim Rosenmüller became full professor at the Faculty of Economics of the University of Karlsruhe (Germany) in 1972 and joined the Center for Mathematical Economics at the University of Bielefeld in 1978.

[26] Also the chapters on utility theory in microeconomics do not pick up results from social sciences, see Hal Varian, *Microeconomic Theory*, New York, 1978.
[27] Rudolf Henn and Otto Moeschlin, *Mathematical Economics and Game Theory – Essays in Honor of Oskar Morgenstern on his 75th birthday*, Berlin 1977, p. 4. The book contains a short bio of Morgenstern, pp. 1-10, and a bibliography of his publications, pp. 695-709.
[28] Philip Mirowski: *Machine Dreams*, (cf. note 6), p. 488.

Morgenstern and Von Neumann proposed, in the foreword of their book, that the solution to social problems could be reached with the aid of Game Theory, but they did not present any such solution. Until now, not a single example for the application of Game Theory to social problems, with an empirically derived payoff table, has been published as Bernard Guerrien proved at the four volume set *Handbook of Game Theory with Economic Applications* (edited by Robert Aumann and Sergiu Hart 2002).[29] Despite of this eminent lack of application, Game Theory held a position of high esteem in the minds of the public. On the life of John Nash, a popular book appeared in 1998 and a movie 2001 *A Beautiful Mind*, supporting the view of Game Theory as a media event.[30]

Zero sum games and lack of applications

As many surveys on Game Theory have pointed out, there was no unifying concept for the "solution" to a game. Morgenstern and Von Neumann proposed, for their two persons zero sum games, the intuitively appealing minimax solution. In the two persons zero sum games setting, only one table exists, displaying the gains of player A as positive numbers that are, at the same time, the losses of player B. This game type could represent the market shares of two competing firms. The gains in the market shares of one firm are the losses of the other one. Player A tries to maximize his gains and player B to minimize his losses. Player A choses a strategy (a row in the table) that maximizes the least gain of whatever player B does. Player B choses a strategy (a column in the table) that minimizes the greatest loss of whatever player A does. A saddle point in pure strategies exists if the least gain maximized by player A is equal the minimum of the greatest loss of player B. This saddle point is seen as a solution to the game. The strategies chosen to obtain the saddle point are called pure strategies.

But in the case that a saddle point in the payoff table does not exist in pure strategies, the authors applied a standard method from mathematics: the generalisation. They assigned probabilities to the strategies of the players and showed that, in this case, an equilibrium point exists for certain probabilities p and q, where the expected gains of player A equals the expected losses of player B. To obtain this kind of solution the players had to play a long sequence of plays and to mix their strategies randomly with probability p and (p–1) for player A and q and (1–q) for player B. This kind of procedure was called mixed strategies. For students in a university course, it is a nice exercise to compute the probabilities p and q by two equations with unknowns p and q in a 2x2 table, but this exercise disguises the lack of application. The generalization of a saddle point as mixed strategies applies very well in mathematics. But how should it be applied in politics? In the context of Game Theory, the Vietnam War was an important issue.[31] The RAND Corporation could have made a proposal in the Vietnam War: throw an atomic bomb onto Hanoi with a probability of 0.30 and make an invasion with ground forces with a probability of 0.70. These applications of mixed strategies with certain probabilities are only possible if one repeats the application and randomly mixes it many times: 30 times the atomic bomb and 70 times the invasion. But history is unique, and not subject to repeated trials. So, it is impossible to apply zero-sum two person games in politics.[32]

[29] See Guerrien and also Syll (cf. note 1).
[30] Sylvia Nasar, *A Beautiful Mind*, New York 1998.
[31] Paul Erickson et al., *How Reason Almost Lost Its Mind*, (cf. note 7), p. 133.
[32] Also Haywoods confessed difficulties to apply mixed strategies, see O. G. Haywood, "Military Decision and Game Theory." *Journal of the Operations Research Society of America*, vol. 2, no. 4 (Nov. 1954), pp. 365-385. Also J. McDonald, *Strategy in Poker, Business and War*, New York, 1950.

Already by the beginning of the 1950s, the lack of applications of Game Theory had become evident at RAND. It was seen as a nice intellectual spirit.[33] Objections arose to the model of zero-sum two person games. The payoff matrix was stripped of its social and political context and was viewed as too simple to display complicated situations in competition between firms or in political conflicts. The RAND Corporation could apply zero-sum two person games to make a re-interpretation of historic battlefield situations in terms of Game Theory, but could not gain new insights.[34] In 1959, criticism arose from Albert Tucker and Duncan Luce that the solution of matrix games did not prescribe rational behaviour nor "predict behaviour with sufficient precision to be of empirical value."[35] The lack of applications observed also Guerrien and Syll in their critical accounts.[36]

The Nash equilibrium and prisoner's dilemma

Albert W. Tucker was a mathematician at Princeton University who, since 1948, had held a contract with the Office of Naval Research for basic research into logistics.[37] This contract shows that the label "logistics" was sufficient to support mathematical research. By editing volumes on Game Theory, the Princeton mathematician Albert W. Tucker, together with Harold W. Kuhn from Stanford University, turned Princeton into an important centre of Game Theory. In 1950, the famous volume *Contribution to the Theory of Games* appeared, published by Princeton University Press. Although supported as a logistics project by the Office of Naval Research, the editors underlined frankly in the foreword that no applications were intended. Instead the papers in the volume would address pure mathematics. The same editors published a second volume in 1953 as part of the Logistics Project of the Office of Naval Research, which would shed some light on the application of Game Theory.[38] Other than in the first volume, which focussed on non-cooperative Game Theory that models situations of competition, the second volume had a section on cooperative n-person games, modelling cooperation in cooperative project work or "coalitions" in voting assemblies.

The later-to-be-famous John Nash was doctoral student of Albert W. Tucker. In addition to the minimax solution in Von Neumann's and Morgenstern's antagonistic two person games, he introduced an element of cooperation between the players. In his 1950 dissertation, through the application of the Kakutani fixed point theorem, he discovered the existence of an equilibrium point for mixed strategies in non-cooperative games but provided no algorithm to compute this equilibrium in mixed strategies. In the equilibrium point, the players could not improve the payoff in their chosen situations. If one player altered their strategy, both players would lose some of their payoff. Therefore, they were dependent on each other. In 1994, Nash received the Nobel Prize in economics for his discovery (together with Reinhard

[33] Philip Mirowski, *Machine Dreams*, (cf. note 6), pp. 327-329.
[34] Robin Rider, "Operations Research and Game Theory: Early connections", in Roy Weintraub (ed.), *Toward a History of Game Theory*, (London, 1992), pp. 225-237, here p. 229, p. 236. William Riker, "The Entry of Game Theory into Political Science", in Roy Weintraub (ed.), *Toward a History of Game Theory*, (London, 1992), pp. 207-224, here p. 216.
[35] Paul Erickson et al., *How Reason Almost Lost Its Mind,* (cf. note 7), p. 140.
[36] (cf. note 1).
[37] Mina Rees, "The Computing Program of the Office of Naval Research, 1946-1953", (cf. note 3), p. 110. Paul Erickson, *The World the Game Theorists Made*, University of Chicago Press 2015, p. 101.
[38] Harold Kuhn and Albert Tucker (eds.), *Contributions to the Theory of Games*, Princeton UP, 1950, foreword of Kuhn and Tucker.

Selten).[39] Between 1950 and 1954, Nash published some minor papers on Game Theory at the RAND Corporation. Afterwards, he turned to pure mathematics, as in the famous Hilbert problems. John Von Neumann and the Game Theory group at RAND rejected the approach of Nash's equilibrium.[40]

To demystify the concept of the Nash equilibrium I give a simple example in pure strategies in tables 4 and 5 which display simple domination points – the concept of dominant strategies was already known from two persons games. Examples of this kind entered the books on microeconomics in the 1980s. The example consists of modified values of the tables 1 and 2. This example shows further, how the concept of a Nash-equilibrium implies some kind of cooperation. They contain the large values 10 and 25 in row 3 and column 2. These dominant values appear in cell (3,2) in both payoff tables. In this case, player A cannot improve his situation when he chose line 3. Player B makes the best choice in selecting column 2 when player A had already chosen line 3.

Table 4 A's Profit. Example of a payoff table with a domination point.

	A's Profits		
	B_1	B_2	B_3
A_1	2	8	1
A_2	4	3	9
A_3	5	**10**	7

Table 5 B's Profit. Example of a payoff table with a domination point.

	B's Profits		
	B_1	B_2	B_3
A_1	11	2	20
A_2	9	15	3
A_3	8	**25**	6

The Nash-equilibrium appears to be a simple domination concept in pure strategies. But if the Nash-equilibrium does not exist in pure strategies, one could find it with the aid of mixed strategies, as Nash showed. But these strategies remained unknown because they could not be computed.[41] The MPI-group recognized Nash's new concept of cooperation, in contrast to Von Neumann's two person games.[42] Because Nash did not deliver an algorithm to determine the mixed strategies, Von Neumann criticised the Nash equilibrium as a pure existence assertion – nothing else as a fixed point theorem.

[39] John Nash, "Non-Cooperative Games." *The Annals of Mathematics*, vol. 54, 1951, (2), pp. 286-295. Already in 1937, von Neumann discovered an equilibrium point by application of Brouwer's fixed point theorem.
[40] Philip Mirowsky, *Machine Dreams*, (cf. note 6), p. 334.
[41] Hal Varian, *Intermediate Microeconomics*, New York 2014, p. 542.
[42] Paul Erickson et al., *How Reason Almost Lost Its Mind,* (cf. note 7), p. 141.

In the 1980s, Game Theory entered microeconomics courses at universities through a rediscovery of the Nash equilibrium, but only in pure strategies.[43] The lack of application induced the lecturers of microeconomics to present invented textbook examples of Game Theory that are not derived from empirical research. The Chicken Game describes the behaviour of teenagers in suburbs. The students in the classroom may have rolled their eyes and asked why this example was important for economics. Some economists argued that Game Theory had been important in resolving the Cuban Crisis of 1961 – a claim that was rejected by the MPI–group.[44] Other than applied economics, Game Theory lacks an intermediate layer between theoretical concepts and application in society. In macroeconomics one can derive, from the concept of Production Theory, for example, the Cobb-Douglas production function from empirical data, and answer the following question: How much does the gross domestic product increase if the supply of labour force increases by 100.000 people? Game Theory cannot answer questions of this kind. Also, Social Sciences provide many techniques, in terms of converting theoretical concepts into empirical measurement, that were not picked up by Game Theory.

The famous Prisoner's Dilemma game is not an abstractification of social relations in prisons, but an invention of the RAND mathematician Merill Flood. He used this game theoretic setting to derive arguments against Nash's equilibrium concept.[45] There are many accounts of Prisoner's Dilemma. I will draw on the most methodologically careful study on Prisoner's Dilemma, which was completed by Anatol Rapoport and Albert Chammah. They showed that this type of game is an abstractification of the behaviour of two competing firms to prevent their markets from excess capacity by joint quotas. Not playing the game only one time, Rapoport and Chammah showed incentives to leave a common cooperative position and end at a defect.[46] This abstractification provides a suitable frame for interpretation in a duopolistic case of firms' competition but gains no new insights beyond the existing literature on duopolistic behaviour.[47] For the Cold War intellectuals at RAND, the Prisoner's Dilemma game was central to describing a rational choice in the conflict between the USA and Soviet Union, as the MPI group pointed out.[48]

Game theory at RAND

Besides what was happening at Princeton, the RAND Corporation also developed as a centre of Game Theory. John Von Neumann played an important role in establishing the research program at RAND and a strong group for Game Theory.[49] RAND had already edited a bibliography on Game Theory, with more than 200 entries, in 1952.[50] The RAND Corporation was an ideal environment for Game Theory. It was assumed that in the Cold War, the

[43] See for example Hal Varian, *Intermediate Microeconomics*, (cf. note 41). Philip Mirowski, *Machine Dreams*, (cf. note 6), p. 348.
[44] Paul Erickson et al., *How Reason Almost Lost Its Mind,* (cf. note 7), p. 141.
[45] Philip Mirowski, *Machine Dreams,* (cf. note 6), pp. 334, 354.
[46] Rapoport and Chammah, *Prisoner's Dilemma*, (cf. note 14), p. 26.
[47] See Guerrien (cf. note 1) for cases of duopolistic behaviour.
[48] Paul Erickson et al., *How Reason Almost Lost Its Mind,* (cf. note 7), p. 320.
[49] Philip Mirowski, *Machine Dreams*, (cf. note 6), 212s.
[50] RAND Publication RM-950, Santa Monica 1952. See also Angela O'Rand, "Mathematising of the Social Science in the 1950s: Early Development and Diffusion of Game Theory." In Roy Weintraub (ed.), *Toward a History of Game Theory*, London, 1992, pp. 177-189. Robin Rider points out to compile bibliographies provided high reputation for the new field of Game Theory, see her paper "Operations Research and Game Theory: Early connections." In: Roy Weintraub (ed.), *Toward a History of Game Theory*, London, 1992, p. 226.

application of Game Theory would be a useful aid for politicians. John Williams, head of the mathematical department at RAND, wrote a popular book on Game Theory for the intelligent layman. In the 1950s, Game Theory was seen as an esoteric and mysterious subject, familiar only to specialized researchers, particularly those in the military. The book *The Compleat Strategyst – Being a Primer on the Theory of Games* was published in RAND's book series in 1954. It aimed to bridge the gap between Game Theory and the public, and was very successful, being pressed ten times and translated into various languages.[51] It even entered the Eastern Bloc, with Russian, Polish and Czech translations. Many universities used this book for their courses in Game Theory. It is remarkable that the book did not rely on complex calculations where a digital computer would be needed but carried out only simple calculations that could be done on a calculator. This conclusion does not support the commonly held view of a close interrelation between digital computers and Game Theory. In the second revised edition of 1966, the book had a sixth chapter added, in which it showed how to compute a saddle point in mixed strategies with the aid of Linear Programming, indicating a close connection between these two strands of theory.

The later-to-be-famous Lloyd Shapley also worked at RAND and issued a long list of RAND-papers on cooperative n-person games. He understood the players as numbers 1,2,…,n and considered subsets of the player set {1,2…,n}. He assigned to each subset ("coalition") a value v, that could be understood as a yield in a working cooperative (coalition), or as a voting power of the coalition in an assembly.[52] Shapley measured the marginal contribution of an individual i to a coalition C as the difference of the coalition's value, once with i as member of C, and once without i. The Shapley value of the individual i became famous as the average marginal contribution over all possible coalitions. The value v was derived from mathematical axioms but not from results of experimental Game Theory. So, the construction of the theory followed, only on a nominal level, the phenomena of social, economic or political life to mediate an intuitive understanding of the reader, but not to investigate empirical phenomena. Shapley made this nominal view explicit as he, in a paper on voting in a stockholder's meeting, underlined that this paper would only be nominal to help the reader, but should not be applied to joint stock companies.[53] In another RAND-paper he judged his examples for games as "artificial".[54] From the years 1950 to 1954 John Nash also held, during the summer months, short term contracts at RAND, where he published small RAND-papers on cooperative two person games in which he reduced to the non-cooperative case and an analysis of the board game "Hex", which was popular in Denmark.[55]

Operations Research

This section provides an overview of the institutionalization of Operations Research (OR), shows reasons for the barriers of application of OR, and describes OR as a research field for mathematicians. Operations Research is the application of mathematical models for planning

[51] John Williams, *The compleat Strategyst*, New York 1954, (cf. note 22), foreword. Williams died in 1964, see the obituary in the *New York Times* on November 22, 1964.
[52] Lloyd Shapley, "A Value for n-Person Games." In Albert Tucker and Harold Kuhn (eds.), *Contribution to the Theory of Games*, vol. 2, Princeton University Press, 1953, pp. 307-318.
[53] Lloyd Shapley, "Values of Large Games III, A Corporation with two large Stockholders." Research Memorandum RM 2650, RAND Corp., 1961, p. 1.
[54] Lloyd Shapley, "Values of Large Games IV", Research Memorandum RM 2650, RAND Corp., 1961, p.1.
[55] John Nash, "Two Person Cooperative Games", RAND Paper P-175, 1950. John Nash, "Some Games and Machines for Playing Them", RAND paper D-1164, 1952.

in administration, in manufacturing enterprises or in transport enterprises and comprises heterogeneous mathematical theories such as Game Theory, production planning, storage policy, networks and queuing theory, with Linear Optimization as a centre. After gathering data, the mathematicians look within their models for the optimal solution in order to minimize costs or maximize profits in a company.

During WW2, OR was founded in Great Britain and the US, developing methods to detect aircraft and submarines. In the UK, the group for Naval Operational Research was founded, and in the US, the Antisubmarine Warfare Operations Research Group (ASWORG). After WW2, the US Navy Operations Evaluation Group (OEG) maintained special OR knowledge, with a reduced staff and further development of OR methods during peace time.[56]

As a newly established branch of the military in 1947, the US Air Force was eager to get a reputation for the application of scientific methods in planning and using the digital computer – expected in the future – for this task as a circular letter from the Chief of Staff on 13 October 1948 indicated.[57] The Air Force developed the optimizing technique Linear Programming as the core of Operations Research during the project SCOOP at the RAND Corporation, 1947 – 1953. This project has already been described in various accounts.[58] The aim of this project was to accelerate the planning steps for a military operation, called a program. In expectation of the digital computer, the application of mathematical planning methods was to shorten the programming steps. The RAND mathematician Georg Dantzig invented a mathematical planning approach in 1947, calling it Linear Programming. It provided computational techniques to maximize a linear function over a convex and compact set in the n–dimensional number space that was spanned by linear inequalities.

As a showcase for Linear Programming application by the Air Force in the Cold War context, the SCOOP group also developed a model for the Berlin Airlift of 1948-1949 (Operation Vittel) and promoted it at various conferences. Abstractifying from the broad variety of aircraft models that were employed in the Berlin Air Lift, the model considered only C7 and C47 airplanes and determined the least costly schedule, taking fuel costs, crews and spare engines into account. The model was never used in day-to-day planning but served as a tutorial example to demonstrate the usefulness of Linear Programming. It attracted academic attention, and some dissertations on this model were written.[59] Murray Geisler, the head of SCOOP, guessed that the requirements of the Air Force were too extensive and surpassed the magnitude that a Linear Program could handle at that time. He guessed that 3600 variables and 3600 inequalities would be necessary.[60]

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[56] Carl Harris, "Center for Naval Analysis." In Saul Gass and Carl Harris (eds.): *Encyclopedia of Operations Research and Management Science*, Boston, Kluver 1996, pp. 62-64.
[57] Paul Erickson et al., *How Reason Almost Lost Its Mind,* (cf. note 7), p. 60.
[58] On the project SCOOP Paul Erickson et al., *How Reason Almost Lost Its Mind,* (cf. note 7), made a careful study. See also Paul Erickson, *The World the Game Theorists Made*, (cf. note 7), p. 96. Paul Ceruzzi, *Beyond the Limits. Flight enters the Computer Age*, MIT Press 1989, pp. 41-43. George Dantzig 1963, *Linear Programming and Extensions*, Princeton University Press, 1963, p. 15. This book appeared under copyright of the RAND Corporation. For the early history of Linear Programming and the forerunner Leonid Kantorovich, see Robert Dorfman, "The Discovery of Linear Programming." In *Annals in the History of Computing*, issue 3, July-September (1984 vol. 6), pp. 283-295. Murray Geisler, *A Personal History of Logistics*, Bethesda 1986, pp. 3-17.
[59] Murray Geisler*, A Personal History of Logistics*, (cf. note 58), p. 6. Marshall Woon and Murray Geisler, "Development of Dynamic Models for Program Planning", (cf. note 55). For the Operation Vittel see Paul Erickson et al., *How Reason Almost Lost Its Mind*, (cf. note 7), 56s.
[60] Murray Geisler, *A Personal History of Logistics*, (cf. note 58), p. 14. Marshall Woon and Murray Geisler, "Development of Dynamic Models for Program Planning." In Tjalling Koopmans (ed.), *Activity Analysis of Production and Allocation*, New York, 1951, pp. 189–215, p. 206.

For an observer, the way the SCOOP group fluctuated between local optimization in a firm or an organisation like the Air Force and the macroeconomic level of the economy appears curious. Ideas about central planning of the economy ("market socialism") were discussed, which prevailed in their enemy country – the Soviet Union. In market socialism, the firms operated independently but the prices of the goods were calculated by a central computer (the "superbrain").[61] Wassily Leontief's research also influenced SCOOP. In the Bureau of Labour Statistics, Leontief gathered data for a national Input–Output–Matrix and earned a high reputation. But this matrix, say A, with 200 rows and columns could only be used by means of a high speed digital computer, only available in the mid 1950s, since the "Leontief–Invers" matrix $(I–A)^{-1}$ had to be computed.[62] As a member of SCOOP, George Dantzig pointed out in a soviet manner at the conference on activity analysis 1949, Leontief's model could answer the central planning question of how much aluminium, steel and electrical power would be needed to meet the demands of a rise in weapon production.[63] As the historian of economic thought, Alexander Nutzenadel, critically noted, it remained open, however, whether the input-output tables merely represent an impressive collection of statistics, or whether they provide a benefit for economic policy decisions.[64]

Projects by the Air Force also pushed the jump from military to civil applications of Linear Programming in administration and industry. Contracts were made with the universities of Chicago and Pittsburgh, where they were generalized to "Operations Research" by Tjalling Koopmans, Abraham Charnes and Herbert Simon.[65] In 1949 – only two years after Dantzig's discovery – RAND organized the famous conference on Linear Programming at the University of Chicago, announced as the "Activity Analysis of Production and Allocation", followed by the First Symposium in Linear Programming in Washington D.C., under the joint auspices of the RAND Corporation and the National Bureau of Standard, in 1951.[66] Both conferences were held without any experience in the high speed digital computers, which were only available at RAND in 1953. Together with the oil refinery manager Bob Mellon, the University of Pittsburgh made a Linear Programming project for the lowest cost blending of aviation gasoline under contract of the Air Force. The model contained 22 variables and was solved by means of office calculators. The authors Charnes et al. did not mention the digital IBM CPC machine or even a digital computer. The motivation of the Air Force contract remains unclear. Was there a prevailing shortage of aviation gasoline? Or was the issue "aviation gasoline" a sufficient justification for an Air Force contract? These questions shed light on the diffuse motivation of

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[61] Dorfman, Robert, Paul Samuelson and Robert Solow, *Linear Programming and Economic Analysis*, New York, 1958, p. 395. This book appeared under copyright of the RAND Corporation. Philip Mirowski, *Machine Dreams,* (cf. note 6), p. 259.

[62] Frederick Moore, "A Survey of Current Interindustry Models." *National Bureau of Economic Research*, 1955, pp. 215–252. For the computing time of matrix inversion on various machines see Saul Gass, "The First Linear-Programming Shoppe." *Operations Research*, 50 (2002), issue 1, pp. 61-68, here p. 62.

[63] Marshall Woon and George Dantzig, "The Programming of independent Activities." In Tjalling Koopmans, *Activity Analysis of Production and Allocation*, New York 1951 (cf. note 60), pp. 15-18, here p. 18. Marshall Woon and Murray Geisler, "Development of Dynamic Models for Program Planning" (cf. note 60).

[64] Nützenadel, Alexander: *Stunde der Ökonomen: Wissenschaft, Politik und Expertenkultur in der Bundesrepublik, 1949-1974*, Göttingen 2005, p. 108.

65 Stephen Johnson, "Three Approaches to Big Technology", (cf. note 6), p. 898. Paul Erickson et al., *How reason almost lost its mind*, (cf. note 7), p. 72. Judy Klein, "The Cold War Hot House for Modeling Strategies at the Carnegie Institute of Technology", lecture at First Annual Conference on the History of Recent Economics (HISRECO), University of Paris X -Nanterre, France, 21-23 June 2007. Tjalling Koopmans, *Activity Analysis of Production and Allocation*, New York 1951.

[66] Tjalling Koopmans, *Activity Analysis of Production and Allocation* (cf. note 60). George Dantzig, *Linear Programming*, (cf. note 58), p. 25.

the Air Force in its R&D policy.[67] The consulting firms also established OR-groups, as William Thomas pointed out in his study.[68] In 1953, Abraham Charnes and William Cooper published the first textbook on Linear Programming.[69] Scientific societies and journals were founded in the 1950s, such as the Operation Research Society of America (ORSA) in 1952 and the Institute for Management Sciences (TIMS) in 1953. In the 1960s, ORSA reached the amazing number of 8000 members.

The founding of ORSA and TIMS were not responses to requests from the industry for OR applications but were rather an autonomous movement of expert mathematicians supported by military agencies. In his book on the automation movement, Herbert Simon characterized Operation Research as a new science of management that was pushed by mathematicians.[70] In a conference on computer and management in 1955, Simon saw in Operations Research a possibility to automate management decisions. OR–models should be applied on the new high speed digital computers, available since 1953.[71] But his hope was not fulfilled. OR–experts were mathematicians not acquainted with empirical data and applications to computers. OR–textbooks contained purely mathematical models without implementation on digital computers.

In the 1950s, Operations Research established chairs in departments of management at universities in the US and Great Britain, and in the 1960s OR chairs opened in Belgium, Switzerland and West Germany. In Zurich, the mathematician Hans Kunzi, who held a doctorate in mathematics, occupied even two parallel OR chairs and became president of the Swiss OR Society.[72] In 1975 the German OR professor Hans-Juergen Zimmermann (Technical University of Aachen since 1969) merged eleven national OR societies in Western Europe (excluding the Eastern bloc) under the umbrella "EURO".[73] The mathematical economist Martin Beckmann achieved a leading position in the European OR network when he held an OR professorship at the University of Bonn (Germany) in 1963. Together with OR professor Hans Kunzi, he edited even two series: the "Lecture Notes in Economics and Mathematical Systems" and the "Lecture Notes in Operations Research and Mathematical Economics" at Springer publisher, from 1968. Both series grew explosively, each with 16 titles per year.

Despite its successful institutionalization, OR's application in industry remained minimal. The president of TIMS, the RAND mathematician Merill Flood, admitted in his presidential address

[67] Charnes, A., W. Cooper and B. Mellon, "Blending Aviation Gasoline. A Study in Programming Interdependent Activities in an Integrated Oil Company." *Econometrica*, vol. 20, 1952, no. 2, pp. 135-159.
[68] Thomas, William, "Operations Research vis-à-vis Management at Arthur D. Little and the Massachusetts Institute of Technology in the 1950s." *Business History Review* 86 (2012), pp. 99-122.
[69] Abraham Charnes and William Cooper, *An Introduction to Linear Programming*, New York 1953.
[70] Herbert Simon, *The New Science of Management Decision*, New York 1960, 14s.
[71] Russel Ackhoff and Herbert Simon, *Proceedings of the Automatic Data Processing Conference*, Graduate School of Business Administration, Harvard University, Boston, 1955. For this conference see also Thomas Haigh, "The Chromium–Plated Tabulator: Institutionalizing an Electronic Revolution, 1954–1958", *Annales in the History of Computing*, 23 (2001), issue 4, 75–104, here 77.
[72] Charles West Churchman, Russell Lincoln Ackoff and Leonard Arnoff, *Introduction to Operations Research*, New York 1957. Philip Morse, "Report on the First Summer Program in Operations Research at M.I.T.", in Journal of the American Operations Research Society, 1, 1953, 303-305. Rudolf Henn and Hans P. Kuenzi, *Einführung in die Unternehmensforschung*, two volumes, Berlin 1968. Philip Mirowski, *Machine Dreams*, (cf. note 6), p. 490.
[73] Bulletin 1 of the European Association for Operational Research, 1975.

of 1955 that OR laid only "in the air".[74] OR researchers had to notice that data collection in an enterprise involved "organized human behaviour" which the mathematicians did not expect.[75] From the management of enterprises, it is known that to gather data inside an enterprise is both tedious and expensive and raises tensions. Management had to balance quality of data and the costs of gathering it and was inclined to use rules of thumb.[76] Because the OR-consultants had to jump over the barrier of high quality data to apply refined methods of Operations Research, the extent of its application in enterprises was low. Lewis Bodin, for example, wondered – when facing 20 years of research – about the low degree of application in the field of vehicle routing for milk collections on farms in the countryside, or the routing of school busses in the suburbs in 1990.[77] When one takes into regard the promises of cost savings, OR consultants could only handle this to a small degree, because many industrial processes carried a high burden of overhead costs, so that a reduction of, say, 5% of variable costs seemed rather unconvincing. In addition to this, many processes exhibited a cost curve that had only a flat minimum at the optimal solution, so that deviations from that point did not carry weight and rules of thumb seemed justified. In the literature, no cost curve is seen that manifests a sharp minimum such as a cleft in a rock and would justify a costly search for the optimal solution.

Although Churchman et al. gave, in their OR book of 1957, some warnings that scholars should not concentrate on methods but had to gather data and become acquainted with social relations inside the enterprise from which they were commissioned, mathematicians ignored these warnings, did not gather data and successfully captured the scientific staff in economics departments of universities.[78] Other than the books by Churchman et al., in which methods of data collection in steelworks and at turn-pike stations in New York are shown in detail, the mathematicians turned their books on Operations Research to pure method bibles.[79] The triangle data-model-computer remained blank. Oriented to mathematical methods, the mathematicians had no experience in social sciences with which to gather in enterprise data for their models. The scholars had no data – so they needed no computer. Remarkably, OR textbooks do not refer to computing, although personal computers had been widely available since the 1980s and spreadsheet software could easily template network models.[80] The scholars compensated for the lack of data by inventing data at their office desks. Every example in university lectures on Game Theory were invented payoff tables. Dantzig (1963), for his book on Linear Programming, invented examples of the transportation problem, the traveling salesman problem and the diet problem, as shown in the following sections.

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[74] Merrill M. Flood, "The Objectives of TIMS" *Management Science*, Vol. 2, No. 2 (Jan., 1956), pp. 178-184.
[75] Russel Ackhoff, "The Development of Operations Research as a Science" *Operations Research*, vol. 4, no. 3, 1956, pp. 268-275, here p. 268.
[76] Robert Kaplan, *Cost & effect: using integrated cost systems to drive profitability and performance*, Harvard UP 1998. Churchman et al., *Operations Research*, (cf. note 66), p. 16.
77 Lewis Bodin, "Twenty Years of Routing and Scheduling" *Operations Research*, vol. 38, no. 4, 1990, pp. 571-579.
[78] Churchman et al., *Introduction to Operations Research*, (cf. note 72), chapter 21.
[79] The first volume of the two volume book of Henn and Kunzi, *Einführung in die Unternehmens-forschung, Berlin 1968,* contained no OR at all, but a basic course in mathematics (linear algebra and calculus). OR invaded successfully also the Eastern Bloc. As a remake of the book of Henn and Kunzi in 1971 appeared a three volume book on OR in Eastern-Berlin. Werner Dueck and Manfred Diefenbach (eds.), *Operationsforschung*, also in volume 1 pure mathematics.
[80] Martin Campbell-Kelly, "Number Crunching without Programming: The Evolution of Spreadsheet Usability", in *Annals of the History of Computing*, Volume 29, Issue: 3, July-Sept. 2007, pp. 6-19.

The artificial content of Cold War Operations Research

The following sections discuss the Transport Model, the Travelling Salesman Problem, and the Diet Problem and highlights their artificial content derived from Cold War Operations Research. But, also other OR questions focus on this artificial content and have not been applied in business, so they remained academic, as is explained here. The literature reveals a lack of critical accounts on these OR–problems.

Dynamic Programming designs models of optimal decisions over time and assumes a fixed future time horizon. As a RAND researcher, the mathematician Richard Bellman first published on this subject in 1957 and found many imitators.[81] It was assumed that Bellman could repeat Dantzig's success with a new approach 10 years after his Linear Programming. This approach explicitly included the time dimension of economic action and divided the future course of time into different periods in which different policy options could be chosen. In a sensational because contraintuitive approach, Bellman first determined the optimal policy in the end period and gradually worked his way back from there to the present time (backward recursion). Dynamic programming was ideal for OR models, since there is no empirical data on future developments, i.e. researchers do not have to work empirically. Like Linear Programming, Dynamic Programming was only able to find optimal solutions with the help of computers because of the complex calculations involved. In 1979, Christoph Schneeweiss pointed out the high main memory requirements of reverse recursion, which could only be met for very small models using the then state of the art computer technology.[82] Thus Dynamic Programming was not in a position to provide calculation programs for the worldwide spare parts supply of the Air Force, as Judith Klein assumed in her study on Cold War Dynamic Programming.[83]

The question why Dynamic Programming was superior to simple decision rules based on uncertain assumptions about future developments, such as investment decisions, remained unanswered. With abstractification, Dynamic Programming transformed uncertain data about the future into seemingly secure, accurate data and does not reflect the curiosity of applying an elaborate, accurate algorithm to uncertain data. Judy Klein's critique of Dynamic Programming as Cold War Science also fails to recognize this weakness of Dynamic Programming.[84]

The network flow model simplifies the partial differential equations known as Navier-Stokes equations about flowing liquids in tubes developed by engineers and physicists in the 19th century. The network flow model abstracts the complex Navier-Stokes equations to such an extent that no friction occurs during the transport of liquids in tubes, i.e. the transport is lossless and vortex-free. In this simplified context, the mathematicians Lester Ford and Delbert Fulkerson were able to formulate the famous duality theorem “Max-Flow-Min-Cut” in 1956.[85] But applications of network flow models remained unknown. The network models of Operations Research were not included in the debate about the network expansion of important infrastructures, such as the electricity grid or the gas pipeline network.

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[81] See for example Martin Beckmann, *Dynamic Programming*, Berlin 1968.
[82] Schneeweis, Christoph: Dynamische Programmierung, in: Beckmann, Martin, Günter Menges und Reinhard Selten (Hersg.): *Handwörterbuch der Mathematischen Wirtschaftswissenschaften*, *Teilband Unternehmensforschung*, Wiesbaden 1979, p. 32.
[83] Klein, Cold War, 2007, cf. note 7.
[84] Klein, ibidem.
[85] Lester Ford Jr., D.R Fulkerson: “Maximal flow through a network.” *Canadian J. Mathematics*, 8, 1956, S. pp. 399-404.

The Quadratic Assignment problem was first formulated by the mathematical economists Martin Beckmann and Tjalling Koopmans in a joint article in Econometrica in 1957, which became famous and was cited about 1500 times.[86] Beckmann and Koopmans worked together at the Cowles Commission in Chicago.[87] Their article deals with an question that only appears at first glance as an economic problem, namely the spatial arrangement of different production plants on given settlement areas. Hypothetical – empirical data were not available – supply relationships are assumed among the enterprises that are included in the model being measured in tons. The spatial distances in kilometres between the factories are known. The question is how the factories should be optimally arranged on the land so as to minimise the transport performance (tonnes*km) when goods are exchanged between the factories. There were also publications at the company level dealing with the arrangement of machinery in an industrial plant with regard to the exchange of intermediate products.[88] The abstractification underlying the Quadratic Assignment problem becomes clear in the one-dimensional goal of minimizing the transport performance. In contrast to Beckmann's assertion that Operations Research can be applied in complicated decision-making situations,[89] the authors Koopmans and Beckmann reduced the complexity of the decision-making situation of the Quadratic Assignment problem to one dimension of transport performance. In a democratic society, the Quadratic Assignment problem is hung in a vacuum. Only soviet planners in Stalinism could gather so much power to take such a one-dimensional approach to the settlement of factories. In democratic societies, however, a large number of criteria are incorporated into location policy. The configuration of factories with machines also has a similarly complex goal bundle, as Gerhard Waescher has demonstrated in his standard book.

In computer science and combinatorial mathematics, the Quadratic Assignment triggered a flood of publications, for example in the Handbook of Combinatorial Optimization, which was last published in five volumes in 2013 and already had predecessor editions.[90] This problem could only be solved exactly up to a problem size of n = 30 by 2013. However, applications with empirical data remain unknown. Axel Nyberg claimed in his lecture on November 15, 2013 at the Abo University in Turku (Finland) that the hospital in Regensburg in Germany, built in 1972, had an optimal layout according to the Quadratic Assignment problem.[91] However, this was only proven in 2000 and could therefore not have played a role in the construction.

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[86] Tjalling Koopmans and Martin Beckmann: "Assignment Problems and the Location of Economic Activities." *Econometrica*, Vol. 25, No. 1 (Jan., 1957), pp. 53-76.
[87] Mirowski, Machine Dreams, (cf. note 6), p. 252.
[88] Waescher, Gerhard: *Innerbetriebliche Standortplanung bei einfacher und mehrfacher Zielsetzung*, Wiesbaden, 1982.
[89] "Mathematical methods are finding ever more applications in the economic and social spheres, especially where decision-making in complicated situations is at stake. Operations Research in particular, which involves the application of mathematical models for economic decisions, has developed rapidly due to this need...", (translated from German by R.V.) in: Beckmann, Martin, Gunter Menges und Reinhard Selten (eds.): *Handworterbuch der Mathematischen Wirtschaftswissenschaften, Teilband Unternehmensforschung*, Wiesbaden 1979, preface.
[90] Burkard, Rainer: "Quadratic Assignment Problems." In: *Handbook of Combinatorial Optimization*, pp. 2741-2814, edited by Panos M. Pardalos, Ding-Zhu Du and Ronald L. Graham, Springer Verlag, 2013. The mathematician Burkhard supplies a bibliography: Burkard, Rainer: "Quadratic Assignment Problems." In: *Handbook of Combinatorial Optimization*, pp. 2741-2814, ibidem.
[91] Nyberg, Axel: "Applications of the Quadratic Assignment Problem", see: http://web.abo.fi/fak/tkf/at/ose/doc/Pres_15112013/Axel%20Nyberg.pdf.

Computed meals as mathematical entertainment

To attach a semblance of application, Dantzig invented new OR–problems to be solved with the aid of Linear Programming: the diet problem and the traveling salesman problem. Here I will focus on the diet problem. This problem was invented by the later Nobel Prize winner and economist George Stigler in 1945. It is a strange problem: How to nourish a person sufficiently for the lowest cost? Stigler contrasted the content of nutrients in various foods (such as vegetables, fruit and meat) with the cost of its procurement and asked how to serve a meal for a person with sufficient nutrients at the lowest cost.[92] Stigler's paper exists in a vacuum and is not linked to the economic situation of the US in 1945. Many consumption goods were rationed due to the war. The municipal and state run programs on social welfare focussed on poor people. Did Stigler want to reduce the cost of these programs? Why did Stigler search for the lowest cost, not for the second lowest or even the maximum cost? The strange diet problem survived for many decades in Operations Research textbooks, without any explanation as to why it might be useful.

In 1947, Jack Laderman of the Mathematical Tables Project in the National Bureau of Standards solved the diet problem with the new technique of Linear Programming. His approach consisted of 9 equations and 77 variables, and he solved it with the aid of office calculators, as an academic exercise without application. Dantzig devoted even a chapter in his 1963 book to this problem. Even on IBM's high speed digital computer 701, he coded the problem at the RAND Corporation, but his computed meals were never served to the pilots of Dantzig's employer, the Air Force. Dantzig did not recognize the double curiosity of applying advanced computational techniques to an invented problem based on only weak data – a problem that was neither posed by industry, councils nor the Armed Forces. As empirical data, he displayed in his book a table with nutrients, where the content of ascorbic acid varied by more than 100 percent between various types of apples.[93] So Dantzig could not answer the question of whether a pilot should eat one or two apples each day. Whereas the MPI–group regarded the diet problem as a serious scientific problem, one can criticise by stating that Dantzig's procedure lowered the cutting edge technology of high speed digital computers to the level of a toy, made purely for mathematical entertainment.[94]

The transportation problem as an abstractification

The transport model discussed in the following shows the paradox that its discoverer was awarded the Nobel Prize for Economics, but that his model was never applied in economic reality. The reasons for this failure will be explained. One can generalise this case to the effect that the transport model stands for many other models of the OR whose relevance is always only claimed.

The Transportation Problem is always an important chapter in every textbook on Operations Research and describes how to distribute the transport of goods between various sources

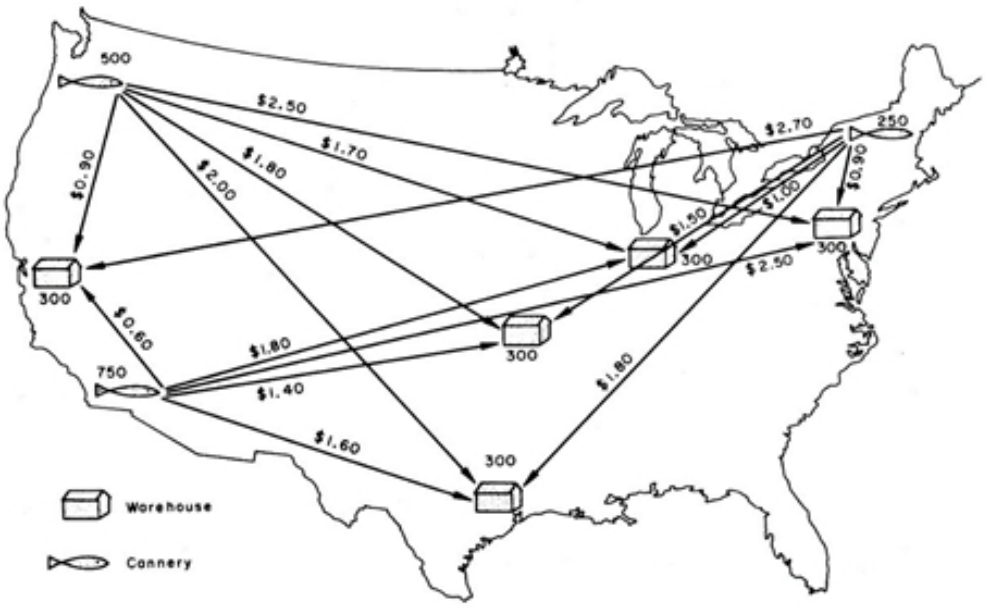
[92] George Stigler, "The Cost of Subsistence." *Journal of Farm Economics*, vol. 27, 1945, no. 2, pp. 303-314.
[93] George Dantzig, *Linear Programming*, (cf. note 58), pp. 551-553. Georg Dantzig, "The Diet Problem." *Interfaces*, vol. 20, 1990, no. 4, 43-47.
[94] Paul Erickson et al., *How Reason Almost Lost Its Mind*, (cf. note 7), p. 65.

and destinations in order to minimize the total costs of transport.[95] Regarding the Transportation Problem, one can reveal the nominal nature of this problem. The economic world is used to identifying and abstractifying transportation problems and converting them into simple mathematical models for the academic world, without the intention of solving a problem in the real world. During WW2, the mathematical economist Tjalling Koopmans – who earned a doctoral degree in mathematical physics in the 1930s – formulated the so-called Transportation Problem. He observed, as a member of the Combined Shipping Board, bottlenecks in the transport chain and received the Nobel Prize in economics in 1975 for his discovery of the Transportation Problem (together with the Russian scientist Leonid Kantorovich for his discovery of Linear Programming).[96] The Transportation Problem can serve as an important example for the procedure of abstractification. Koopmans envisioned suppliers and receivers of goods, but he narrowed the focus to only one kind of goods, so that it remained indifferent for a receiver from which supplier they get the goods. As a consequence, the model cannot handle different types of goods. A motor truck or a ship could not load different types of goods as it is common in the real world. Furthermore, Koopmans excluded the economies of scale – commonly prevailing in the economy – in transportation costs, so that the transportation of one ton had to pay the same rate as a transportation of 1000 tons. Finally, he did not consider fluctuations in transportation rates during the lapse of time, which are also common in the real world. In this stripped version of the transport problem, the reader can gain impressive insights into primal and dual variables and their economic interpretation. Very appealingly, this problem can be graphically sketched with a view of the fishing industry's locations, for example, by a map of the United States which displays where canneries and warehouses are located and connected by transportation relations. George Dantzig did this in his book already in the introduction on page 3 to underline the importance of his book, cf. figure 1.

As a nominal approach, Dantzig produced the map in figure 1 as an invention on his office desk, but not from empirical data of a contract with a cannery firm. While the map calls upon the authority of an important economic problem, this impression is misleading. Like Game Theory, until now, no application of the Transportation Problem has been published. Koopmans abstractified this problem so much that it remains in the world of numbers and could not gain traction in the real world. No enterprise in the transportation trade (ship, aircraft, railway, motor truck) called for a project to optimize routes by the transportation problem. Remarkably, many OR textbooks did not apply a spreadsheet software to present and compute the transportation problem but preserved old-fashioned methods for finding an optimal solution. The north-west rule and the stepping stone method were outdated in the age of spreadsheet software, where one can apply Excel's Visual Basic to determine dual variables.

[95] George Dantzig, "Linear Programming", (cf. note 58), chapter 14. Dorfman et al., *Linear Programming*, (cf. note 56), chapter 5.
[96] Tjalling Koopmans, "Optimum Utilization of the Transportation System." In *Proceedings in the International Statistical Conferences*, Vol. 5, Washington DC, 1947. (Reprint in *Econometrica*, vol. 17, 1949, Supplement). George Dantzig, *Linear Programming*, (cf. note 58), 300. For the discovery of Linear Programming by Leonid Kantorovich see Dorfman, "The Discovery of Linear Programming" (cf. note 53). Dorfman et al., *Linear Programming*, (cf. note 58), p. 284. The Transportation problem was also discovered in 1941 by Frank Hitchcock, "The Distribution of a Product from Several Sources to Numerous Location." *Journal of Mathematics and Physics*, vol. 20, 1941, pp. 224-230. For the research of Koopmans at the Cowles Commission at University of Chicago and the cooperation with RAND see Philip Mirowski, *Machine Dreams*, (cf. note 6), pp. 263–272.

Figure 1 The Transportation Problem in Dantzig's book 1963. Transportation inside the US between canneries and warehouses with transport rates.[97] (Source: George Dantzig: *Lineare Programmierung und Erweiterungen*, (German edition) Springer Verlag, Berlin, 1966, p. 3)



In the academic field of Operations Research, scholars were interested in their models but not in application, and so the question did not attract their attention in the 70 years since its discovery of 'why' the Transportation Problem is insufficient to be applied to problems in the world of economy. At first sight, the coordination of empty railcars in a railway company to be sent back to the sources of material seemed to be an appropriate application for the Transportation Problem. However German Railways did not coordinate their trains loaded with coal but rather used shuttle trains between the sources of coal and consumption destinations. Empirical research into railway systems revealed the time structure of transportation. The railway company needed forecasts for the demand of empty railcars that the Transportation Problem could not provide.[98]

The travelling salesman as invention

In the United States of the 1940s, the profession of the traveling salesmen was held in high esteem by the public. Dantzig took this up when he invented the so–called traveling salesman problem. Also, this famous problem arose in the academic environment of the RAND corporation as an invention of the mathematician Dantzig to shed some light of application on Linear Programming, but not as a contract with a firm that wanted to improve its sales organisation. At RAND, the Travelling Salesman problem was seen as an additional intellectual challenge to Game Theory. Dantzig abstractified a problem of the daily life of a traveling salesman to visit customers and proposed with a small semantic shift that a traveling salesman has to visit not a number of customers but a number of cities. Danzig's question was how to organise the travel visiting these cities with the least sum of distances to be travelled. The RAND researchers, the mathematicians George Dantzig, Delbert Fulkerson

[97] Also in their joint paper "A Model of Transportation" Koopmans and Reiter showed maps of shipping routes of the world, 245s, in Koopmans, *Activity Analysis*, (cf. note 60), pp. 222-259.
[98] Michael Gorman, "Empty Railcar Distribution." In Bruce W. Patty (ed.), *Handbook of Operations Research Applications at Railroads*, New York 2015, pp. 177 – 190.

and Selmer Johnson, proposed on their office desk a route through the 48 states of the United States where they picked for each state one city. The route contained even the thinly populated state of Montana with less than half a million inhabitants where a salesman could hardly sell products in contrast to heavily populated states as California or Pennsylvania. In addition, the district Washington D.C. was merged into the route – a route that a traveling salesman in the physical world never would travel. The road distances between the cities were derived as "desktop research" from a road atlas.[99] The proposed route through the 48 states of the United States did not serve a sales organisation to guide its salesmen but was a good marketing story of Dantzig as he – supported by a map of the United States – appealed to the national proud of US citizens in every state. He showed that Linear Programming is a unifying tie connecting the single states. Gass and Assad made the humorous remark in their timeline: "See the USA in a Chevrolet", underlining the not very serious approach of the Travelling Salesman problem.[100] In the last 60 years the traveling salesman problem, with its semblance of application, fascinated mathematicians with a steady growing number of cities to be visited – parallel to the rising computing power of digital computers – until by the year 2017 they considered a route through 1.9 million cities of the world. Empirical surveys on the need for solution methods for the Travelling Salesman problem in industry remained unknown. The leading OR scholar in Germany, Andreas Drexl, who was the leading researcher at the University of Kiel (Germany) according to the press release of his University of Kiel, reported in a press interview that he was impressed by the beauty of the Travelling Salesman problem. Merill Flood reported in his paper that he had heard of applications.[101]

Conclusion

This paper explores the influence that mathematicians took in the development of Game Theory and Operations Research at the RAND Corporation and in the academic world of mathematical and economic departments. It shows how mathematicians abstractified problems from social life to derive simple models as material for academic purposes and raises some doubts on the widely held view of important applications of Game Theory and Operations Research. The paper shows that important theorems in Operations Research were based on simple models and inventions and reveals the lack of empirical research. Examples, such as mixed strategies and the Transportation Problem, show how abstractification leads into the space of numbers where no applications in the real world were possible. The method of abstractification generates formal models that could not be supplemented by empirical data and lacks a layer of empirical research to generate data and apply their methods to economics and society. Therefore, their models were only nominal mathematics, without application.

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[99] G. Dantzig, R. Fulkerson and S. Johnson, "Solution of a Large-Scale Traveling-Salesman Problem." RAND Research Memorandum, P 510, April 1954.
[100] Gass and Assad, *Timeline*, 2005 (cf. note 12), p. 48.
[101] Press release University Kiel on 28 November 2005. Handelsblatt on 12 December 2005. "Flood, Merrill: The Traveling-Salesman Problem." *Operations Research*, Vol. 4, No. 1 (Feb., 1956), pp. 61-75., here p. 65.

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Real GDP: the flawed metric at the heart of macroeconomics

Blair Fix, Jonathan Nitzan and Shimshon Bichler [Toronto, Montreal and Jerusalem]

The study of economic growth is central to macroeconomics. More than anything else, macroeconomists are concerned with finding policies that encourage growth. And by "growth", they mean the growth of *real GDP*. This measure has become so central to macroeconomics that few economists question its validity. Our intention here is to do just that.

We argue that real GDP is a deeply flawed metric. It is presented as an objective measure of economic scale. But when we look under the surface, we find crippling subjectivity. Moreover, few economists seem to realize that real GDP is based on a non-existent quantum – utility. In light of these problems, it seems to us that much of macroeconomics needs to be rethought.

1. Calculating real GDP

Macroeconomists entertain two related measures of GDP: nominal GDP, which is the total *money value* of goods and services produced in an economy in a given period (say a year); and real GDP, which is the total *quantity* of these same goods and services.

The challenge for macroeconomists is that the "quantity" of goods and services – and therefore real GDP – cannot be aggregated directly. Since goods and services are qualitatively different, economists cannot sum their quantities in their natural units (try adding 10 lb of tomatoes to two laptops to five financial services). While each commodity bundle has its *own* quantity, these quantities are incommensurable.

Fortunately, there is a simple way around this difficulty – or so say the macroeconomists. To understand their solution, we need to backtrack a bit. Unlike real GDP, nominal GDP can be readily calculated in universal money terms. If the price of a commodity *i* is P_i and its quantity is Q_i, then its money value $Y_i = Q_i \times P_i$. Aggregating the money values across all n commodities produced in the economy gives us nominal GDP:

$$1. \quad nominal\ GDP = \sum_{i=1}^{n} Y_i = \sum_{i=1}^{n} Q_i \times P_i$$

As Equation 1 makes clear, over time nominal GDP can grow or contract for two reasons: (1) because *quantities* change (through greater or lesser production), and (2) because *prices* change (via inflation or deflation). And here, say the macroeconomists, lies the solution: if we "purge" nominal GDP from the effect of inflation and deflation, we end up with real GPD.

This purging is technically straightforward. Instead of multiplying each commodity by its *current* money price P_i (which changes from year to year), we multiply it by the price prevailing in a particular "base year" Pb_i. In this calculation, prices are always the same, by definition. And since the only things that change now are the quantities being produced, we end up with real GDP denominated in base-year prices:

$$2. \quad real\ GDP = \sum_{i=1}^{n} Q_i \times Pb_i$$

2. Which base year?

But there is a slight conceptual problem. It turns out that the growth of real GDP – ostensibly a single, objective quantity – is highly sensitive to our choice of base year.

To illustrate, consider a hypothetical economy that produces only two commodities: 1,000 lb of tomatoes and two laptops. Next, let's choose 1990 as our base year and assume that tomatoes in that year cost \$2/lb while a laptop costs \$2,000. In this case, real GDP, denominated in 1990 dollars, would be \$6,000 (=1,000 × \$2 + 2 × \$2,000). Now, skip to 1991 and imagine that, in that year, the economy grows by producing one additional laptop. This increase means that real GDP in 1991, denominated in 1990 prices, is \$8,000 (=1,000 × \$2 + 3 × \$2,000). Compared to 1990, real GDP grew by ***33.3 per cent***.

So far so good. Now, instead of using 1990 as our base year, let's use 1991. Production levels remain unchanged: 1,000 tomatoes and 2 laptops in 1990, and 1,000 tomatoes and 3 laptops in 1991. Base-year prices, though, are no longer the same: in 1991, our newly chosen base year, tomato prices double to \$4/lb, while laptop prices are halved to \$1,000. Under these new conditions, real GDP for 1990, this time denominated in 1991 dollars, is \$6,000 (=1,000 × \$4 + 2 × \$1,000), while real GDP for 1991, also in 1991 dollars, is \$7,000 (=1,000 × \$4 + 3 × \$1,000). Unlike before, in this example real growth is only ***17 per cent***.

And this is the simplest of examples. A slightly more involved example – for instance, one in which the production of laptops is rising and of tomatoes falling – might yield positive real GDP growth with one base year and negative real GDP growth with another.[1]

In other words, real GDP is affected not only by the *actual quantities* being produced, but also by *our choice of base year*. And since there are numerous base years to choose from, the same real GDP can end up having many different magnitudes!

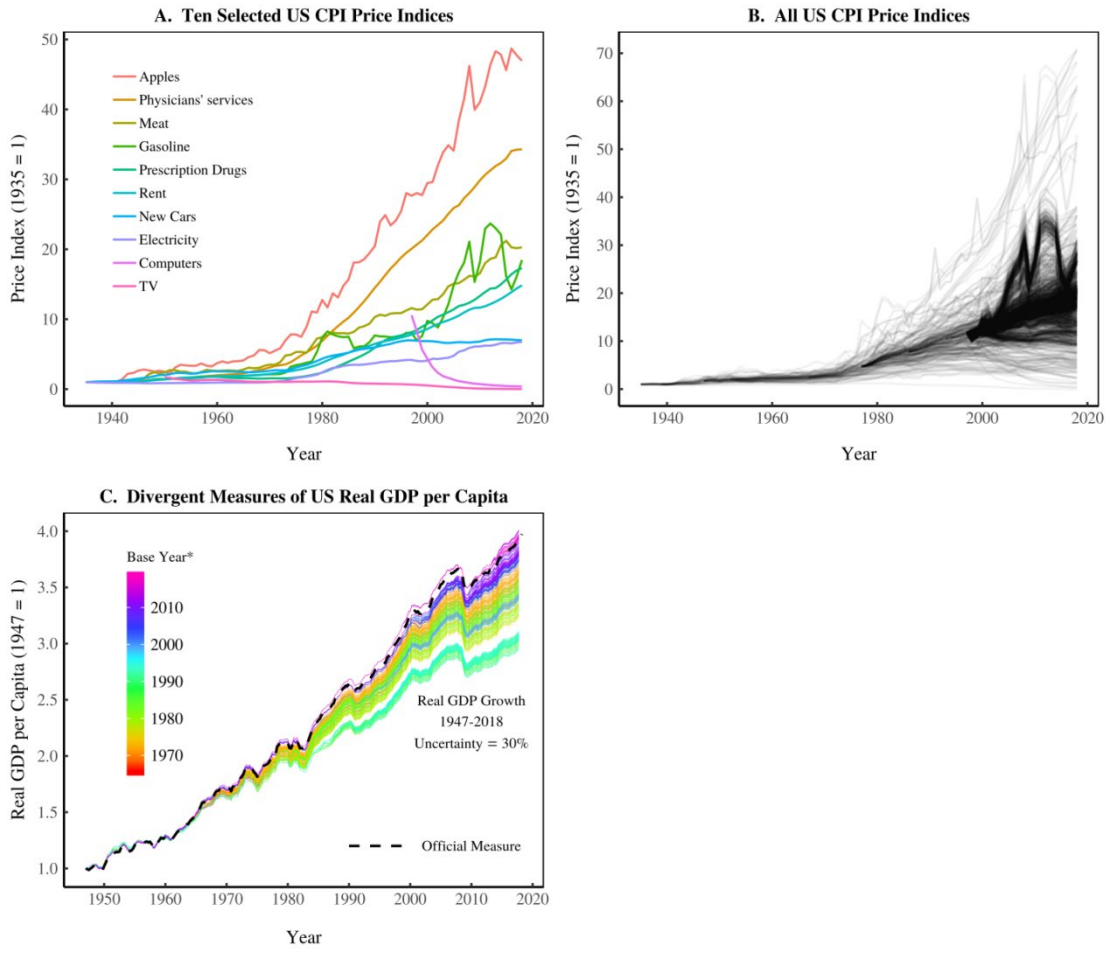
3. Inherent, irreducible uncertainty

The base-year problem logically means that there is *uncertainty* in real GDP. Because relative commodity prices change from year to year, each base year will generate a different measure of real GDP. And since there is no way to determine which base-year measure is "correct", the choice is always arbitrary. This arbitrariness leaves us with inherent, irreducible measurement uncertainty.

[1] For visual illustrations of the base-year effect, see Nitzan and Bichler (2009: Ch. 8), Bichler and Nitzan (2015) and Fix (2019: Section 2.2).

Here is the curious thing: economists do not report this uncertainty. Scientists know that measurement uncertainty must be reported. The uncertainty indicates the confidence in the measure. The larger the uncertainty, the less confident we are. If we do *not* report uncertainty, we are not telling the truth to other scientists. We make our measure appear certain when it is not. Although economists are aware of the base-year problem (it is taught to undergraduates), one will never find an official measure of the uncertainty in real GDP growth data. The government publishes only *one* measure of real GDP, with no reported uncertainty.

Figure 1 Divergent price change and divergent measures of real GDP



This figure was first published in Fix (2019). It shows how divergent changes in price affect the measurement of US real GDP. Panel A shows historical price changes in ten selected commodities tracked by the Bureau of Labor Statistics. Panel B shows divergent price change for all CPI commodities. Divergent price change means that the choice of base year has a strong effect on the measurement of real GDP growth, as shown in Panel C. For sources and methods, see the Appendix in Fix (2019).

In a recent paper, Blair Fix (2019) estimates the uncertainty in real GDP resulting from the base-year problem. To reiterate, this uncertainty is caused by instability in relative prices. Over the long term, this instability is spectacular. Figure 1A shows the divergent price change of 10 selected commodities from the US Consumer Price Index. Figure 1B shows the price

change of *all* CPI commodities. Figure 1C shows the resulting uncertainty in real GDP growth – about 30 per cent since 1947.[2]

Curiously, the *official* measure of real GDP is right at the upper range of this uncertainty. Is this a coincidence? Or have government statisticians simply chosen the method that yields the maximum growth (so as to appease their superiors)? This is an important question that, as far as we know, remains uninvestigated.

4. Chain-weighting the base-year problem

To reiterate, the base-year problem leads to uncertainty in the calculation of real GDP. But instead of openly reporting this uncertainty, government economists have devised a "fix". Rather than using a single base year, they "chain" together many adjacent base years. This is a bit like a moving average. They calculate the growth of real GDP between consecutive years, using the first year in each pair as the base, and then "chain" together the resulting growth measures to calculate real GDP levels. This method claims to "fix" or at least lessen the base-year problem. It doesn't.

The appeal of chain-weighting, according to economists, is that it gets closer to their theoretical ideal. According to this ideal, the weight of each commodity in real GDP is provided by its "true" or "natural" price. When using a single base year, the implicit assumption is that relative prices in that base year are "true" and therefore constitute the "correct" weights (Equation 2). However, if the "correct" weights change over time, and if these changes are mirrored in the movement of relative market prices, we can do better by changing the base year more often (every year) and chain-weight the results.

This argument is superficially convincing, but it falls apart on further inspection. First, chaining together base years is better than using a fixed base year *only if* the "true" weights indeed change over time, and *only if* "truth" here is indeed revealed by relative market prices. Unfortunately, there is no way to ascertain either "if". And as long as these two "ifs" remain hanging – which might be forever – chain-linked measures must be deemed as arbitrary as their fixed-based cousins.

Second, if the "correct" weights of commodities change over time we can no longer be sure that producing more units of a given commodity constitutes "real" growth. For example, the production of 20 per cent more laptops whose "correct" weight falls by 40 per cent *reduces* the "true" output of laptops by 28 per cent $(= (1 - 1.2 \times 0.6) \times 100)$. Moreover, changing weights makes temporal comparisons impossible, since the basic unit of measurement – the "correct" weight – is no longer fixed (more on this issue in the discussion of quality change below).

The only solution to the base-year problem would be if prices were stable. But since we cannot change history, this solution is unattainable.[3]

[2] Note that this estimated range of uncertainty assumes that at least one of the years since 1947 was a "correct" base year. However, if that assumption is false – in other words, if the "correct" set of relative prices was never mirrored in prevailing market prices – the range of possible real GDP measures can be much wider. Worse still, if we reject the very notion that there is a "correct" set of relative prices to start with, estimating the uncertainty range becomes impossible if not totally meaningless. The best we can do, then, is speak of a "possibility space" for real GDP, defined by the range of subjective measurement choices.

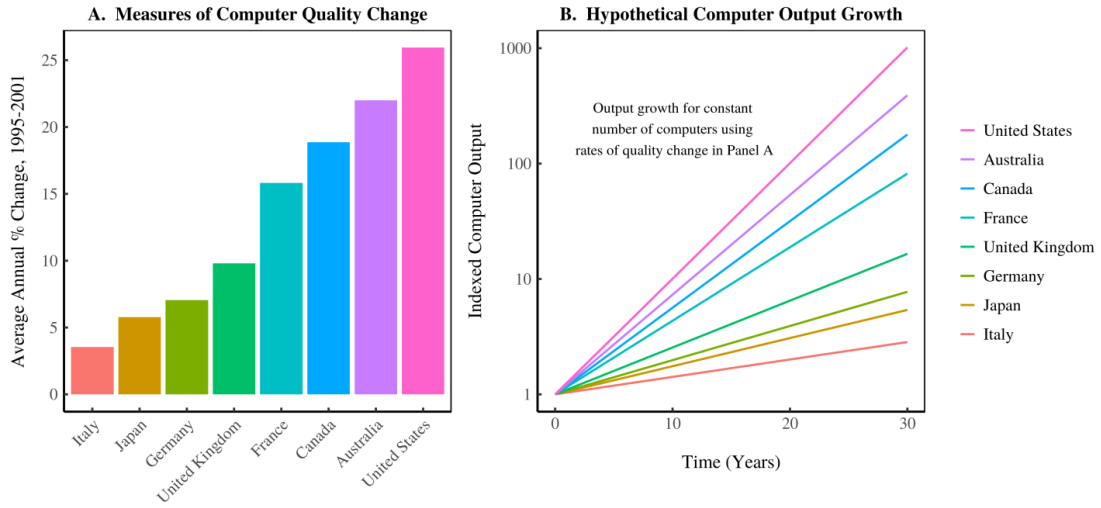
5. Unknowable unknowns: quality change

And this is just the tip of the iceberg. Lurking underneath the base-year issue is a far bigger problem – the measurement of quality change. And unlike the base-year problem, the scope of the quality-change problem is difficult, if not impossible, to estimate quantitatively.[4]

When measuring price change, economists attempt to adjust for changes in the *quality* of a commodity. An increase in the *quality* of a commodity is recorded as an increase the *quantity* of real GDP. So if computers get 10 times better, then computer output is recorded as increasing by a factor of 10.

And here arises the question: *how do we measure quality change*? Before diving into the specifics, we should recognize that there is little agreement on this topic. The governments of the world use different methods, and the result is wildly different measures of quality change.

Figure 2 Divergent measures of computer quality change



This figure was first published in Fix (2019). It illustrates the dispersion in national estimates of computer quality change. Panel A shows computer quality change estimates for eight OECD nations. Bars represent the average annual growth rate of computer quality between 1995 and 2001. Panel B shows how these quality-change measurements would affect the growth of computer "output" over 30 years. Assuming the number of computers produced remains the same in each year, the different quality adjustments lead to divergent measures of computer output growth spanning three orders of magnitude.

Take computers. Figure 2 shows the different measures of computer quality change used by eight different OECD countries. Now, to a first approximation, computers are the same everywhere. So these different measures reveal nothing about the actual change in computer quality. They are just an artefact of the different methods being used. If we project these different quality-change measures over 30 years, the divergence is spectacular. Assuming no

3 One of us (Fix) recently engaged in a lengthy debate with an (anonymous) economist who defended the practice of chain-weighting GDP. The exchange can be found on the capitalaspower.com forum: http://www.capitalaspower.com/forum/viewtopic.php?f=4&t=505

[4] Note that, at its root, the base-year problem is a quality-change problem: to halve or double the weight of a given laptop computer from one base year to another is to halve or double its relative quality-read-quantity. For simplicity, we discuss the two problems separately.

change in the underlying number of computers produced, we find a 1,000-fold disparity in the growth of computer output across the different countries. Clearly, we have a problem.

The problem is that measuring quality change requires numerous subjective decisions. There are so many such decisions, in fact, that it is virtually impossible to keep track of the ways that quality changes affect the measure of real GDP.

Natural scientists have the concept of "error propagation". In each step of analysis, we have uncertainty in our measurement. To keep track of this uncertainty, we "propagate" it through our calculation. If economists were serious scientists dealing with an objective reality, they would do the same with real GDP. Each time they made a subjective decision about how to measure quality change, they would keep track of the results that would have occurred if other choices had been made. This would give a *possibility space* for the range of possible measures of real GDP.

How large is this possibility space? We have no idea. In fact, since quality is partly subjective, this space might be undefinable (more on this below). But even if it can be defined, governments report only one measure of quality change. It is thus virtually impossible to know how alternative ways of measuring quality change would affect the measure of real GDP growth. This is an "unknowable unknown". At present, there is no way to estimate the uncertainty in real GDP that results from different ways of measuring quality change. And not only can we not answer this question, but most macroeconomists are not even interested in asking it. To ask the question is to admit the arbitrary nature of real GDP.

6. The unasked question: what is the unit of real GDP?

Most economists believe that "constant dollars" – i.e. dollars expressed in *fixed prices* of a given year – are the unit of real GDP. For instance, the Federal Reserve Bank of St. Louis reports that real GDP has units of "Chained 2012 Dollars". Unfortunately, this belief is false – or, worse still, meaningless. It is logically untenable when we reflect on the methods that go into measuring real GDP.

As soon as we start "adjusting" for quality change, we are no longer using prices as the unit of analysis. Instead, we are appealing to some *other* unit – the unit of *quality* that is hidden in the commodity. What is this unit? It is *utility* – the quantity of pleasure that consumers derived from a commodity. Here is the US Bureau of Labor Statistics describing how "hedonic" adjustments appeal to "utility" to measure quality change:

> In price index methodology, hedonic quality adjustment has come to mean the practice of decomposing an item into its constituent characteristics, obtaining estimates of the value of the utility derived from each characteristic, and using those value estimates to adjust prices when the quality of a good changes (Bureau of Labor Statistics 2018).

The problem is that this utilitarian approach is built on foundations of sand. Utility, even if it were commensurable across individuals, is *unobservable* directly. But economists are not deterred. They hypothesize that prices *reveal* the utility of a commodity. They then use prices to estimate the utility embodied in each characteristic of the commodity. This method allows them, or so they think, to measure quality change.

Unfortunately, the whole operation is circular. And when we look at the logic closely, it is indefensible. Prices are taken to reveal the utility of a commodity. But having made this assumption, we then find that prices change through time. This means that nominal prices cannot be trusted to reveal utility. So we have to "correct" for price change to measure the "true" change in utility. But we make this correction by appealing to *prices* – the very unit we just rejected. The logic is torturous when stated clearly.

In reality, economists never get close to measuring utility. Instead, their hedonic quality adjustment is an arbitrary algorithm for calculating quality change. It is based on a host of subjective decisions. These include the choice of the relevant characteristics of the commodity, the choice of functional form of the hedonic regression used to weigh these characteristics and the choice of the cross-section method. Different assumptions will yield different measures of quality change. And there is no way to know which measure, if any, is "correct".

As a PhD student, Jonathan Nitzan wrote a paper pointing out these difficulties in quality-change measurement (Nitzan 1989). But he found that the paper was unpublishable. He was scolded by reviewers. "These problems have been solved", they said. Unfortunately, the supposed "solutions" remain unknown to us, some 30 years later. In fact, we think that the problems are unsolvable. Economists assume that utility is the unit of quality. But this unit is unobservable – or put more strongly, it is *non-existent* (Nitzan and Bichler 2009) .

To summarize, whether openly or tacitly, the methods used for quality-change adjustment take the true unit of real GDP to be *utility*. To justify measuring aggregate utility, economists need a host of assumptions. These are:

(a) All consumers must be identical. This identity ascertains that utilities are commensurable and substitutable, and that the quantities of commodities, measured in utility, are independent of whoever happens to own them.

(b) Consumer preferences must be independent of income, so that a redistribution of income from poorer to richer consumers, or vice versa, will not alter the utility generated by a given array of goods and services.

(c) Preferences must remain temporally fixed to ascertain that, over time, a given array of goods and services will yield the same measure of "real GDP".

(d) All markets must be in a perfectly competitive equilibrium to ascertain that prices reflect the underlying utilities; alternatively, economists must know the "correct" prices that would have prevailed had markets been in a perfectly competitive equilibrium.

Since assumptions (a), (b), (c) and (d) are never satisfied, the resulting measures of "real GDP" are meaningless. In our view, the correct acronym for "real GDP" should be AWUGDU – pronounced "a-woogdoo". It stands for "Arbitrarily Weighted Unquantifiable Gross Domestic Utility".

7. Solutions: differential measures for income and assets and biophysical measures for scale

If real GDP is largely meaningless, as we have argued, the result is a conceptual void that fundamentally undermines the field of macroeconomics. It means there is no single measure of economic output on which to build a theory of economic growth. Consequently, much of macroeconomics must be questioned.

If we discard real GDP, then what are the alternatives?

We propose, tentatively, two different approaches. First, if we are interested in *income and assets*, then there is no need to use "real", inflation-adjusted metrics. We can simply compare the dollar value of one owned bundle of commodities to the dollar value of another (or the relative incomes these bundles generate). We call this a "differential" measure. Nitzan and Bichler (2009) have proposed a theory of capitalism that appeals only to differential measures. They named it *capital as power,* or CasP for short. These differential measures, the theory argues, represent not relative utilities but organized power. As capitalism advances and spreads, the theory continues, differential money values – for example the profit or market capitalization of Amazon relative to those of Apple – come to denote the power of their respective owners, while the grid of these multiple differentials increasingly approximates the overall power structure of society.

Regardless of whether one accepts this "capital as power" hypothesis, differential measures of money income and assets – unlike "real" utilitarian magnitudes – can be studied objectively.

Second, if we are interested in the overall *scale* of human production we can use *biophysical* measures. Fix (2015b, 2015a) has argued that *energy use* is an important measure of economic scale. Keen, Ayres and Standish (2019) have recently reiterated this idea. The laws of thermodynamics dictate that energy is essential for sustaining complex systems. Its necessity makes it a prime candidate for measuring the scale of production.[5]

Energy use can help us scientifically define the boundaries of production, as well as to assess the impact of that production on the biosphere. Note, however, that we do not equate *more* energy use with a *better* quality of life. More energy use is simply more energy use. To measure the quality of life – and human wellbeing more generally – we need a *new accounting system altogether*. This system must be based not on neoclassical notions of perfect competition and individual utility, but on a democratic articulation of what constitutes the "good life" and a "good society" within our broader biosphere.

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[5] Measuring energy use is not without problems, of course, but these problems involve far less built-in subjectivity that those plaguing the measurement of "real" GDP.

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Realism and critique in economics: an interview with Lars P. Syll

Lars P. Syll and Jamie Morgan [Malmö University, Sweden; Leeds Becket University, UK]

Lars Pålsson Syll is Professor of Social Science, Malmö University, Sweden. He holds two PhDs awarded in the 1990s. One in economic history and another in economics. His regular blog postings have made his name familiar to *Real-World Economics* readers. In 2018 his blog was recognized by Focus Economics (their list of bloggers also included Steve Keen and Michael Hudson).[1] His postings and publications have drawn on and drawn attention to the continued relevance of Keynes, the work of post-Keynesians, and the scope and potential of modern monetary theory. However, he is perhaps best known for his criticism of the methodological foundations of mainstream economics. His book *On the use and misuse of theories and models in mainstream economics (2016)* is published by WEA.

His work can be accessed at: https://larspsyll.wordpress.com

He is interviewed by Jamie Morgan for *RWER* ….

Jamie: Lars, perhaps a useful place to start would be with some introductory comment on what informs your reasoning. Whilst your postings range across many subjects you regularly return to a common theme. Specifically, the use economists make of mathematics to express theory and of analytical statistical techniques to conduct research. What are the key problems you see here and in what sense can or should matters of methodology provide a common thread to this common theme?

LPS: Well, I think the main problem here when it comes to applying mathematics and inferential statistics to economics is that mainstream economists usually do not start by asking themselves if the ontology – real-world economies and societies – is constituted in a way that makes it possible to explain, understand or forecast our economies and societies with the kind of models and theories that mathematics and inferential statistics supply. The basic fault with modern mainstream economics, in my view, is that the concepts and models it uses – often borrowed from mathematics, physics, and statistics – are incompatible with the very objects of economic study. The analytical instruments borrowed from the natural sciences and mathematics were constructed and used for totally different issues and problems. This has fundamentally contributed to the non-correspondence between the structure of economic science and the structure of real-world economies. And I think it may also be one of the main reasons why economists so often have come up with doubtful – and sometimes harmful – oversimplifications and generalizations.

Using simplifying tractability assumptions – rational expectations, common knowledge, linearity, ergodicity, etc. – because otherwise one cannot "manipulate" the models or come up with rigorous and precise predictions and explanations, does not really exempt economists from having to justify their modelling choices. Being able to manipulate things in models cannot be enough to warrant a methodological choice. Take, for example, the discussion on

[1] https://www.focus-economics.com/blog/top-economics-finance-blogs-of-2018

rational expectations as a modelling assumption. Those who want to build macroeconomics on microfoundations usually maintain that the only robust policies are those based on rational expectations and representative actor models. As I tried to show in my book *On the use and misuse of theories and models in mainstream economics* (2016) there is really no support for this conviction at all. If microfounded macroeconomics has nothing to say about the real world and the economic problems out there, why should we care about it? The final court of appeal for economic models should not be if we – once the tractability assumptions are made – can manipulate them. As long as no convincing justification is put forward for how the inferential bridging is made, mainstream model building is little more than hand-waving.

Mathematics can be an excellent tool for constructing models. But – it should never become a goal in itself. Most mainstream economists construct mathematical theories and models for the purpose of being able to deliver purportedly rigorous deductions that may somehow be exportable to the real world. By analysing a few causal factors in their "laboratories" they hope they can perform thought experiments and observe how these factors operate on their own and without impediments or confounders. But it does not – at least not as far as I can see – work. Causes have to be set in a contextual structure to be able to operate. Instead of incorporating structures that are true to the real world, the settings made in economic models are standardly based on mathematical tractability. In the models, they often appear as unrealistic tractability assumptions, usually playing a decisive role in getting the deductive machinery to deliver precise and rigorous results. This, of course, makes exporting to the real-world problematic, since these models are thought to deliver general and far-reaching conclusions that are externally valid. But how can we be sure the lessons learned in these theories and models have external validity when based on highly specific and unrealistic assumptions? As a rule, the more specific and concrete the structures, the less generalizable the results. Admitting that we in principle can move from falsehoods in theories and models to truth in the real world does not take us very far unless a thorough explanation of the relation between theory, model and the real world is made – and to just have a deductive warrant for things happening in a mathematical model is no guarantee for them being preserved when applied to the real world (see for example, Freedman, 2010).

In my view, what is wrong with mainstream economics is not that it employs models. What is wrong is that it employs poor models. They – and the mathematical-logical tractability assumptions on which they to a large extent build – are poor because they do not bridge to the real world in which we live.

Now, in mathematics, the deductive-axiomatic method has worked just fine. But science is not mathematics. As far as I can see, conflating those two domains of knowledge has been one of the most fundamental mistakes made in modern economics. It has made economics both narrow and hopelessly irrelevant.

Let me just round off this already far too long answer to your question with some remarks more specifically on the use of inferential statistics in economics.

As a critical realist, I must confess to not being surprised to find that an approach that – like econometrics – presupposes a closed system, fails when it is applied to essentially open systems such as real-world economies. Although the mathematical-statistical theory upon which it builds presupposes the existence of stable parametric relations, the identified relations are almost always unstable. Simply assuming that you can model social and

economic relations with mathematical-statistical functions, is not a recipe for being able to contribute explanations of real-world phenomena and structures.

Limiting model assumptions in science always have to be closely examined since if we are going to be able to show that the mechanisms or causes that we isolate and handle in our models are stable in the sense that they do not change when we export them to our "target systems", we have to be able to show that they do not only hold under *ceteris paribus* conditions and so are of only limited value to our understanding of the real world. Now, I would maintain, the kinds of laws and relations that econometrics has established, are laws and relations about entities in models that presuppose causal mechanisms that are atomistic and additive. When causal mechanisms operate in the real-world they, however, only do it in ever-changing and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do so, as a rule, only because we engineered them for that purpose. Outside man-made "nomological machines" they are rare, or even non-existent. Unfortunately, that also makes most of the achievements of econometrics – as most of the contemporary endeavours of mainstream economic theoretical modelling – rather useless.

In my view, economics should be a science in the true knowledge business, and so I remain a sceptic of the pretenses and aspirations of econometrics. Its ever-higher technical sophistication in no way makes up for the lack of serious under-labouring of its deeper philosophical and methodological foundations. Economists who consider it "fruitful to believe" in the possibility of treating unique economic data as the observable results of random drawings from an imaginary sampling of an imaginary population are skating on thin ice.

Jamie: As you say, a ("long") thorough answer. However, it does create something of an issue that begs answers that are as much sociological as they are methodological. You state:

> "This, of course, makes exporting to the real-world problematic, since these models are thought to deliver general and far-reaching conclusions that are externally valid. But how can we be sure the lessons learned in these theories and models have external validity when based on highly specific and unrealistic assumptions? As a rule, the more specific and concrete the structures, the less generalizable the results. Admitting that we in principle can move from falsehoods in theories and models to truth in the real world do not take us very far unless a thorough explanation of the relation between theory, model and the real world is made…"

Your main point focuses on mismatch between mathematical and statistical forms and the reality investigated – inducing failure of adequate description and of explanation and prediction (if prediction is ever possible). Consider, a member of the public may well in the modern world be sceptical of "expertise" but this applies to all purveyors of knowledge that have influence. A great deal is made of post-truth etc. At the same time, in general it makes no sense to deny good faith (good intentions) to experts. A mainstream economist, just like you, has a sense of self, an identity. It would be unreasonable to assume they collectively do not care about the *status* of what it is they produce. Like any other field, the majority have integrity and would not be able to do what they do if they did not in some sense consider it a contribution to "knowledge". Moreover, as we are all aware, mainstream economists come in many forms, with a range of socio-political affiliations. They are not mere ideologists who all conform to the same political position in the same way, which then provides some reason to

deform or subvert the use (misuse) of theory. *And*, mainstream economists have and continue to have (criticisms not withstanding) great power and influence in the world (a valued skillset that in most countries makes them amongst the highest paid university graduates). To a member of the public it must seem weird that it is possible to state, as you do, such fundamental criticism of an entire field of study. The perplexing issue from a third party point of view is how do we reconcile good intention (or at least legitimate sense of self as a scholar), and power and influence in the world with error, failure and falsity in some primary sense; given that the primary problem is methodological, the issues seem to extend in different ways from Milton Friedman to Robert Lucas Jr, from Paul Krugman to Joseph Stiglitz. Do such observations give you pause? My question (invitation) I suppose, is how does one reconcile (explain or account for) the direction of travel of mainstream economics: the degree of commonality identified in relation to its otherwise diverse parts, the glaring problems of that commonality – as identified and stated by you and many other critics?

LPS: When politically "radical" economists like Krugman, Wren-Lewis or Stiglitz confront the critique of mainstream economics from people like me, they usually have the attitude that if the critique isn't formulated in a well-specified mathematical model it isn't worth taking seriously. To me that only shows that, despite all their radical rhetoric, these economists – just like Milton Friedman, Robert Lucas Jr or Greg Mankiw – are nothing but die-hard defenders of mainstream economics. The only economic analysis acceptable to these people is the one that takes place within the analytic-formalistic modelling strategy that makes up the core of mainstream economics. Models and theories that do not live up to the precepts of the mainstream methodological canon are considered "cheap talk". If you do not follow this particular mathematical-deductive analytical formalism you're not even considered to be doing economics.

So, even though we, as you formulate it, can identify many "diverse parts" of modern mainstream economics, the "degree of commonality identified" comes from the non-negotiable demand that the proliferation of models has to take place as a kind of axiomatic variation within the standard neoclassical model. But to me, and I guess most other heterodox economists, no matter how many thousands of "technical working papers" or models mainstream economists come up with, as long as they are just "wildly inconsistent" axiomatic variations of the same old mathematical-deductive ilk, they will not take us one single inch closer to giving us relevant and usable means to further our understanding and explanation of real economies.

The kind of "diversity" you asked me about, is perhaps even better to get a perspective on, by considering someone like Dani Rodrik, who a couple of years ago wrote a book on economics and its modelling strategies – *Economics Rules* (2015) – that attracted much attention among economists in the academic world. Just like Krugman and the other politically "radical" mainstream economists, Rodrik shares the view that there is nothing basically wrong with standard theory. As long as policymakers and economists stick to standard economic analysis everything is fine. Economics is just a method that makes us "think straight" and "reach correct answers". Similar to Krugman, Rodrik likes to present himself as a kind of pluralist anti-establishment economics iconoclast, but when it really counts, he shows what he is – a mainstream economist fanatically defending the relevance of standard economic modelling strategies. In other words – no heterodoxy where it *would really count*. In my view, this isn't pluralism. It's a methodological reductionist strait-jacket.

To me this also shows – to answer another part of your question – that the relationship between political/ideological views of economists and theory are not of the one-to-one kind. Leon Walras was a socialist. Knut Wicksell a social democrat. Paul Krugman is a political "radical". But that's not the point. A lot of my economics teachers at university in the 1970s and 1980s were Left party members, but they still preached neoclassical general equilibrium theory as if it were a gospel that had to be learned without question. To me, the political affiliations of these people were totally uninteresting. I am sure most of them had, as you put it, "good intentions" and looked upon themselves as "scholars". But I fiercely criticized them then – as I do now – not because of their political/ideological views, but because they taught irrelevant mathematical-formalist theories and models that had nothing to do with real-life. The ideology that I, as an economist focusing on science-theoretical and philosophical aspects of economics, am interested in is not of a political kind, but rather of a methodological kind. And that ideology is pervasive in economics!

Today we debate diversity a lot in economics in terms of what some call an "empirical revolution" that is said to have taken place within economics the last couple of decades (for example, Angrist and Pischke, 2010; Starr, 2014). It is often seen as a new kind of paradigm where economists in rigorous ways now try to test theories against reality, and where we can see a shift to empiricism away from philosophy with the use of imaginative empirical methods – such as natural experiments, field experiments, lab experiments, and RCTs.

I don't share that view. Why? Well, because these new methods face the same basic problem as theoretical models. They too are built on rather artificial conditions and have difficulties with external validity. If we see experiments as tests of theories or models that ultimately aspire to say something about the real "target system", then the problem of external validity is central. By this, I do not mean to say that empirical methods are so problematic that they can never be used. On the contrary. I am basically, though not without reservations, in favour of the increased use of experiments within economics. Not least as an alternative to completely barren "bridge-less" axiomatic-deductive theory models. My criticism is more about aspiration levels and what we believe we can achieve with these tools and methods in the social sciences. Making appropriate extrapolations from experiments to different settings, populations or target systems, is not easy. "It works there" is no evidence for "it will work here". Causes deduced in an experimental setting still have to show that they come with an export-warrant. The causal background assumptions made have to be justified, and without license to export, the value of "rigorous" methods and "on-average-knowledge" is despairingly small.

I find it hard to share the uncritical enthusiasm and optimism on the value of experiments and all the statistical-econometric machinery that comes with it. Although different empirical approaches have been – more or less – integrated into mainstream economics, I would argue there is still a long way to go before economics has become a truly empirical science. Sure, the "empirical turn" has made mainstream economics more diverse, but the "commonality" you referred to in your question still rules the roost. It is still mostly diversity within the mainstream methodological straightjacket!

So why do they do what they do then, these mainstream economists? And why do they still have so much "power and influence" in the world?

Most academic economists probably do what they do because that is what they have been taught to do and believe in. They have – almost exclusively nowadays – been trained to learn

how to construct and use mathematical models, and preferably without asking if these models are appropriate or not for the problems at hand. Most economists today are brought up within the same neoclassical tradition, a tradition that looks upon economics as a kind of social physics and applying the same kind of methods that are used in the natural sciences. Many of them look upon their science as "the queen of social science" also for that reason. Mathematics is conceived of as being somehow "objective" and "neutral" and hence contributing to the scientific image of economics. And most of them are happy with just continuing to play along with the inherited smorgasbord of mathematical tools and models. They usually take for granted that being scientific means that you unquestionably have to use mathematics. The fruitfulness of mathematics is taken as an article of faith, and if you want to do serious economics, you simply have to express your ideas and theories in mathematical form. Mathematising physics and the natural sciences turned out to be a success in the 18th and 19th centuries, so it just has to (it is argued) be a success to do the same with social sciences and economics.

To me, all this is, however, disheartening nonsense. Why? Because these guys have never sat down and earnestly asked themselves the one fundamental question every scientist has to ask herself: is the "thing" we study really of the sort that makes the methods used feasible? Instead, they are so eager to appear "scientific" and just take for granted – or pretend – that the preferred mathematical-formalist methods they use are appropriate. In my eyes, this is nothing but incomprehensibly and outrageously unscientific! Before any epistemological elaborations on models and theories are made, it should be imperative that this ontological question has to be posed and answered. Since modern mainstream economists almost without exception do not (dare to) do so, they happily go on doing what they have always done. But – neglected ontology returns with a vengeance! The kind of "laws" and regularities they come up with have no export license to the real world. The quest for "rigour" and "precision" in the end only turns out to be obtained at the cost of losing contact with the real world. The (pretended) scientific "rigour" evaporates when confronted with real-life. Economics has to be more than a simple intellectual exercise! Sure, you can always SAY that it is possible to learn things of significance about our world from constructing these kinds of models. But the proof of the pudding is in the eating, and I have still not seen a single convincing demonstration of HOW this is done! And for those of us who go for reliable knowledge and want to use economics for understanding and explaining things in the world in which we live, that means these "as if" model results are nothing but a useless and irrelevant waste of time! The (in my view perverted) allegiance to "rigour" and to a focus of scientific endeavour on proving things in mathematical-formal models is a gross misapprehension of what economics is or ought to be. The total lack of explanatory success of mainstream economics when it comes to things like (real-world) unemployment, structural change, financialization, and economic crises is a testament to the futility of building theories without solid ontological under-labouring! A relevant and realist economics should never give up on the real world and content itself with proving things about thought up worlds in "fables", "parables", "stories", "fictions", "narratives" or what have you.

On your question of "power and influence" of economists, I do actually think mainstream economics has lost much of it – at least to "members of the public" – during the last decade. Very few people outside economics departments – rightly – take the kind of analyses that mainstream economists come up with seriously. And I think it has much to do with methodology. Mainstream economists still embrace simplistic theoretical assumptions and almost religious faith in mathematical techniques where real-world applicability isn't even on the agenda. And even if they expand the smorgasbord of analytical models with new

empirical studies, that isn't enough. The crisis of 2007-08 showed more than anything else that mainstream economics with all its fancy mathematical and econometric models and theories had no answers to the questions that the UK Queen and others posed to understand what happened to our economies.

Today, when university students all over the world are increasingly beginning to question if the kind of economics they are taught – mainstream economics – is of any value, and some even question if economics really is a science at all, it is all-important to ask ourselves how we are going to proceed if we want to re-establish credence and trust in economics as a science (see, for example, Rethinking Economics & The New Weather Institute, 2017).

For a start, I think we should stop pretending that we have exact and rigorous answers on everything. Because we don't! Mainstream economists build models and theories and tell people that they can calculate and foresee the future. But they do this based on mathematical and statistical assumptions that often have little or nothing to do with reality. Then I think one should really reconsider the use of mathematics in economics since the kind of formalism that mathematics instantiates is perhaps the deepest source of the irrelevance and uselessness of modern economics. Mathematics gives exact answers to exact questions. But the relevant and interesting questions we face in the economic realm are rarely of that kind. Instead of a fundamentally misplaced reliance on abstract mathematical-deductive-axiomatic models having anything of substance to contribute to our knowledge of real economies, it would be far better if we pursued "thicker" models and relevant empirical studies and observations. Models that we already know are nothing but absurd fictions, are not the stuff that real science is made of! And – finally – we should end treating other social sciences as poor relations. We have to be more open-minded and incorporate knowledge and perspectives from other disciplines. Economics has long suffered from hubris, and a more broad-minded and multifarious science would definitely enrich today's altogether still too introverted economics.

Jamie: To follow up on some of what you say here and to make some additional sense of what I was getting at by "sociological"; if we grant that mainstream economics persists with theorems, methods, models and applications that are rooted in fictions and that mainstream economists make claims to precision or relevance that are ultimately falsely premised, this does not make them irrelevant nor does it imply they are without consequence. Future facts can be created by the (sometimes unintended) consequences of past fictions (and those fictions can be unrecognized falsities inaccurately considered true when false or more diffusely simply convenient or necessary points of departure or building blocks of model forms). Economics, it seems worth emphasising, has long been caught in the tension between representing itself as a science that describes a fundamental economic reality that can then be manipulated *and* producing a form of theory that (inadvertently) influences the nature of that economic reality (it is not fundamental in a fixed sense) *and* advocating a form or forms for that economic reality. It is perhaps because of the resistance to taking ontology seriously that the role of economics in social reality has remained so problematic for economists and for societies. Mainstream economics is not without norms or advocacy, but does tend to assimilate these on the basis of its own (implicit) ontology and methodological commitments – the role of *normativity* itself as a constituting part of social (economic) reality is rarely at issue; instead norms become predicates or hypotheses to test.

In any case, social (economic) reality is not a product of what is true, but rather (arguably) a consequence of what we do based on the complex structuring of activity that in part has

depended on the influence of mainstream economics. RCTs, for example, have had profound effects on development policy (as Martin Ravallion and others note), modern macroeconomics and financial theory have, as you note, played a role in financialization and financial crises. Here, there are various points one might make that speak to your argument regarding economics as "science". You note: "The fruitfulness of mathematics is taken as an article of faith, and if you want to do serious economics, you simply have to express your ideas and theories in mathematical form." One might elaborate that the "empirical turn" (including the claimed "credibility revolution", Angrist and Pischke, 2010) has not to any great degree reversed a general tendency in mainstream economics to *not* apply standard scientific practice. There is a significant difference between adopting methods that one thinks are social science equivalents of natural science and making use of them in the same way they are made use of in natural science (and this is an additional point to whether in fact the adoption is appropriate or the understanding of natural science they are posed within is adequate – as Phil Mirowski or Tony Lawson or Edward Fullbrook might note; for example, Fullbrook, 2016). By this I mean that mainstream economics has placed increasing emphasis on empirical work but still pays considerably less to duplication, replication or confirmation of results (a matter one should not conflate with the issue of whether in fact there are problems of philosophy of science with falsification, confirmation etc. if thinking about positivism or empiricism). The *American Economic Review,* for example, published two sections on this problem recently (both May 2017, 107(5), for example, Hoffler, 2017), albeit without this making any great difference to the direction of travel of the mainstream (a point one might also make about the turn towards an ethical code for economists that the AEA has sponsored).[2] Sociologically, surely this lack of focus on replication of results is one (not the only) way in which the status of the mainstream based on its current commitments is able to persist?

More broadly, there is a complex socialisation process that might account for economists' self- image and that might account for the nature of their influence in the world and how it persists (scepticism notwithstanding regarding expertise). Fourcade and various collaborators (for example, Fourcade et al, 2015) have done a lot of interesting work on this, as I am sure you are aware (as has, over the years, David Colander, and before his demise Fred Lee; for example, Colander, 2005; Colander and Klamer, 1987; Colander et al., 2005). This brings me to something else that perhaps you can usefully clarify, not least because it tends to confuse the status and consistency of critique, and that is use of the term neoclassical. Throughout your comments you seem to refer to neoclassical and mainstream as though the two are synonymous. There are those who consider this confusing for various reasons: the term had a particular meaning when coined by Veblen, the term was taken up by Stigler and others subsequently and became synonymous with Chicago School and associated thought and the term is often used more diffusely as a general term for mainstream economics and to imply an orthodoxy that is little changed (and concomitantly is sometimes used as a pejorative term by critics and sometimes used as a self-identification by advocates – though less today than in the past; see, for example, Arnsperger and Varoufakis, 2006; Morgan, 2016). I am sure I am not telling you anything you don't know here since you have a deep background in the history of economic thought as well as an interest in philosophy of economics. But your main purpose has always been constructively focused on argument intended as a contribution to transforming economics. With this in mind and on reflection, how do you see the term neoclassical and your use of it? You might also want to place this in the context of pluralism –

[2] The May 2017 issue of *American Economic Review* also contains an article on abduction and one might think this too is significant in terms of changing face of economics; however, the term is used for an iterative approach to multiple modeling and hypotheses – a highly limiting understanding of what abduction allows.

what scope do you see for a more pluralistic economics and how do you see current tendencies in mainstream economics (in so far as instantiated in schools of thought) as part of that pluralism?

LPS: A couple of years ago I was interviewed by a public radio journalist and we were discussing the monumental failures of the prediction-and-forecasting-business. But – the journalist asked – if these cocksure economists with their "rigorous" and "precise" mathematical-statistical-econometric models are so wrong again and again – "why do they persist wasting time on it?" Yes, indeed, why do they? Do we want to make claims about the real world in which we live, well, then we have to start using assumptions and models that we at least believe are true. Starting – as most economists do today – with assumptions that we make for mathematical tractability reasons and which no one believes are in any sense true, is a non-starter that only perpetuates endless and totally irrelevant model exercises that makes up such a big part of economics today. In my view, at least, real-world accuracy always beats model rigour and precision. We want to know *true* things about *reality*, not *consistent* things about *models*. The more I think about it, the more I wonder why any sane person should be interested in that kind of endless parade of known to be stupid models.

But maybe, as you suggest, one has to put a more sociological or psychological view on these matters. I know that people, like Marion Fourcade, have tried to explain economists' self-confidence as "intellectuals" and "scientists" from those perspectives. Economists, more than any other social scientists, concern themselves with measurable quantities, use quantitative methods and mathematics, emulate "real" sciences like physics, and so, of course, have to be considered much more "objective" and in possession of higher intellectual capabilities than the rest of the "riffraff" social sciences. I have spent forty years within the academic economics tribe, and have never been able to understand or share that inflated and self-congratulatory superiority view on our discipline.

Sure, I have met a lot of both talented and intelligent economists, but sad to say, talent and intelligence are no guarantee for delivering truly *interesting* and *relevant* knowledge. They publish a lot, are invited to conferences, have highly-paid jobs and are considered to be "experts" and "authorities" on almost everything that comes their way. Why? One important reason is that economists have been successful in selling the image of themselves as knowledgeable truth tellers and "pure" scientists equipped with scientific models and theories that are "objective", "apolitical" and "non-normative", and that politicians and business leaders can use as some kind of cooking recipes or blueprints for solving all kinds of problems they may encounter.

To me, however, this is nothing but an example of economics' pretence-of-knowledge syndrome (a term used by Hayek in his Swedish Bank Prize speech and more recently popularised by Caballero, 2010). The "econ tribe" (Leijonhufvud, 1973) has become so entranced with its own deductive-axiomatic models that it has forgotten that there is an all-important difference between the rigour and precision they manage to achieve in their models and the real-world in which policymakers and politicians have to apply these models. In the "econ" models, all uncertainty can be reduced to calculable risk, all actors have rational expectations, and they always optimize. Reality, however, is different. More complex. Genuine uncertainty is everywhere, people are not "rational", and do not always optimize. Although economists think they are in possession of relevant knowledge, trying to use those models in that real-world context is not only pointless but also many times harmful and makes

a colossal muddle of things. It would indeed be better, as Keynes once said, if economists could look upon themselves as humble and competent dentists!

You did also raise a couple of terminological questions about "neoclassical" and "pluralism", so let me say a few words about that.

Looking at what has happened with what we used to call neoclassical theory, it is obvious to everyone that it in many respects has become more diverse over time. Most mainstream economists today do not characterise themselves as neoclassical, but rather self-identify as game theorists, experimental economists, behavioural economists and so forth. But many of them share the basic core of the neoclassical tradition out of which these varieties of modern economics have emerged. Usually, that is rather unproblematic since, in the context of the given discourse, we usually know what it refers to. To me, it has, however, become natural to use the term "mainstream economics" and by that I am referring to what I consider the shared core of neoclassical economics – the methodological imperative of using mathematical-deductive-axiomatic methods. You could, of course, argue that the genealogy of the term "neoclassical" – going back to Veblen–says something else. But I think somewhere we have to accept that terms and concepts live their own lives. That is not a big problem for me, as long as you make clear to your readers in what sense you yourself use those terms and concepts. Much of what you mention – the new "empirical turn" and the "credibility revolution" – has in some ways definitely broadened the scope of economics, but I still think the new approaches and sub-disciplines are pervaded with neoclassical thinking and its inherent bias towards analytical formalism. It is also for that reason, as I said before, that I think the "empirical" transformation of economics is mischaracterized. I can't really see that it constitutes a "paradigm shift". I look upon it more as an extension of the field of *application* of economics.

There has long been a need for more pluralism in economics, on that most heterodox economists agree (see for example, Lee and Cronin, 2016; Jo et al, 2018). The question, however, is what *kind* of pluralism. Here my view is that what we need is not so much more of different theories and models, but rather *methodological* pluralism. That kind of pluralism would also open-up a much needed philosophical and science-theoretical awareness.

In mathematics, the deductive-axiomatic method has worked just fine. To reiterate because this bears repeating, science is not mathematics. Conflating those two domains of knowledge has been one of the most fundamental mistakes made in economics. There is no way you can relevantly analyse economic phenomena as a purely logical relation between hypothesis and evidence. In economics, we have to argue and try to substantiate our beliefs and hypotheses with reliable evidence. Deductive inferences are purely *analytical* and it is this truth-preserving nature of deduction that makes it different from all other kinds of reasoning. But it is also its limitation, since truth in the deductive context does not refer to a real-world ontology, and it is totally non-ampliative – the output of the analysis is already given by the input.

Instead of this insistence on using mathematics and the deductive kind of inference I would rather see economics orient as more of an abductive science. Using abduction we infer something based on what would be the best explanation of data given some contextual background assumptions. We start with a body of (purported) data and search for explanations that can account for the data. Having the best explanation means that you, given the context-dependent background assumptions, have a satisfactory explanation that can

explain facts better than any other competing explanation – and so it is reasonable to consider the hypothesis to be true. Even if we do not have deductive certainty, our abductive reasoning gives us a license to consider our belief in the hypothesis as reasonable. This, of course, does not mean that we cannot be wrong. Abductions are fallible inferences. The premises do not logically entail the conclusion. But if the abductive arguments put forward are strong enough, they can be warranted and give us justified true belief, and hence, knowledge. As economists we sometimes – much like Sherlock Holmes and other detectives that use abductive reasoning – experience temporary delusion. We thought that we had reached a strong abductive conclusion by ruling out the alternatives in the set of contrasting explanations. But what we thought was true turned out to be false. That does not necessarily mean that we had no good reasons for believing what we believed. If we cannot live with that contingency and uncertainty, well, then we're in the wrong business. If it is deductive certainty you are after, rather than the ampliative and defeasible reasoning in abduction – well, then get into math or logic, not economics.

I know that mainstream economists do not want to make this methodological change, because then they would have to give up their dream of building a "rigorous" and "precise" science on a par with physics. They do not want to admit that there are severe limits to formalism. I am a pluralist and wouldn't dream of saying that we should have none of that. Like Keynes when he criticized Tinbergen, I say: let them go on with their modelling and methods. But there has to be an end to the insistence that you must work within the constraints of the mathematical-deductivist frame. The mathematisation of economics since more than seventy years now has made these economists more or less obsessed with their formal-deductive-axiomatic models. Confronted with the critique that they do not solve real problems, they often react as Saint-Exupéry's Great Geographer, who, in response to the questions posed by The Little Prince, says that he is too occupied with his scientific work to be able to say anything about reality. Sure, modern mainstream economics is in some sense "rigorous" – but if it's rigorously wrong, who cares? Method and theory pluralism shouldn't be an end in themselves, but instead of making formal logical argumentation based on deductive-axiomatic models the message, I think we are better served by economists who – like dentists – try to contribute to solving real problems. As John Maynard Keynes (allegedly) stated – "It is better to be vaguely right than precisely wrong."

Jamie: OK, there is a consistent theme here that underpins much of what you say and that anyone familiar with your work would recognize. And I expect most readers of RWER have great sympathy with the general framing and direction of travel of your comments. But, in the spirit of critique it is also worth considering whether a consistent theme has a consistent analogue. One might, for example, wonder how far *non-mainstream* economists exhibit the desirable characteristics you state (abductive reasoning, limited or contextual use of rigorous deductive-analytical methods, emphasis on evidence, commitment to realism, methodological pluralism, an open–minded approach to alternatives etc.). For example, to what degree would you characterize post-Keynesians as developing their work based on these kinds of characteristics, and, in so far as they do, in what sense has this provided more adequate accounts of real economies?

LPS: Well, I think it is fair to say that being a science-theoretical critical realist I find myself having a lot in common with several heterodox traditions. I've always been interested in studying the work of people like Veblen, Commons, Marx, Keynes, Kalecki, Åkerman, Davidson, Minsky and (on epistemological issues) Hayek. There's a common ontological

orientation in these economists' methodological stance that I appreciate. They don't conflate model and reality.

To me, especially in times when scientific relativism is expanding, it is important to keep up the claim for not reducing science to a purely discursive level. We must maintain the Enlightenment tradition of looking upon science as in the truth business. Science is made possible by the fact that there are structures that are durable and are independent of our knowledge or beliefs about them. There exists a reality beyond our theories and concepts of it (even if social reality is also in some sense concept-dependent).

The problem with modern economics – which to a large extent is nothing but a variety of positivist social science – is not that it gives the wrong answers, but rather that in a strict sense it does not give answers at all. Its explanatory models presuppose that social reality is "closed", and since social reality is fundamentally "open", models of that kind cannot explain anything about what happens in such a universe. Mainstream economics has to postulate closed conditions to make its models operational and then – totally unrealistically – impute those closed conditions to society's real structure. But – the world itself should never be conflated with the knowledge we have of it. Science can only produce meaningful, relevant and realist knowledge if it acknowledges the divide between model and reality – and then, most importantly, earnestly tries to bridge it! Ultimately this also means my critique of mainstream economics is that it doesn't take that ontological requirement seriously.

When I read post-Keynesian economists I notice most of them share that fundamental realist view, and already by doing so actually provide more adequate accounts of real economies than does mainstream economics (for example, on methodology, Dow, 1996). Following in the footsteps of Keynes, post-Keynesians try to develop an economic theory that does not portray monetary economies as if they were barter economies; that does not reduce genuine uncertainty to calculable risk; that takes finance and instability seriously; that does not treat real historical time as if it was possible to analyse with an ergodicity postulate that more or less reduces the future to a repetition of the past.[3]

Money matters. Unemployment is to a large degree involuntary. The future is non-ergodic. To me, those views are some of the hallmarks of post-Keynesian theory – a theory that gives a far more adequate account of real-world economies than formalistic-deductive-axiomatic mainstream economics.

The basic post-Keynesian pillar is the recognition and acceptance of an ontological fact – societies and economies are permeated by genuine uncertainty. But in "modern" macroeconomics – Dynamic Stochastic General Equilibrium, New Synthesis, New Classical and "New Keynesian" – the variables used in the models are treated as if drawn from a known "data-generating process" that unfolds over time. "Modern" macroeconomics obviously did not anticipate the enormity of the problems that unregulated "efficient" financial markets created. Why? Because it builds on the myth of us knowing the "data-generating process" and that we can describe the variables of our evolving economies as drawn from an urn containing stochastic probability functions with known means and variances. Some macroeconomists, however, still want to be able to use this tool (their "hammer"). So, they decide to pretend that the world looks like a nail, and pretend that uncertainty can be reduced to risk. They construct their mathematical models on that assumption. The result: financial crises and economic

[3] Note from Jamie: for a recent post-Keynesian collection on these issues see Dow et al. (2018).

havoc. The most basic lecture post-Keynesian economists – like Minsky and Davidson – have taught us is this: trying to cope with an unknown economic future in a way similar to playing at the roulette wheel, is a sure recipe for only one thing – economic disaster.

Nowadays there is a lot of discussions about Modern Monetary Theory (MMT). To me – an old student of Minsky – that is also a sign of heterodox economics contributing to developing economics in the right – realist and relevant – direction.[4] MMT rejects the traditional Phillips curve inflation-unemployment trade-off and has a less positive evaluation of traditional policy measures to reach full employment. Instead of a general increase in aggregate demand, it usually prefers more "structural" and directed demand measures with less risk of producing increased inflation. At full employment deficit spending will often be inflationary, but that is not what should decide the fiscal position of the government. The size of public debt and deficits is not – as already Abba Lerner argued with his "functional finance" theory in the 1940s – a policy objective. The size of public debt and deficits are what they are when we try to fulfil our basic economic objectives – full employment and price stability. That government can spend whatever amount of money they want is a fact. That does not mean that MMT says they *ought* to – that's something our politicians have to decide. No MMTer denies that too much of government spending can be inflationary. What is questioned is that government deficits are *necessarily* inflationary.

Take Sweden, for example. In my country, as in so many other countries, neoliberal "norm politics" invaded the economy in the 1980s and 1990s. The mantra was that it was high time for Sweden to follow in the footsteps of Thatcher and Reagan. Deregulate the economy – especially the financial markets – and make the central bank independent, so that one could concentrate economic policies on inflation-targeting rather than on low unemployment, then Sweden would prosper. Today we have a Finance Minister that still keeps on talking about how necessary it is to balance the budget. And that in a situation where the deficit is at its lowest in 40 years and still falling![5]

What MMT shows, is how harmful this penny pinching really is. The Swedish experience illustrates how a government's ability to conduct an "optimal" public debt policy may be negatively affected if public debt becomes too small. To guarantee a well-functioning secondary market in bonds it is essential that the government has access to a functioning market. If turnover and liquidity in the secondary market become too small, increased volatility and uncertainty will, in the long run, lead to an increase in borrowing costs. Ultimately there's even a risk that market makers would disappear, leaving bond market trading to be operated solely through brokered deals. As a kind of precautionary measure against this eventuality, it may be argued – especially in times of financial turmoil and crises – that it is necessary to increase government borrowing and debt to ensure – in the longer run – good borrowing preparedness and a sustained bond market.

Jamie: We've travelled some distance here and touched on a lot of subjects that we could probably discus at much greater length. For example, following themes central to post-Keynesian work on money economies, the significance of how the majority of money is created and what it is actually created through and for. That is, bank money generated from

[4] Noting, of course, that there is debate within post-Keynesian and structural Keynesian circles regarding the originality and adequacy of MMT – Paul Davidson and Thomas Palley are to different degree sceptics. For example, contrast Davidson (2017) and Wray (2015).
[5] Note from Jamie: Belfrage and Kallifatides (2018) provides an interesting contemporary analysis of some of the issues.

borrowing, principally for the production and trading of financial assets rather than as a by-product of primary productive investment (see McLeay et al, 2014; Kumhof and Zoltan, 2015). It strikes me that post-Keynesians have had far more insightful things to say regarding this, financialisation, credit cycles etc. than most others. Still, there remains some obscurity regarding what money "is", if one wanted to explore this at the most basic level of conceptualization via ontology. For example, is it appropriate to advocate a "credit theory of money" or is this more accurately phrased as a "theory of credit money"? Is it that *all* money must be credit or is it that money as we know it is typically positioned as a credit relation but need not be so? This is a question I have been thinking about recently having read Tony Lawson in debate with Geoff Ingham on this (see Lawson, 2019; Ingham, 2018). Lawson is one of the if not *the* prime mover in rehabilitating philosophy (as ontology) in and for contemporary economics (see Fullbrook, 2009; Morgan and Patomäki, 2017). He seems to be someone you have a lot of time for. As a way to sign off, how would you place your work and influences in terms of his and other methodologists?

LPS: I think it is natural for someone like me – a critical realist – to embrace post-Keynesianism. But there are, of course, different varieties of realism and different views on how to relate to the object of study. If you take Tony Lawson and, for example, Uskali Mäki, they obviously share a common interest in analysing the ontological assumptions – explicitly or implicitly – made in the modelling strategies used by economists (see for example, Lawson, 2015; Mäki, 2013, 2001). But where Mäki is mostly focused on performing a traditional, rather "detached" academic analysis, Lawson also – like myself – is more openly critical of the state of "modern" economics and wants to actively contribute to change the direction of economics.

Lawson and Mäki are both highly influential contemporary proponents of economic methodology and philosophy. Next to Nancy Cartwright and Kevin Hoover, I guess they are those contemporary methodologists I have learned most from. Although to a certain degree, probably also a question of "temperament", I find Lawson's critique of mainstream economic theories and models deeper and more convincing than Mäki's more "distanced" and less critical approach. Mäki's "detached" style probably reflects the fact that though he is trained in economics he works as a philosopher with an interest in economics, rather than as an economist (whilst Lawson remained in an economics department at Cambridge). Being an economist myself it is easier to see the relevance of Lawson's ambitious and far-reaching critique of mainstream economics than it is to value Mäki's sometimes rather arduous application of the analytic-philosophical tool-kit, typically less ambitiously aiming for mostly conceptual and terminological "clarifications."

Just to round off this interview a little, let me say some words about the future of economics.

Contrary to people like Dani Rodrik – who totally dismisses calls for methodological pluralism in economics and think that, just because the "smorgasbord" has grown with the (alleged) "empirical turn" in economics, we have had a tremendous paradigm-shift in economics – I would rather argue that this continuing insistence on using only a deductive mathematical framework for approaching economic issues is a strong sign of how limited the change in mainstream economics really has been. From a methodological point of view, the message is still "business as usual."

From my own point of view, I think it is safe to say that if economics is going to be a relevant and useful project in the future it will have to redirect its present underlying methodology and

philosophy. Although very much in favour of the quest from economics students for more pluralism and the need for more and different theories and models, I don't think that is enough. The cut has to go deeper! Economics has to get back to being – as in the 19th century – more than just an "intellectual exercise" and reorient itself into being a real-world science. It has to become aware of and accept, the limits of analysis set by ontological facts. The world is, to a large extent, a complex, open, evolving, genuinely uncertain, emergent, non-ergodic, nonhomogeneous, and organic totality. Mainstream economics has refused to earnestly reflect on what these impregnable facts do to our possibilities of making relevant models and analyses. Instead, they have contented themselves with building toy models of ideal non-existent worlds. Going on just refining that project will not constitute a real advance. To progress, economics has to totally re-evaluate the basic premises of that modelling strategy. If not, economics will remain a useless "intellectual exercise."

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Digital currency. Design principles to support a shift from bankmoney to central bank digital currency

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Abstract
Central bankers as well as monetary reformers are discussing the introduction of central-bank issued digital currency in coexistence and competition with bank deposits (bankmoney). Among the reasons for this are the gradual disappearance of cash and a far-reaching loss of monetary control. However, a general shift to digital currency (DC) cannot be taken for granted. The paper discusses the conditions and design principles that are tipping the scales in the competition between bankmoney and DC.

Relevant issues include access to and available quantities of DC, mutual convertibility of bankmoney and DC, parity of bankmoney with DC, how to deal with bank run situations, central-bank support and government warranties for bankmoney, deposit interest on DC, and the question of negative interest on DC.

JEL codes E42, E52, E58, G21

Key words digital currency, central bank digital currency, DC, CBDC, sovereign money, monetary policy, bankmoney, deposit money, cryptocurrency

Digital currency: cryptocoin or money-on-account?

People have become used to hearing about digital currencies (DC) such as Bitcoin. These currencies are based on new technology known as distributed ledger and blockchain technology and are also referred to as cryptocurrencies because of the data encryption involved. Cryptocurrencies represent a radical alternative to the current banking system, in that they bypass retail banks and defy central-bank control from the outset.[1]

Against this background, central banks are now thinking about producing their own DC. Initially, such central-bank issued DC was imagined in the technical form of cryptocurrency.[2] The new technology, however, is still in its infancy.[3] In comparison, tried and tested ways of managing account balances and payments from and to accounts are well suited for implementing DC. In the foreseeable future, central-bank issued DC is thus likely to take the form of account balances (money-on-account). In this context, “digital money” and “electronic money” are interchangeable terms.

First design studies of DC were put forward by Barrdear and Kumhof of the Bank of England, the Swedish Riksbank and the Basel Bank for International Settlements, and were also presented at an early stage by monetary reformers and other economists.[4] The number of

* The author is professor emeritus of economic sociology.
[1] See Carney, 2018.
[2] Cf. Andolfatto, 2015; Danezis and Meiklejohn, 2016; Bech and Garratt, 2017.
[3] Related problems include the high volatility of cryptocoins, which arises from being used as speculative casino tokens rather than a general means of payment. The transfer of cryptocoins is not yet sufficiently fast, and is much too energy-intensive and thus expensive. Crypto trading platforms are vulnerable to hacker attacks, and legal questions of liability and identifiability are not entirely settled.
[4] Barrdear/Kumhof (BoE) 2016 pp. 3-18, Kumhof/Noone (BoE) 2018 pp. 4-22, 35-37, Sveriges Riksbank 2017; 2018, 2018b; Ingves (Sveriges Riksbank) 2018; Dyson/ Meaning (BoE) 2018; BIS, 2015; 2018;

central banks and international monetary institutions that have expressed an interest in DC has been steadily growing.[5]

DC is conceived of as a means of payment in general use among nonbanks, similar to traditional cash. The use of central-bank digital or electronic money would thus no longer be the privilege of banks in the interbank circulation (where DC is called "reserves"). Instead, DC would basically become available to everyone. In public circulation, DC is intended to circulate in parallel and in competition with bankmoney (i.e. bank deposit money, generally referred to as sight or demand or overnight deposits). Bankmoney side by side with DC is comparable to the familiar co-existence of bankmoney with cash.

At the beginning, this may not involve immediate individual access by nonbanks to the central bank payment system. As long as there is not a comprehensive infrastructure of DC accounts, payment service providers (including banks providing such services) can manage the DC of customers in custody as a separate trust asset. DC can also be managed by using mobile apps and e-cash cards.[6] This does not exclude the future application of cryptographic technology to managing DC.

People and organisations would opt to maintain their present bankmoney account (bank giro account), or to use a DC device or digital currency account as the new alternatives, or to use bankmoney and DC in parallel, depending on the prevailing conditions and individual preferences. However, the introduction of DC in public use is bound to face a variety of possible obstacles and problems, as discussed hereafter.

Motives behind and advantages of DC

The reason for considering DC in Sweden is the provision of a modern central-bank-issued means of payment as a successor to traditional cash, which has fallen almost into disuse in the country. An unspoken worry related to the long-term decline of cash is that a central bank with no central-bank money in public circulation might seem somewhat redundant, sort of King Lackland.

Today, bankmoney has marginalised central-bank money (cash and reserves) in all advanced countries, presently at the ratio of 85–95% bankmoney to 5–15% cash. Overnight bankmoney and cash together represent the monetary aggregate M1.[7] The actual share of cash is even smaller as more than half of it is not in active circulation but held as a safety buffer or, in the case of the US dollar and the euro, used as a parallel currency abroad.[8] This is even more pronounced for the US dollar. Statistically, US cash still represents 45% of M1, but most of these dollar notes are held abroad.[9] Put differently, cash does not count for much any longer

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Bech/Garratt (BIS) 2017; Niepelt (Swiss National Bank) 2015; 2018. Pioneering inputs from monetary reformers were made by Dyson/Hodgson 2016; Wortmann 2016; Yamaguchi/Yamaguchi 2016; Huber 2017a pp. 188-190. Other economists supporting DC include Bordo/Levin, 2017; Bordo, 2018; Eichengreen, 2017; Roubini, 2018.

[5] Recent studies include IMF, 2018 and Central Bank of Iceland, 2018. Also see BIS 2019, a survey on central-bank attitudes towards DC.

[6] Cf. Kahn/Rivadeneyra/Wong 2018, Ingves, 2018, 4 [9], Sveriges Riksbank, 2018, p.15.

[7] ECB, *Economic Bulletins*, Tables 5.1.

[8] Esselink/Hernández, 2017; Krueger/Seitz, 2014, p. 7; Rogoff, 1998.

[9] https://fred.stlouisfed.org/series/M1SL/TCDSL .

and has lost its systemic importance anyway due to the primary creation of bankmoney and related fractional amounts of central-bank reserves.

Furthermore, money market fund shares (MMF) are now used as a deposit-like new money surrogate, largely based on bankmoney. MMF amount to more than two times M1 in the U.S. and a third of M1 in Europe.[10] It is unclear exactly how much of that is used as a means of payment. Furthermore, new types of securitisation help liquidate capital that would otherwise be idle. This does not create new money surrogates, but does accelerate the circulation of money in the financial economy, which has the same effect as an increase in the stock of money.

Taken together, these and other developments (such as, for example, ongoing market concentration in the banking sector) make central-bank cash and reserves an almost insignificant factor, indicating the dwindling influence of conventional monetary policy on banking and finance. If the shift towards bankmoney could be reversed, however, resulting in a growing proportion of central-bank money, the transmission lever of conventional monetary policy could be expected to become correspondingly more effective again. A working paper by Bank of England staff Dyson and Meaning emphasises the potential role of DC in restoring a higher degree of central-bank control over the existing stock of money, and thus improving the transmission of monetary policy to banking and finance.[11]

Another fundamental advantage is the complete safety of DC as it represents central-bank base money in full, not a money surrogate such as second-level bankmoney (a mere promise to pay central-bank money on demand) and third-level MMF (a promise to pay bankmoney). In contrast to DC, bankmoney is inherently unsafe and must be backed by a number of auxiliary constructions of uncertain reliability and questionable legitimacy such as, for example, deposit insurance and government bail for bankmoney.

The reason for the structural non-safety of bankmoney is its just fractional base of cash and reserves. The respective fraction amounts to 2.5–3% of the stock of bankmoney in the euro area, consisting of 1.4% vault cash, a 1% largely idle minimum reserve requirement and 0.1–0.6% excess reserves (active interbank payment reserves, depending on a bank's size). In the countries of the British Commonwealth and a number of other countries, minimum reserves no longer exist. In the U.S., there is still a formal reserve requirement of 10% minus vault cash. However, the majority of banks (the smaller ones) is unbound by reserve requirements, certain positions such as large time deposits are generally exempt from the requirement, banks are allowed to temporarily "sweep" deposits into accounts that are not subject to reserve requirements and the Fed, furthermore, pays deposit interest on reserve balances (as most central banks now do). As a result, the actual U.S. reserve requirements have "rapidly been losing relevance" and are now near the vault cash.[12]

Adding to the safety of DC, the counterparty risk in bank payments is eliminated as DC payments are made directly from the payer to the payee (without connection to the interbank circulation of reserves). DC would thus enjoy a higher level of trust and acceptance than bankmoney, in spite of central-bank support and state warranty for the latter. As far as DC is concerned, there is no longer a need to prop up banks in a crisis in order to save the nation's money and to maintain payment transactions. The safer money is, and the

[10] Huber, 2017a, p. 111.
[11] Dyson/Meaning, 2018.
[12] Bennett/Perestiani, 2002, pp. 53, 65.

more stable the monetary system, the more this will contribute to overall financial and economic stability.

In terms of comfort and costs, the handling of DC and bankmoney is the same, and also the costs of handling DC are likely to be equal to those for bankmoney. The relatively high costs of handling cash cease to exist.

Regarding the costs to the banks for funding DC, the situation is comparable to that of cash. For a long time banks managed with cash-to-bankmoney ratios of 70, 50, 30 per cent. Why would they then have problems dealing with ratios for DC of 30, 50, 70 per cent? If profit margins from bank lending and security purchases are somewhat reduced, this reflects a corresponding reduction in today's bankmoney privilege.

Another advantage, particularly for the public purse, is increased seigniorage in proportion to the stock of DC. No matter how DC enters circulation, banks will have to finance that money in full, in the same way as for cash. Even under conditions where bankmoney is still predominant, this will result in an increased amount of seigniorage.

Problems with the coexistence of DC and bankmoney

Impaired ability of banks to lend and invest?

One of the concerns that have been expressed about the introduction of DC is that with a growing share of DC "deposit-funded bank credit might be undermined"[13] and that "with too widespread a DC, it might threaten the banks' lending activity, if banks cannot use deposits for that purpose".[14] Such statements are totally missing the point. Under fractional reserve banking, deposits are not loanable funds for the banks, and banks are not financial intermediaries but creators, de- and re-activators and extinguishers of bankmoney.[15]

The proposals published so far do not intend to strip banks of the privilege to create themselves the money on which they operate in their business with nonbanks. The banks' ability to create bankmoney at their discretion will fully be maintained rather than impaired – and this is what creates problems in the first place.

A new problem that could arise, however, is a temporary shortage of eligible securities if customers demand too much bankmoney to be converted into DC in too short a time. This is analogous to a bank run, representing a run on DC rather than traditional cash. In either case, the problem continues to be the in-built lack of bank liquidity and easy-to-liquidate assets in any bankmoney regime based on fractional reserves.

Fractional reserve banking and bank runs

Not surprisingly then, the biggest concern of DC designers is mass migration from bankmoney to DC, that is, a veritable bank run. This remains a problem – not, however, a problem with DC (that is, central-bank money, representing safe sovereign money in most

[13] Niepelt, 2015.
[14] Broadbent, 2016, p. 5.
[15] Jakab/Kumhof 2018; Huber, 2017a, p.59; 2017b.

currency areas), but the fundamental problem of bankmoney based on fractional reserves. It is irksome when the bank run problem is attributed to the introduction of DC, when in actual fact it is a persistent threat inherent in bankmoney.[16]

The continued creation of bankmoney will remain a major source of instability. In substantial volumes, DC can mitigate the dynamics of monetary overshoot and proneness to crisis inherent in the bankmoney regime, but DC cannot prevent those dynamics, particularly because, in the current proposals, it is not the central bank but the banks themselves who in the first instance decide whether and how much money to create.

The long existing problem of bank runs in the bankmoney regime is normally much played down, whereas when discussing DC it is unduly exaggerated. Bank runs do not occur in a situation of business-as-usual; they arise when banks get into trouble. Sovereign money and bankmoney have coexisted for over three hundred years, at first in the form of precious metal coins coexisting with private banknotes, and subsequently in the form of central-bank cash (notes and coins) coexisting with bankmoney (deposit money). What would overall be different if this problem-ridden coexistence were to continue with DC used side by side with bankmoney?

Crises of various kinds will recur. The demand for cash and safe DC then will increase accordingly, and the banking sector will not be able, in the short run and in standard ways, to procure enough eligible securities to take up enough money to fulfil its largely empty promise to convert bankmoney into cash or DC. Such a situation would indeed be destabilising. Central banks would have little choice but to resort again to Quantitative Easing (QE). However, if DC devices and currency accounts were available, central banks could do QE in a more effective and sensible way than has hitherto been the case. (More on this below.)

The parity question and state guarantees of bankmoney

In a side-by-side constellation of DC and bankmoney, would the present 1:1 parity between bankmoney and central-bank money endure? Or might a new type of Gresham situation arise, with bankmoney in the role of the less valued means of payment while DC, that is, central-bank or sovereign money, would be valued higher?

The question of parity is intriguing, examined of late particularly by Bjerg.[17] Parity is more closely ensured if the following conditions apply:

- Bankmoney, which is in fact a private means of payment, is allowed to be denominated in the official national unit of account.
- The central bank treats both monies at par, thereby in actual fact administering their parity.
- All state bodies accept and use bankmoney at par.
- The state gives extensive guarantees for bankmoney, for example,
  - in that the central bank almost unconditionally acts as the banks' lender of last resort; within the frame of QE even as the banks' securities dealer of last resort
  - by the government recapitalising banks if need be, and

[16] As an example, see the IMF study on DC in which, however, it is conceded at the same time that DC "could even help the central bank ease liquidity pressures and thus contain bank runs" (IMF, 2018, p. 24).
[17] The problem of parity between monies from different originators, and especially parity between bankmoney and sovereign money, is discussed in great detail in Bjerg, 2017 and 2018, 6ff, 9ff, p. 18.

- by warranting bankmoney in each account up to 100–200 thousand euros.[18]

The answer to the parity question largely hinges on the degree to which central banks and governments will maintain those auxiliary constructions to stabilise the inherently unstable bankmoney regime. Equally, this will also decide on the extent to which firms and people will want to use DC in addition to or instead of bankmoney.

At a crossroads

One might assume that as soon as DC is available there will be strong migration from bankmoney to DC. However, DC may not in fact be a fast-selling proposition, and its advantages may not materialise automatically. For example, one would not expect firms and people to feel urged to switch accounts:

- under conditions of business-as-usual when there is no sense of heightened uncertainty,
- if central banks and governments maintain far-reaching state guarantees for bankmoney, and
- if banks pay some deposit interest on bankmoney, while none or less is paid on DC.

Under such conditions, it remains unclear whether a significant shift from bankmoney to DC would occur at all.

As long as there is a dominant bankmoney regime on a small base of cash and a much smaller base of excess reserves, the advantages of DC will fail to materialise. Conversely, however, if there were a critical and growing mass of DC, a greater positive impact on the stability of money, banking and finance would become apparent, thus opening up the prospect of a future sovereign currency system.

Design principles that make the difference

What then are the conditions and design principles that can be expected to support a gradual switchover from bankmoney to DC, so that central-bank money would over time again be the dominant and system-defining means of payment?

No restrictions on access to and relative quantities of DC

The first principle is to secure countrywide access to DC devices and currency accounts according to customer demand. In most proposals put forward thus far, DC is rightly intended to be a *universal* means of payment. What does the universality aspect of DC come down to, if not to the wider principle not to restrict access to and the use of DC?

In contrast, however, current approaches restrict the use of DC. In one model variant, access to DC is reserved for financial institutions.[19] In an earlier concept paper, the quantity of DC

[18] The pivotal role of state guarantees for bankmoney as a decisive system element is particularly emphasised in Wortmann, 2016; (2017a+b). Equally emphasised is cancellation of those guarantees as a precondition for establishing a sovereign money system.

was restricted to 30% of GDP.[20] In the Swedish concept, the use of e-kronas is not expressly limited, but only DC devices would be available at the beginning of the process (mobile apps, DC cards), and these are subject to the legal ceilings on cash payments in Sweden, currently at a maximum equivalent to about 250 euros (285 dollars) for each transaction.[21] This means restricting the use of DC to small retail transactions. It is even considered to waive the obligation to accept e-krona if the latter is granted the status of legal tender.[22]

The status of legal tender is self-evident if DC is introduced as a successor to central-bank cash. Why would it make sense to restrict the use of digital legal tender? *Qui bono*? Such limits and restrictions clearly contradict the claim of DC to be a universal means of payment. Should the non-financial public even be excluded from using DC, the whole project would in fact be pointless.

Merging DC and interbank reserves into one circuit

The next design principle is the merging of DC and interbank reserves into a single circuit. Thus far, the English and Swedish proposals have kept reserves and DC apart from one another. This is another arbitrary and implausible design feature. The terms "reserves" and "digital currency" do express different but overlapping functions and owners, and both refer to the same kind of central-bank money-on-account. There is no difference regarding the digital nature and the monetary quality of the central-bank money involved.

A desirable design principle is therefore to treat excess reserves like DC in general. This means

- merging the banks' excess reserves and their DC, and
- maintaining free exchange between the banks' and nonbanks' DC, thereby creating a single DC circuit, involving banks as much as nonbanks.

This does not imply a blurring of the difference between a pure DC transaction account and a bank's central-bank refinancing account, nor does it impair monetary policy.[23]

Full convertibility between bankmoney and DC

A subsequent principle is full convertibility between bankmoney and DC. Bankmoney must be freely convertible into DC, and vice versa. This poses no problem in a technical sense, as can be seen in the example of bank-mediated payments between the central-bank transaction accounts of state bodies and bank giro accounts of nonbanks.
Convertibility of bankmoney into traditional cash was, and essentially still is, a prerequisite for the acceptance of bankmoney and its parity with central-bank money.[24] This will also apply to the conversion of bankmoney into DC, particularly as both monies offer the ease of cashless payment.

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[19] Kumhof/Noone, 2018, pp.18, where three model variants are discussed: (1) access for financial institutions (FI) only, (2) economy-wide access for everyone, and (3) FIs only combined with narrow banking based on DC.
[20] Kumhof/Noone, 2018, pp.18; Barrdear/Kumhof, 2016, 3, p. 50.
[21] Sveriges Riksbank, 2017, p. 21.
[22] Sveriges Riksbank, 2018, p. 22.
[23] The IMF study on DC, too, concludes that "CBDC is unlikely to affect monetary policy transmission" (IMF, 2018, pp. 4, 25).
[24] Ingves, 2018, 2 [9].

Central bank guarantee of converting bankmoney into DC, particularly in a bank run

Convertibility of bankmoney must all the more be ensured in a bank run situation. In actual fact, warranted convertibility of bankmoney is the definite answer to the bank run problem. This is to say that in a bank run situation, central banks should stabilise banking and finance *not* by trying to stop the bank run, but by supporting the conversion of bankmoney into DC.

To this end, central banks would have to implement QE by granting special credit to banks specifically for the conversion of bankmoney into DC. In a state of financial emergency this might involve a degree of unsecured book credit entailing a heightened risk for the central banks. However, the measure itself would effectively prevent banks from going bust and would actually help forestall a bank run situation altogether. The promise by a central bank to provide the funds necessary for the conversion of bankmoney if need be would be even more convincing than the comparable government promises that effectively helped avoid larger bank runs in euro countries during the peak of the debt crisis in 2010–12.

Another generally stabilising element in this regard might be support for the emergence of payment service providers exclusively specialising in the management of DC transactions and other payment services, while not being active in other types of banking activities such as lending and investing.

Gradually reducing and ultimately removing state warranties for bankmoney

The question arises as to whether retain or withdraw state warranties for bankmoney. As long as these government guarantees are maintained, combined with basically unrestricted pro-active bankmoney creation, one cannot seriously expect the introduction of DC to eventually lead to a situation in which central-bank money would again be dominant and system-defining. Therefrom, another design principle is to reduce and finally remove the state guarantees of bankmoney. The bigger the share of DC has become, the more the state guarantees of bankmoney can be withdrawn. DC, by contrast, does not need to be guaranteed because *it is* central-bank base money or legal tender, a fully valid value-token by itself.

Gradual increase in the use of currency accounts by public bodies

Some of the payment transactions of public bodies are carried out today via transaction accounts with the central bank, and others via bank accounts. It is among the absurdities of the present bankmoney regime that state bodies demand to be paid in private bankmoney rather than in the sovereign currency of the state's central bank.

Public bodies should therefore be obliged to conduct transactions via currency accounts. However, the state's acceptance of bankmoney is a key pillar in the state's warranty of bankmoney. Should this pillar be removed too fast, with public expenditure at 35–55% of GDP depending on the country, bankmoney would be undermined in a way similar to a bank run. Nevertheless, public bodies can begin to use currency accounts in addition to bank giro accounts, steadily increasing their use of DC.

Central-bank credit to banks not the only channel for issuance of DC

The Swedish and English concepts of DC continue the practice of issuing central-bank money by way of disbursing credit to banks in reserves or cash against collateral. The Swedish model involves converting bankmoney into e-krona. This presupposes the banks to have created bankmoney as well as the central bank to sell or lend e-kronas to the banks. The English model issues DC by way of central-bank purchases of sovereign bonds from financial institutions. This presupposes the financial institutions to have acquired the larger part of these bonds with bankmoney rather than reserves. Either way, it is not the central bank but the banks themselves who in the first instance decide whether and how much money is created, while the central bank continues to accommodate the facts the banks have created beforehand.

However, DC also can and ought to be issued in a direct way. This would include measures such as helicopter money. In the eurosystem this might include revising Art. 123 (1) and (2) TFEU, also known as Lisbon Treaty. In its present form, this Article is overtly inconsistent in that its first clause prohibits direct monetary financing of government expenditure, while the second clause indirectly permits monetary financing of sovereign bonds by way of massive bond purchases on the open market.

Central bank deposit interest on DC equal to deposit interest on bankmoney

In the concept variants by Barrdear/Kumhof and Kumhof/Noone, DC is interest-bearing. In the Swedish concept, the e-krona does not yield interest. Why would DC be interest-bearing? Interest is paid on credit and debt positions, or say, on promissory items. DC, however, is *not* a promissory position. It *is* fully valid fiat money in its own right that does not need coverage by another kind of money or collateral. That is, by the way, why prior to the euro the German Bundesbank refused to pay deposit interest on bank reserves. An IMF study of DC has now found that "none of the central banks surveyed are seriously considering interest-bearing DC".[25]

Why then do some scholars argue the case for interest-bearing DC? One reason is to create a peg on which to hang negative interest (next paragraph). Another reason is "to clear the market".[26] The idea of market equilibrium, however, is empirically hard to substantiate. By contrast, it is quite obvious what deposit interest *on DC* actually can do: it complements the deposit interest *on bankmoney* that banks are likely to pay.

In a pronounced shift from bankmoney to DC, banks would certainly not fail to offer high-enough deposit interest (as was formerly paid on private banknotes) to prevent deposits from draining away. In the same way, central-bank deposit interest on DC could be set higher or lower than the banks' deposit interest on bankmoney. This would allow influencing customers' preferences for bankmoney or DC.

If deposit interest were paid on bankmoney, but none on DC, this would significantly contribute to an undesirable effect of pro-cyclical fluctuation: conversion of bankmoney into safe DC in times of heightened uncertainty, and back to interest-bearing bankmoney in times of business-as-usual. In this regard, paying deposit interest on DC can be a neutralising

[25] IMF, 2018, p. 29.
[26] Kumhof/Noone, 2018, p.8.

measure if the interest rate paid on DC is *equal* to the interest rate on bankmoney. This will create a level playing field and counteract the undesirable pro-cyclical shifting.

Ruling out "negative interest"

For some experts, DC is a vehicle for abolishing cash so as to pave the way for imposing negative interest.[27] The reasons for this are as follows. Since the 1970–80s, the growth of money, credit and debt has ongoingly been overshooting the nominal growth of GDP. This resulted in a huge overhang of financial assets and debt, harmlessly called "savings glut", in other words, over-abundant supplies of capital, producing asset inflation and a pronounced demand market that depresses interest rates. The Fed Funds rate, for example, went from a 16% peak in 1980 down to 0% in 2009–16.[28] Interest rates cannot naturally slip below that 0% "lower bound", and close to it there is no more room for conventional policy maneuver, in this case, lowering base rates in a downswing or even crisis. To some, 0% interest thus means "monetary paralysis".[29] The ready suggestion is breaking through the lower bound by imposing negative interest.[30] That is, rather than receiving deposit interest, having to pay interest; more generally speaking, making a creditor pay interest to the debtor, rather than the debtor paying interest to the creditor.

Some supporters of negative interest expressly refer to Gesell's 1916 concept of demurrage on holding cash and the "Wörgl miracle" of 1932.[31] Gesell thought in terms of a cash economy. He almost equated holding cash with hoarding it. The question inspired Keynes's notions of liquidity preference and the liquidity trap. To discourage "hoarding" money and stimulate spending for money to "make the world go round", Gesell considered a demurrage rate of 6% p.a. to be appropriate.

In Wörgl, a small town in Tyrol, the mayor resorted to issuing municipal emergency notes in 1932, after local unemployment had reached 30% due to factory shutdowns during the Great Depression. The notes were subject to demurrage, that is, an artificial loss of purchasing power, of 1% at the end of each month. The money was put into circulation in payment of public works. After initial difficulties, particularly hesitant acceptance by local businesses, the initiative was crowned by success. The local economy recovered to a degree, which was perceived as a little economic miracle.[32] After just one year, however, the Wörgl notes were suppressed at the instigation of the Austrian National Bank.

To believers, the Wörgl experience is the irrefutable proof of concept. In reality, it is likely to have been a miracle that wasn't. The decisive point was that people had skills and resources, means of production and infrastructure, but no money. They were given money, and that was what did the trick. Without the 1% demurrage, the economy would have recovered just the same, as no one in town was sitting in a "liquidity trap". The message is straightforward: rather than fiddling with interest rates and taking away people's purchasing power, simply add to their purchasing power, the more so in the midst of a crisis.

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[27] Among those who see DC as a suitable vehicle for imposing negative interest are, for example, Bordo/Levin, 2017, p. 3; Bordo, 2018, p. 3. The IMF study on DC also states that DC "would eliminate the effective lower bound on interest rate policy", even if the central banks surveyed in the study declared negative interest not to be a reason for introducing DC (IMF, 2018, pp. 4, 29).
[28] fred.stlouisfed.org/series/FEDFUNDS
[29] Rogoff, 2017.
[30] Buiter/Panigirtzoglou, 2003; Buiter, 2009; Rogoff, 2017.
[31] Buiter, 2001, p.32; Buiter/Panigirtzoglou, 2003.
[32] Broer, 2007.

Negative interest is counter-productive, because reducing mass purchasing power will reduce rather than stimulate economic activity. This applies all the more as fluctuations in the actors' liquidity preference are relevant to capital expenditure and high-end consumption, while most elements in the composition of mass consumption do not admit much delay. Adding to this, most people do not react as expected. Negative interest, rather than spurring faster or additional expenditure, is more often likely to trigger compensatory spending cuts. If money is confiscated from people, they do not hurry to spend what is left, but try to make up for what has been taken away (except under conditions of runaway inflation).

Moreover, the Gesellian demurrage approach was born from the under-consumption theory of the business cycle (lack of effective demand). This certainly had a point during the Great Depression. In the context of the 2008 banking and debt crisis, some over-indebted nation-states as well as people from the lower social strata in all the involved nations again had to suffer austerity, even though at a level much less miserable than was formerly the case. Instead of imposing austerity, which once more has proven to be counter-productive, debtors *and* creditors would have fared much better by adding mass purchasing power, for example, by way of monetary financing of so-called helicopter money or a citizens' dividend, that is, *QE for the real economy* rather than pouring hundreds of billions into *QE just for finance* – which has retained rather than resolved the underlying problems of "too much finance", that is, too much capital and debt, postponing them into the future.

In general, however, there is no situation of under-consumption today. Attempts to stimulate growth have acquired an ambivalent character, particularly in view of widespread over-consumption in ecological terms, owing to an ecologically still ill-adapted technological basis of industrial production and products. It therefore seems very strange that many green-minded people are to be found among the most fervent supporters of negative interest with the aim of stimulating growth.

What does negative interest really come down to? Is it artificial "inflation", a "fee", a "tax", or indeed "interest"? None of these apply. Inflation is an increase in the price level and thus far a general loss of purchasing power. From a money owner's point of view, paying negative interest on account balances may seem to be the same. Inflation, however, affects all actor groups and does not one-sidedly burden bank customers, while augmenting bank profits. That needs to be understood: *Negative interest on bankmoney* reduces the liabilities of banks to their customers and results in higher balances of a bank's profit account. This is tantamount to an illegal private tax on deposit money to the benefit of the banks. At the same time, the stock of available money is dysfunctionally reduced.

The same would apply if banks had to pass on their receipts of negative interest *on bankmoney* to the treasury or the central bank. Such mingling of monetary and fiscal policy is questionable. Taxing is not a central bank's business. Independently, negative interest as a money tax is questionable by itself, for it represents a kind of additional, downstream tax on everyone's available income after income tax, social security and public transfer payments. People would certainly try to evade the loss by "sweeping" liquid balances as much as possible into short-term savings and time deposits or MMF.

As regards interpreting negative interest as a surcharge on top of the fees for account management and payments, negative interest in fact adds to a bank's profit account in much the same way as the fees payable by the customers. However, there is no additional service

at all, rather a disservice. Furthermore, fees are charged for a specified service, not as a fixed-percentage deduction from an account balance.

Finally, it has often been said, negative “interest” is an unnatural, confused concept. It refers to something which does not in fact exist. For example, “real interest” is commonly defined as the actual interest rate minus the inflation rate. The result may be positive or negative. Combining two different classes of operands in this way makes sense when considering the actual-versus-nominal purchasing power of various kinds of income (earnings, interest, transfers). But it does not make the inflation rate an interest rate. An interest rate may be zero but is never negative. In a non-manipulative market environment, interest rates are always positive.

Similarly, an individual can have a greater or lesser income or no income at all, but not a negative income; rather, a positive-figure amount of debt. Breaking through the “lower bound” is possible in the world of numbers, but not in the real world. You pay interest to someone who has lent you money, but you do not agree to pay interest to someone who has borrowed from you. Similarly, it would be nice to go shopping and to have the shopkeeper pay you the purchase. Apparently, this would be turning the real world upside down.

Negative interest is a technocratic folly born from unworldly model economics, a measure that inappropriately expands and hence distorts conventional interest rate policy in a desperate attempt to regain the latter’s effectiveness which has largely been lost in the present bankmoney regime. As an instrument of monetary and economic policy, negative interest is counter-productive and unjust, even likely to be unlawful when benefitting the banks, and should thus generally be ruled out, also and particularly in connection with the introduction of DC.

Concluding remarks

When reading central bank statements on DC, one is left with an ambivalent impression, as if central bankers were running ahead of their convictions, seriously considering DC and yet being afraid of it. Sure, the envisaged coexistence of bankmoney and DC raises the questions discussed above, and in some respects watertight answers cannot yet be given. However, it is not necessary to know all answers and details in advance. As already mentioned, the modern world has been living for over 300 years with the conflicting situation that arises from the coexistence of sovereign currency (coins, notes and reserves) and bankmoney. The equally conflicting situation formed by the coexistence of DC and bankmoney will not be too different from this.

The questions discussed above, by the way, would not arise if central banks and politics had the courage to make a full transition from bankmoney to DC. The problems dealt with in this paper are in fact tied to the preferred step-by-step approach to introducing DC. Nevertheless, any step of introducing DC, in whatever variant, is a step forward, and to a certain extent offers the advantages mentioned above. By comparison, the problems inherent in the present near-complete rule of bankmoney are still much larger than those relating to DC might be.

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Economics and the shop floor: reflections of an octogenarian

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Introduction

A. The Problem

In 2000, the *Post-Autistic Economics Newsletter* began covering the resistance of French students against the "uncontrolled use of mathematics" in their discipline. The students spoke of a "need to liberate economics from its autistic obsession with formal models that have no obvious empirical reference" (*Post-Autistic Economics Newsletter*, issue 3, 27 Nov 2000). In an earlier newsletter they had elaborated,

> Too often the lectures leave no place for reflection. Out of all the approaches to economic questions that exist, generally only one is presented to us. This approach is supposed to explain everything by means of a purely axiomatic process, as if this were THE economic truth. We do not accept this dogmatism. We want a pluralism of approaches, adapted to the complexity of the objects and to the uncertainty surrounding most of the big questions in economics (unemployment, inequalities, the place of financial markets, the advantages and disadvantages of free-trade, globalization, economic development, etc.) (*Post-Autistic Economics Newsletter*, issue 2, 3 Oct 2000).

The students did not object to economics being a science; they just wanted to make it empirically relevant. This search for an empirically relevant science of economics has been a hard slog, for if the effort to show the autistic nature of orthodox nomothetic neoclassical economics has been relatively easy, the search for a praxis relevant alternative has not.

I suggest that in looking for empirical relevancy primarily through expansion into the social sciences, real-world economists have been looking in the wrong place. For empirical relevance they need to focus on the relationship between economics and the shop floor.

B. The Historical Focus

As I observed in a previous *RWER* article, "historians have a heightened sense of the specious present" (Locke, 2016, p. 50) – the duration that is perceived as present when expanded in time into the past -- because they have more knowledge of the past and can perceive it as still active in the now, the specificities depending on place and time. Not only can historians find the past in what most non-historians call the present, but the historians' consciousness of the echoes of past eras allows them to perceive the presence of the past in the past of previous eras.

Accordingly, the reflections of this octogenarian are about the presence of the past in a previous era of business studies. This is true in terms of the author's experiences, because it is not anchored in his immediate past, but in the 25 years before he retired in 1999, when he

was very actively engaged in learning about the comparative development of business education in various venues: As Senior Fulbright Fellow, Max Planck Institute for History (Goettingen) 1977; Esso Chair, European Institute for Advanced Studies in Management (Brussels), 1982-84; Senior Fulbright Fellow, Business History Unit, London School of Economics and Political Science,1985; Visiting Professor, Business History Unit, London School of Economics and Political Science, 1988-90; Visiting Professor, Meiji University, Summer 1990; Visiting Professor, Departments of Economics, Reading University (UK),1990-98; and Visiting Professor, School of Management, Queen's University Belfast, 1999-2000.

If, therefore, for empirical relevance, business economists need to concentrate on relationships with the shop floor, they will not find them here in a discussion of events after 2000, e.g., the financial crisis of 2008, mounting concerns about economics and ecology, or the challenge to Western firms of China's belt and road initiative. Rather, the emphasis, looking into the past from the year 2000, is on how the development of business studies in the 20th century came to ignore the shop floor, thereby provoking the revolt, circa 2000, of the French students, and how business studies outside the US-UK orbit (mainly in Germany) over a century of development, did not ignore it, with serious comparative repercussions particularly for manufacturing firms.

Strategic vision and the shop floor

Lawrence E. Mitchell described how on Wall Street, the triumph of Finance over Industry had already been completed by the late 1920s (Mitchell, 2007). Mike Rother points at a very significant example of how change in strategic vision affected the shop floor—when Alfred P. Sloan at General Motors announced: “We are not in the business of making cars. We are in the business of making money.” The new vision spawned a financial revolution in top corporate management, which led to the elaboration of financial reporting techniques (Rother, 2010, *Toyota Kata: Managing People for Improvement, Adaptiveness, and Superior Results*, pp. 65-69) “By the 1950s [the strategic vision and the management methods it engendered],” he continued, “had become general practice at US corporations and at companies around the world. Today [2010] it is so pervasive that it is essentially invisible. It is simply how things are done” (p. 64).

The pervasiveness of the new strategic vision even made business history respectable among historians. Alfred D. Chandler, Jr. in several books ((1962) *Strategy and Structure*; (1977) Pulitzer Prize winning *The Visible Hand: The Managerial Revolution in American Business*; and (1990) *Scale and Scope: The Dynamics of Industrial Capitalism*) celebrated the strategy of turning corporate governance into a money mill. The reputation of the Harvard Business School drew students nationally and internationally into Chandler’s seminars from whence his views and his school of business historians spread. Sloan’s new strategic vision of the 1920s not only shaped the visible hand of corporate governance, but throughout the third quarter of the last century, it also shaped the work of business historians to which H. Thomas Johnson and I belong.

In a career change, Johnson described in a prize-winning book with Robert S. Kaplan how the financialization strategy of US corporate governance isolated it from the shop floor (1987, *Relevance Lost: The Rise and Fall of Management Accounting*). He expanded his views in a 1992 book (*Relevance Regained: From Top-Down Control to Bottom-Up Empowerment*) and

then explained them further in (2000) *Profits Beyond Measure: Extraordinary Results through Attention to Work and People*.

He observed, in this last work, co-authored with Anders Bröms,

> Successful [US] managers believed they could make decisions without knowing the company's products, technologies, or customers. They had only to understand the intricacies of financial reporting. … [B]y the 1970s managers came primarily from the ranks of accountants and controllers, rather than from the ranks of engineers, designers, and marketers. [This new managerial class] moved frequently among companies without regard to the industry or markets they served. … A synergistic relationship developed between the management accounting taught in MBA programs and the practices emanating from corporate controllers' offices, imparting to management accounting a life of its own and shaping the way managers ran businesses (Johnson & Bröms, p. 57).

Johnson despised these lifeless pyramidal structures, managed by computer-oriented production control experts, imposed on shop floor work processes.

> At first the abstract information compiled and transmitted by these computer systems merely supplemented the perspectives of managers who were already familiar with concrete details of the operations they managed, no matter how complicated and confused those operations became. Such individuals, prevalent in top management ranks before 1970, had a clear sense of the difference between "the map" created by abstract computer calculations and "the territory" that people inhabited in the workplace. Increasingly after 1970, however, managers lacking in shop floor experience or in engineering training, often trained in graduate business schools, came to dominate American and European manufacturing establishments. In their hands the "map was the territory." In other words, they considered reality to be the abstract quantitative models, the management accounting reports, and the computer scheduling algorithms… (Johnson & Bröms, p. 23).

If change in corporate governance strategy so transformed the top's relationship with the shop floor, the general financialization of the US economy in the last quarter of the century carried the alienation further. Sloan's corporate headquarters viewed a business as a vehicle for maximizing "returns on investment … based on the value created by productive enterprise." The general financialization of the US global economy views businesses "as assets to be bought and sold for maximizing profits through financial strategies." (R. Ball & E. Appelbaum, 2013, "The Impact of Financialization on Management and Employment Outcomes," p. 91)

Investors are familiar with the institutions that the new strategy created to maximize profits, the proliferation of stock markets, financial investment firms, hedge funds, etc. that resulted in an extraordinary volatility in firm turnovers. R.N. Foster and S. Kaplan (2001) in *Creative Destruction, Why Companies that are Built to Last Underperform in the Market and How to Successfully Transform Them,* proclaimed that in the late twentieth century corporations lived in an era of "discontinuity." They observed that if Forbes' list of the 100 largest US corporations in 1987 is compared to Forbes' first list from 1917, only 18 of the original 100

firms appeared on the list 60 years later; 61 of the firms on the 1917 list no longer even existed. Scanning the S&P 500 for the period 1957-1998, they reported that the pace of turnover at the top was accelerating so quickly that by 2010 the average life span of an S&P listed firm would be ten years. By the year 2020, J. Edwards and K. Fisher had predicted in 1994 "no more than one third of today's major corporations" on the S&P list "will have survived in an economically important way" (*Banks, Finance, and Investment in Germany*, p. 5). However, this was not undesirable because the finance capitalists believe stock markets cull out by stock price underperforming firms from the international corporate pool.

On the other hand, institutional investors, who now dominated Wall Street, were not praised when making decisions for having acted from a knowledge of the shop floor—on the contrary. Lawrence Mitchell, in (2008) *The Speculative Economy: How Finance Triumphed Over Industry*, describes how one popular investment decision technique, the Capital and Pricing Model (CAPM) invented by pioneering finance mathematician and Noble Prize economist Harry Markowitz, ignores the shop floor.

> [T]he product of a regression analysis called *beta*, CAPM allows investors to build the kinds of potentially lower-risk, higher-return portfolios ...described by Markowitz, based solely upon a narrow range of information about the stock. The business itself matters little, if at all. All an investor needs is *beta*. No balance sheet, no profit and loss statement, no cash flow information, no management analysis of its performance and plans, no sense of corporate direction, no knowledge of what is on its research and development pipeline, no need even to know what products the corporation makes or what services it provides. Just *beta*. The stock is virtually independent of the corporation that issued it. CAPM has been adopted and is daily used by countless stock analysts and institutional money managers. Almost every American who invests in the market through mutual funds or other institutional media has invested on the basis of CAPM. (Mitchell, 275)

The Emergence of an alternative strategic vision on the sustainability of firms, economic communities, and nations, and how it affects relations with the shop floor

First, a short excursion into almost forgotten history.

The smug assurance of US financialized corporate capitalists came apart, if briefly, during the Japanese industrial challenge, which was often called a "shock" because it happened rather suddenly. The "shock" led to a wide conviction that the Japanese produced the most successful economy. Evidence to support the contention piled up quickly in the early 1980s. W. Mark Fruin in (1992) *The Japanese Enterprise System: Competitive Strategies and Cooperative Structure*, pointed out that the "[m]ost astonishing aspect of Japan's post-war economic recovery has been a surprisingly rapid penetration of the world oligarchy in such capital-intensive industries as automobiles and in related industries of steel glass and tires." (Fruin, p. 294) The automobile story could be quickly told. The number of American workers in that industry dropped from 802,500 in December 1978 to 487,700 in January 1983. The steel story was even worse. According to a survey completed by *Business Week*, 18 major steel companies recorded a combined loss in 1982 of $3.2 billion. Keitaro Hasegawa wrote in (1985) *Japanese Style Management: An Insider's Analysis of Corporate Success*, that American steel was an "industry in crisis" (p. 251). Half of the routine jobs vanished between

1977 and 1988 (from 489,000 to 260,000). To these horror stories could be added others about American failures in many other mass-production industries—transistor radios, cameras, binoculars, radios, sewing machines, color television sets, etc. as well as in glass and tire manufacturing.

The Japan threat was especially painful in machine tools, deemed an essential industry. Max Holland in (1989) *When The Machine Stopped: A Cautionary Tale from Industrial America* wrote that "overall, perhaps twenty-five percent of the industry evaporated before 1986" (p. 274). Another was the high-tech electronic industry, so vital to defense and future industrial leadership. By 1991, Herbert A. Henzler and Lothar Späth figured, Japanese manufacturers would produce 41 percent of the world's integrated circuits and 29 percent of the computer hardware, as opposed to the US's 26 percent of integrated circuits and 30 percent of the hardware. Whereas the US had provided half of the world's industrial output in 1950, by the mid-1980s the number was 21 percent as compared to Japan's 19 percent. Since the trends were all unfavorable, Japanese superiority long-term seemed assured.

The threat to industries extended to Europe. Volkswagen's export to North America halved between 1965-1975 (down because the Japanese were capturing America's small car market). During the 1980s the European Economic Community's share of exports to the US fell from ten to four percent. Germany exported more to tiny Austria (eight million inhabitants) than it did to all of East Asia. Indeed, Germany's exports to non-European countries during the decade did not grow. Europe, deprived of world market share, found itself falling back on European markets that they protected. In Germany there was a sea change from optimism to pessimism, perhaps expressed best in the title of Henzler and Späth's popular book (1993) *Sind die Deutschen noch zu retten?* (*Can the Germans still be saved?*).

This challenge to American stable industries and firms evoked the lean production and Toyota *kata* movements, which sought to promote a management that would advance the sustainability of threatened firms. James Womack, Daniel T. Jones, and Daniel Roos's (1990) *The Machine That Changed the World: The Story of Lean Production* gained perhaps the widest attention, but it was followed by numerous works, including H. Thomas Johnson's (1992) *Relevance Regained,* and his co-authored book (2000) *Profits Beyond Measure,* Mike Rother's book with John Shook (1998) *Learning to See: Value Steam Mapping and the Elimination of Mudda,* and Rother's book, *Toyota Kata.* But it was not just about books; the Japanese challenge spawned a broad movement to implement sustainability management in the work world of America and Europe.

University of California physicist Fritjof Capra, in (1982) *The Turning Point: Science, Society, and the Rising Culture*, a book H. Thomas Johnson admires and recommends, described the management organizational culture in which sustainability thrives:

> Most living systems exhibit multileveled patterns of organizations characterized by many intricate and nonlinear pathways along which signals of information and transaction propagate between all levels, ascending as well as descending. That is why I have turned the pyramid around and transformed it into a tree, a more appropriate symbol for the ecological nature of stratification in living systems. As a real tree takes its nourishment through both its roots and its leaves, so the power in a systems tree flows in both directions, with neither end dominating the other and all levels interacting in

> interdependent harmony to support the functioning of the whole (Capra, 1982, pp. 281-2).

This is essential to process management reality.

Others, realizing sustainable management emphasized human relations, not methods and techniques, pointed out that the extra firm educational centerpiece in Japan, delivered in grades K through 9, prepared people for regimes of sustainability.

Educational specialists observe that, despite changes in education brought on after World War II through Western emulation, the cultivation of group consciousness remained the focus in these grades. William K. Cummings, in (1980) *Education and Equality in Japan*, noted that Japanese teachers spend an inordinate amount of time at the beginning of the school year just establishing order in the classroom so that learning subsequently can take place. "Classroom order is developed by having students cooperate in groups that prepare contributions for the rest of the class" (p. 150).

Classes break into groups, with teachers sitting by rather unobtrusively. Bright students work with slow learners whose performance they help raise to the group pace. Teachers and administrators do not discipline individuals, by, say, sending a pupil to the office, but let the group to which the problem pupil belongs decide and administer "punishment." Assertive discipline is "antithetical" to the Japanese style of pupil management. Japanese teachers at the preschool level defer discipline authority to the group. Small work groups are held collectively responsible for homework assignments so that if a group member does not do this work the others receive demerits. Groups are assigned tasks, sometimes too difficult to do, just to see how well they can cope with them – they are stretched. (Joseph Adams (1995) "The British Disease" and the "Japanization" of British Industry: Conjuncture or Continuity in World History," Master's thesis, University of Hawaii at Manoa (p. 69).

Within the system moral education is taught by experience as well as precept. "Regardless of pupil preferences," Adams (an American who taught in Japan's system in the 1990s) stated, "they are supposed to do the work their group is assigned to do, not out of preference but out of an understanding of their 'duty' and the importance of the job." Moral education is an important aspect of group work tasks, including food service and clean-up. Cummings comments:

> This lunch routine contains several moral messages: no work, not even the dirty job of cleaning, is too low for a pupil; all should share equally in the common tasks; the maintenance of the school is everyone's responsibility. To underline these messages, on certain days each year the entire school, from the youngest student to the principal put on their dirty clothes and spend a couple of hours in a comprehensive cleaning of the school building and grounds" (p. 117).

Also see, Joseph J. Tobin, David Y. H. Yu, and Dana H. Davidson (1989) *Preschool in Three Cultures, Japan, China, and the United States*; and chapter 3 "Japanese Self-Absorption" in Locke (1996) *Collapse of The American Management Mystique*.

Since process education stresses the procedure through which results are obtained, not the results, K-9 education is ideal psychological preparation for people being incorporated into a

kata – "a routine," as Rother describes it, "or method that is practiced and used time and again… until it becomes second nature" (2010, p. 165).

People in firms involved in continuous improvement process reform, in order to sustain the vitality of a firm, cannot implement change without a clear and accurate knowledge of what is going on in firm process. That knowledge could not be provided by the management taught and practiced in US firms. But it could in the Toyota *kata*, which helps explain why people advocating its adoption always take a swipe at US management systems. Johnson did, by juxtaposing a list of phrases that pinpoint the behavioral traits suited to Big Three automobile management-driven manufacturing contrasted to those of the Toyota collaborative process production system (Johnson & Bröms, *Profits Beyond Measure*, 186-87):

Big Three, Management by Results	TPS, Management by Means
The "I" stands alone	Relationships are reality
Control the result	Nurture relationships
Follow finance-driven rules	Master life-oriented practices
Manipulate output to control costs	Provide output as needed on time
Increase speed of work	Change how work is done
Specialize and decouple processes	Enhance continuous flow
An individual is the cause: blame	Mutual interaction is the cause: reflect

In the foreword to *Toyota Kata*, Johnson wrote "Mike Rother penetrates Toyota's management methods to a depth never before reached.… [H]e offers a set of new ideas and practices that enables any organization, in any business, to do what it takes to match Toyota's performance." (vii) He added: "In my opinion, the greatest change [his book] can bring to the non-Toyota business world is to replace traditional financial-results driven thinking with an understanding that outstanding financial results and long-term organization survival follow best from continuous and robust process improvement and adaptation." (ix) Unlike the data processing procedures of finance management that blind those at the top's situational awareness of what is happening on the shop floor, the procedures of the improvement *kata* are not only based on what is happening on the shop floor, but integral to it, since they constantly monitor human behavior within, in order to reveal process shortcomings, thereby continually correcting them in order to promote sustained firm competitiveness.

In the book, Rother noted that

> Toyota's way, as it is sometimes called, is characterized less by its tools or principles than by sets of procedural sequences – thinking and behavior patterns – that when repeated over and over in daily work lead to the desired outcome. These patterns are the context within which Toyota's tools and principle are developed and function (p. 15).

Rother also affirmed that the Toyota improvement *kata*, described in part III of his book, is "a scientific approach, and thus universal in nature and applicable in many organizations and to many different situations. I have utilized it successfully many times. It works, and I have no hesitation in recommending it" (p. 227).

Johnson was a professor of accounting who retired as a professor of sustainable management. Others in the sustainable management movement were Japanologists, and

social scientists, manufacturers, members of associations, like Deming Societies, the Association for Manufacturing Excellence, founded in 1985, the Philadelphia Area Council for Excellence, the Growth Opportunity of Alliances of Greater Lawrence (Massachusetts), and a list of others that brought together businessmen, union leaders, and civic dignitaries concerned about the de-industrialization of their regions. Those directly involved in transferring lean production processes and the Toyota *kata* to client firms were mostly engineers like Mike Rother and Jeffrey K. Liker in Industrial and Operations Engineering at the University of Michigan, for to work in the implementation process meant that the reformers had to be fully conversant with shop floor environments. [Liker et al. (1999). *Remade in America: Transplanting and Transforming Japanese Management Systems*]

None/few were economists in universities and business schools trying to mine the vein of sustainable management to make their studies more empirically relevant. In particular, participants in the post-autistic/real-world movement paid scant attention to the sustainability movement, either in their writings (although the editor of the *Post-Autistic/Real-World Economics Review* has generously published my papers that touch on the subject, which makes me the exception), or in their actual work to implement sustainable shop floor processes. But such an involvement would have been unexpected.

When neoclassical economists and decision theorists reformed US business school curricula post-World War II in order to introduce a scientific paradigm into the schools, they also, perhaps because of the success of their reforms, subsequently turned business schools away from investigating Total Quality Management, thereby preventing business school students in standard MBA courses from learning about continuous improvement sustainable management. (See, on the introduction of the scientific paradigm in business schools, Khurana (2007) *From Higher Aims to Hired Hands*, 195-290; on the role of operations research, Locke (1989) *Management and Higher Education Since 1940*, pp. 1-55.)

Robert S. Kaplan, co-author with Johnson of *Relevance Lost*, supports this conclusion. After reviewing articles published in leading operations management journals and examining research and teaching in top business schools, he found that only one to two percent of the schools had "truly been affected, as of early 1991, by the Total Quality Management revolution that had been creating radical change in many U.S. and worldwide businesses." (Kaplan, "Quality in Business School Education and Research," 1) He concluded that "American business school research and teaching contributed almost nothing to the most significant development in the business world over the past half century – the quality revolution" (p. 1)

Germany: An alternative scenario

But in German-speaking central Europe (Germany, Austria, and Switzerland), people educated in economics did. In German areas those who study economic subjects major in *Volkswirtschaftslehre* (economics, eighth on the list of the ten most popular majors in 2017), *Betriebswirtschaftslehre* (business economics, first on the list, accounting for 30 percent of the students in the top ten majors), or *Wirtschafts-Ingenieurwesen* (the economics-engineering degree), with a curriculum composed half of economics and half of engineering courses studied primarily in technical universities (*technische Hochschulen*) and sixth on the list of the ten most popular majors. VWL (*Volkswirtschaftslehre*) has little to do with the shop floor, but from its origins in the late 19th century BWL (*Betriebswirtschaftslehre*) not only

covered business and commerce but manufacturing; lecturers in business economics regularly taught factory people about business administration. (See on the origins of BWL, Locke (1984) *The End of the Practical Man: Entrepreneurship and Higher Education in Germany, France, and Great Britain, 1880-1940*, chs. 5, 6, and 7.)

This connection between business economics and engineering developed decisively when BWL Professor Willi Prion established the *Wirtschafts-Ingenieur* degree in the technische Hochschule at Charlottenburg (Berlin) in 1924. From there it spread, especially after World War II, to other *technische Hochschulen* to become the popular major it is today.

I learned about the role that Germans educated in business economics and economics-engineering played in the propagation of sustainability management when visiting Germany in 1994. Before leaving the US, I asked Robert W. Hall, founding member of the Association of Manufacturing Excellence (AME), about Germans to contact. He replied that I should above all see Horst Wildemann, because he is the "repository of nearly all the coming of manufacturing excellence practice to Germany, a part of it almost from the beginning." (Letter, 25 June 1994) Wildemann, indeed, had started to learn about Japanese methods early – while attending a seminar in 1978 at the European Foundation for Management Development in Brussels – from a Japanese professor who was also an invitee to the gathering; he introduced the German professor of business economics to Just In Time, *kanban*, and other Japanese techniques.

In Germany I spoke first with Dr. Dieter Kirchner, chief executive of the German Trade Association Gesamt-Metall (an employer group). In response to my question about influential management bestsellers, Kirchner unhesitatingly pointed to his bookshelf at the German language edition of *The Machine That Changed the World*, the famous exposition of Japanese lean production in automobiles, with the comment: "This book is not just a bestseller but an eye-opener for automobile and nonautomotive executives alike." (Interview, Gesamt-Metall headquarters, Cologne, 18 July 1994) I asked the same question of all people I interviewed and received the same response. The mass circulation newspaper *Süddeutsche Zeitung* that I picked up at a newsstand in Munich was running a series on "lean production," another indicator of how much the ideas in *The Machine that Changed the World* were being popularized in Germany.

When I interviewed Wildemann, he was Professor of Business Economics with emphasis on Logistics at Munich Technical University where he taught courses primarily to engineering students on work-process innovation. In 1994 he headed a substantial group of over 100 research-consultants (30 percent with business degrees (*Betriebswirte*), 50 percent with degrees in economics-engineering (*Wirtschafts-Ingenieure*), 20 percent with engineering degrees (*Diplom-Ingenieure*), which included 35 graduate assistants. Their work was heavily oriented to mathematical modeling and computer simulations. At the time, from five to ten students earned doctorates under him yearly; about 120 of his current students and assistants were active consultants with firms; former students and assistants had also moved into consulting agencies, including German branches of American consultancies, the Boston Consulting Group and Arthur D. Little. Like all German professors, he had also published books, often with his associates as co-authors, on such subjects as strategic investment planning, creating synchronized production, the Just In Time concept, and the introduction of continuous quality improvement. Here are some of the titles in German of books he gave to me:

(1987) *Das Just-in-Time-Konzept: Produktion und Zulieferung auf Abruf*
(1987) *Strategische Investitionsplanung*
(1988) *Produktionssynchrone Beschaffung*
(1989) (ed.) *Fabrikplanung: Neue Wege—aufgezeigt von Experten aus Wissenschaft und Praxis*
(1992) *Arbeitszeitmanagement: Einführung und Bewertung flexibler Arbeits- und Betriebszeiten*
(1993) *Optimierung von Entwicklungszeiten: Just-In-Time in Forschung und Entwicklung und Konstruktion*
(1993) *Unternehmungsqualität: Einführung einer kontinuierlichen Qualitätsverbesserung*
(1994) *Qualität und Produktivität: Erfolgsfaktoren im Wettbewerb*

By 1994 Wildemann's team had already introduced Japanese production processes in 200 European (mostly German) firms over a period of 11 years, including Daimler-Benz, Grundig, Philips, and Volkswagen. At Volkswagen his group, when I caught up with him, had just spent three years teaching small-group quality control management techniques in five-day courses to over 2,500 managers (Locke, 1996, *The Collapse of the American Management Mystique*, pp. 199-201).

In the interview Wildemann confirmed that his team had worked with 200 firms on the introduction of reformed work processes, and since I knew that German laws on co-determination required employee-elected works councils to participate in the implementation of new training schemes, I anticipated that there had been resistance from them especially at Volkswagen, where IG Metall, the powerful trade union, dominated works councils. Wildemann replied to my question about reform under co-determination that in four years at Volkswagen he had worked closely with works councils and IG Metall shop stewards. The works councilors he worked with, in his words, were "very intelligent people," who fully appreciated the need to improve work processes, but also understood the impact that the changes would have on jobs numbers in the workplace and on the need to reduce work time and pay.

After noting that his group taught the new techniques to VW shop stewards at the same time that it taught them to management, he commented that the union (IG Metall) not only promoted the implementation of Just In Time and other work processes but often led management instead of following it in their adoption.

Some German work place features other than co-determination also favored over American counterparts the introduction of lean production processes of continuous improvement. Daniel Friel, in (2005) "Transferring a Lean Production Concept from Germany to the U.S." *The Academy of Management Executive*, 19:2 (May), pp. 50-58, writes about one German multinational corporation's efforts to introduce the same lean production concept in two of its factories, one in Germany, the other in the US.

> Although new organizational charts were drawn up on their facility in the United States, the actual way work was done did not change. Hence the German multinational lean production program failed to alter production or research and development times at the US facility. In stark contrast, the manner of work at the German plant facility altered dramatically, enabling the

> firm to reduce product development times from seven to three years and to cut production times by half (p. 50).

The author concluded about lean production reforms that

> at least part of the success of any lean production program depends on the institutional environment within which a firm is operating. Lean production functions best when a training system provides workers with a high level of broad-based analytical skills [as in Germany] and labor laws that engender the retention of employees and facilitate their integration into the decision-making process [as in Germany] (p. 50).

Wildemann estimated in 1994 that from 30 to 50 percent of German industry had already successfully implemented Total Quality Management, Just in Time, *kaizen*, and/or other Japanese work-process techniques. His group, moreover, did not work in isolation. The graduates in business economics and economics-engineering always worked alongside engineers like Mike Rother on process reform. He began his career in the manufacturing division of Thyssen AG, and while propagating the Toyota *kata* as an associate in the Department of Industrial and Operations Engineering at the University of Michigan, was a guest researcher at the Technical University Dortmund and a researcher at the Fraunhofer Institute in Stuttgart, implementing sustainable firm continuous improvement reforms. Now, in 2018, Rother and Professor Constantin with associates, organize conferences in which they actively participate and teach on-line *kata* courses through The KATA School, Germany. Because he and others in Wildemann's successor generation, like those working in the Lean Enterprise Institute (in Festool), continue the work, Wildemann's 1994 estimate of the successful completion of work process reform in Germany expands.

Consequence: Financialized management liquidates the "results" of the Japan "shock" in US industry

Real world economists would surely have made their own work more empirically relevant had they gotten involved in the sustainable management movement. That they did not meant that they left the finance capitalists alone to manage the fallout in America. This financialized world does not sustain firms but treats them as assets to be exchanged in order to maximize returns primarily to those involved in the deal-making, with results during the Japan "shock" that diminished firm cohesion mainly at the expense of the non-dealmakers.

Within firms the incomes of the richest have benefitted handsomely from the financialization of CEO salaries through stock options. Petra Dünhaupt in (2011) "Financialization and the rentier income share – evidence from the USA and Germany," *International Review of Applied Economics*, claimed that the introduction of stock options into American CEO pay alone is responsible for increasing their share of total incomes from two percent in 2000 to eight percent in 2007 (p. 19). Dünhaupt concludes that given the proximity of the CEO's position to capital owners rather than to workers, the stock option is closer to capital income than to wage income and should be classified with the former, i.e., with financialization rather than with earned wages.

Outside the firm, intensifying in the 1980s, the US financial experts entered into various kinds of deal making. Some in the caste guided quite successful public firms into leveraged buyout

schemes that converted them into private equity companies. Only firms with significant untapped borrowing capacity, undervalued assets, and high cash flows – "common characteristics of many, if not most, of America's largest and more prosperous corporations" (J. S. R. Shad, 1984, "The Leveraging of America," *Security Exchange Commission News*, 7 June, 6) could get involved because buyouts were financed from money borrowed on a target company's own credit line, and the huge debt incurred was paid back from the target company's own cash flow. (J. Kosman, 2009, *The Buyout of America: How Private Equity Will Cause the Next Great Credit Crisis*)

The language that managers and business school academics use in articles about restructuring, mergers, acquisitions, leveraged buyouts and the like rarely, if ever, touches on how employees are affected – other than to mention as an afterthought that rumors of these deals affect employee morale and retention rates and must be managed carefully. Nor are they concerned with the sustainability of firms, if their liquidation can be for the liquidators highly profitable.

If these deals made money for institutional investment funds that lent the money (e.g., public employees' pension plans), i.e. the deal makers, the target company shareholders (who received 50 to 100 percent premiums over the current market price of their stock), and managers, who were given golden handshakes, they severely affected the welfare of employees.

One important example of how employees suffered is the management caste's desire to break pension and benefit agreements in renegotiations to save firms. Defined benefit private pension plans, entered into during the pre-1980 era, were a big cost problem. There were 112,000 of them in the US in 1983, each guaranteeing fixed levels of income to retirees. Many defined benefit plans were not fully funded; that is, management, pressed by stockholder desire for good quarterly income statements and dividends to keep the stock price high, had made funding their employee pensions a low priority. Tough-minded managers during the 1980s sustainability crisis preferred to eliminate pension and benefit plans altogether or, failing that, to move employees into undefined contribution schemes that did not guarantee fixed incomes for retirees, or to establish individual pension savings accounts that greatly reduced company contributions and obligations.

Undefined benefit plans and individual savings accounts permitted management variously to lower the benefit amounts, to borrow from their employees' individual accounts, to pressure workers to put company stock into their personal retirement accounts, or to manipulate a plan's fund in ways that let a company appear to be more profitable than it was. In these schemes, the workers usually assumed all the risks when the companies suffered from stock and bond market declines. Financial institutions, too, preferred undefined contribution and individual savings account plans because fees for managing them were high – typically two to four percent of a worker's contribution –a significant reduction in his/her pension, although a steady stream of income for financial institutions managing accounts.

Jack Ramus, in (2004) *Pension Plans in the Corporate Cross-Hairs* calculated that

> From Reagan through [George H. W.] Bush business schools and financial crisis corporations have been terminating and undermining group pension plans by shutting down plants and moving companies, underfunding the plans, diverting funds to other corporate use when they can get away with it,

> and then, when the plan is in jeopardy, with the assistance of government and the courts, funneling whatever remains into 401-K type personal savings plans. From the passage of the Employee Retirement Income Security Act (ERISA) in 1974 until 2003, more than 160,000 Defined Benefits plans have gone under in the US (p. 3).

During the same time the number of personal retirement accounts mushroomed. Very few households had such accounts in 1982, but by 1995 23 percent of households had a 401-K or an equivalent individual retirement account. That percentage reached 67 in 2004. The financial management caste, allied with friends in Congress and the Oval Office, carried through this radical transformation of private pension planning to the great detriment of employees and their local economic communities everywhere in America. (A. H. Munnell et al. (2008) *The Financial Crisis and Private Defined Pension Plans*)

Those who terminated defined pension plans became management heroes, like Richard S. Miller, CEO of Bethlehem Steel, who jettisoned the company's $3.7 billion unfunded pension obligation to its retirees. This obligation removed, venture capitalist Wilbur L. Ross, now President Trump's Secretary of Commerce, bought the firm, combined it with four other derelict steel firms, and then sold the amalgamated firm, which had cost him $400 million to buy, for $4.5 billion (M. W. Walsh, 2005, "Whoops! There Goes Another Pension Plan," *The New York Times,* 15 October).

"What bothered Mr. Conway, the union leader [at the demise of Bethlehem Steel]," NYT reporter Walsh wrote,

> was not so much Mr. Ross's inability to wring more money out of the pension system or his remarkable profit on the deal. What troubled him, he said, was that the country seemed unable to take any lessons away from the demise of the steel companies and how it affected so many working people. 'It just staggers us that America's not caught on to what's happening to it' (p. 4).

Counterfactual judgments in history are always difficult. If the Post-Autistic / Real-World economists had delved seriously into the sustainable management movement, would they have advanced towards their goal, as the Paris students desired, of making economics more empirically relevant? I think they would have, but we will never know because they did not, and this failure contributed to the denouement under the reign of US financialized management just described.

With the Germans educated in economics we do not have to engage in counterfactuals. They did become heavily involved in the firm sustainability management movement alongside engineers, with government cooperation, which has had a discernible effect on the preservation of German firms and their economic communities.

That the German outcome is quite different from the American can be demonstrated through comparative analyses of the top 20 firms in each country in 2012, ranked by revenues.

United States
1.Exxon 2. Wal-Mart 3. Chevron 4. Conoco-Philips 5. General Motors 6. General Electric 7. Berkshire-Hathaway 8. Fannie Mae 9. Ford 10. Hewlett-Packard 11. AT&T 12. Valero Energy 13. Bank of America Corp 14. McKesson 15. Verizon Communications 16. JP Morgan Chase & Co 17. Apple 18. CUS Caremark 19. IBM 20. Citi Group

(Source: C. Stahl (2013). "Corporate Social Responsibility in U.S. and German Firms." Master's thesis. Graduate School of Business, University of Grenoble, 59)

Germany
1.Volkswagen 2. EON 3. Daimler 4. Siemens 5. BASF 6. BMW 7. Metro 8. Schwarz 9. Deutsche Telekom 10. Deutsche Post 11. Aldi Group 12. BP Europa SE 13. Robert Bosch 14. RWE 15. Rewe Group 16. Edeka Group 17. Audi 18. Thyssen Krupp 19. Deutsche Bahn
20. Bayer

(*Ibid*., 61)

Some firms on each list are classifiable under the same rubric, e.g., retail giants (in the US, Wal-Mart and McKesson; in Germany, the Aldi and Edeka Groups). Others are famous oil and energy firms, mostly on the US list. But there are two significant differences between the lists that are of interest here. One is that among the top 20 US firms there are many drivers of financialization (Berkshire-Hathaway, Fannie Mae, Bank of America, JP Morgan Chase Co, Citi-Group, and GE Financial), or US firms that are the creation of financialization (Hewlett-Packard: IPO 1957; Apple: IPO 1980). On the German list, there are none, i.e., not one is a financial institution, not one is a stock market IPO creation.

The second big difference gleaned from the comparison pertains to manufacturing. Few of the manufacturing firms on the US list were famous before World War II (Ford, GM, GE, which are in trouble now), but such firms dominate the list of the German top 20, many of them prominent even before World War I (Deutsche Post, Robert Bosch, Daimler, BASF, Thyssen Krupp, Bayer, and Deutsche Bahn).

The reasons for their sustainably should not be traced to inherited management methods – although fame and importance to community had much to do with it – but to their ability to work closely with sustainable management teams to reform production processes, such as Horst Wildemann's team at Volkswagen, number one on Germany's top 20 list.

Plausible conclusions

1. Whereas financialized management frustrates the implementation of sustainable management in US firms, in Germany, the reformers, including business economists and economics-engineers succor the survival of the communities in which they are embedded, thereby, after the fallout from the Japan "shock," leaving German-speaking central Europe with fewer economically blighted rust-belt communities than America.

There is, however, a caveat: East Germany. Sustainable process reform there sometimes encountered the resistance of local workers and management with mentalities inherited from the communist era that obstructed the intrusion of new management with lean production ideas into their world—as in the eastern town of Gőrlitz, where Bombardier's attempt to modernize management in its railroad car factory failed and the factory closed, resulting in unemployment and economic hardship in the community.

2. If people studying economics, finance, and business want their studies and know-how to be empirically relevant, they need to follow the example of German *Diplom-Wirtschaftsingenieure* in education and career.

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Can redistribution help build a more stable economy?

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Introduction

The ongoing recovery that started in June 2009 is about to become the longest recovery in the history of the United States. At the same time, the unemployment rate is low and there are no signs of significant inflationary pressures.

Nevertheless, seen from other angles the situation is bleak. The current recovery is the weakest in the postwar history of the US economy, and the jobs that have been created are mostly low-productivity and low-skill jobs. As we enter the second quarter of 2019, many clouds have gathered, which make a recession more probable than at any other time since the Great Recession of 2007–09.

In the present report, we analyze the main structural problems of the US economy. For better or worse, these structural characteristics – with some important exceptions – have not changed significantly over the last two-and-a-half decades. Hence, this identification allows us to analyze in a coherent way the factors that led to both the crisis and the weak and increasingly fragile recovery.

We point to four main structural problems: (1) weak net export demand; (2) fiscal conservatism; (3) increasing income inequality; and (4) financial fragility. These four problems are related to each other and can account for most of the financial woes of the US economy – but they can also explain a significant part of the otherwise perplexing political developments of the last few years.

Importantly, the situation on most of these fronts is getting worse. The economies of US trading partners are slowing down; income inequality keeps increasing (the latest step in this process was last year's tax reforms); the balance sheets of the private sector, especially nonfinancial firms, are more fragile than ever; and the stock market is clearly overvalued. These factors – and the feedback among them – will be the causes of the next recession.

For a robust and sustainable economic future, the US economy requires deep structural reforms that deal with the aforementioned problems. There is no single policy that can achieve this. Policymakers need to introduce and experiment with a wide range of measures. One such measure is an increase in the taxation of very high incomes and net worth. We simulate two variations of recent proposals that move in that direction. In the first, in accordance with a recent proposal by Senator Elizabeth Warren, there is a progressive annual wealth tax on households with high net worth (2 percent on household net worth above $50 million, with an additional 1 percent tax on net worth above $1 billion). In the second, there is a 10-percentage-point increase in the average tax rate for households belonging to the top 1 percent of the income distribution. Although the main justification for such policies is not economic, our simulations show that if these tax increases are accompanied by an equivalent increase in government outlays, they can have significant macroeconomic benefits.

The recovery so far

Output

According to the Business Cycle Dating Committee of the National Bureau of Economic Research, the current recovery, which started in June 2009, is about to become the longest since 1854 (the earliest the data allow us to pinpoint the stages of the business cycle). As of April 2019, the duration of the current expansion is 118 months, only two months shorter than the expansion of the 1990s, which lasted 120 months (March 1991 to March 2001). As Table 1 shows, this is three times the duration of the average expansion for the period 1854–2009. In the postwar period, active fiscal and monetary policy have doubled the average duration of expansions (from 28.8 months before 1945 to 58.4 months afterward). Still, the current expansion is also double the postwar average.

Table 1 Average duration of us business cycle expansions and contractions (months)

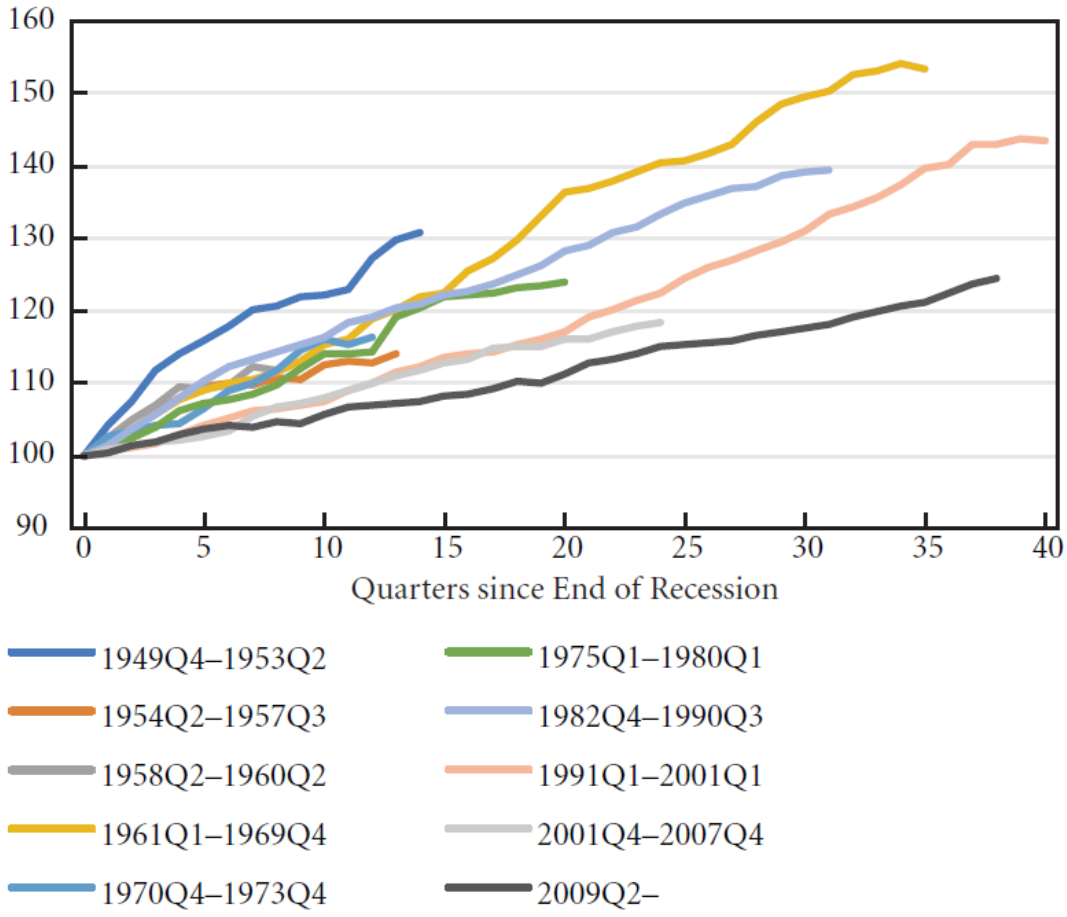
	Contraction	Expansion
1854–2009 (33 cycles)	17.5	38.7
1854–1919 (16 cycles)	21.6	26.6
1919–1945 (6 cycles)	18.2	35.0
1945–2009 (11 cycles)	11.1	58.4
Latest	18 (Dec '07 – Jun '09)	98 (Jun '09 –)

Source: NBER.

On the other hand, the duration of the latest contraction (18 months, from December 2007 to June 2009) is close to the prewar average. If we exclude the Great Recession, the average contraction of the postwar period is much shorter, around 10 months. Active fiscal and monetary policy are the main reasons behind this change as well.

As Figure 1 shows, the current expansion is at the same time the weakest over the postwar period. For example, the gains that have been recorded in the 38 quarters of the current expansion (as a percentage of real GDP at the trough of the cycle) are roughly the same as the gains recorded in the expansion of 1975Q1–1980Q1, which were achieved in just half the time (that expansion of the second half of the 1970s was considered – and actually was, as the figure shows – weak by the postwar standards of that time).

Figure 1 Index of Real GDP in US Recoveries, 1949Q4–2018Q4 (trough=100)



Sources: BEA; authors' calculations.

Labor market

As output has recovered, albeit slowly, there has been a considerable decrease in the unemployment rate. According to the latest Bureau of Labor Statistics data, the unemployment rate was 3.8 percent in March 2019, down from 10 percent in October 2009, its highest level after the crisis.

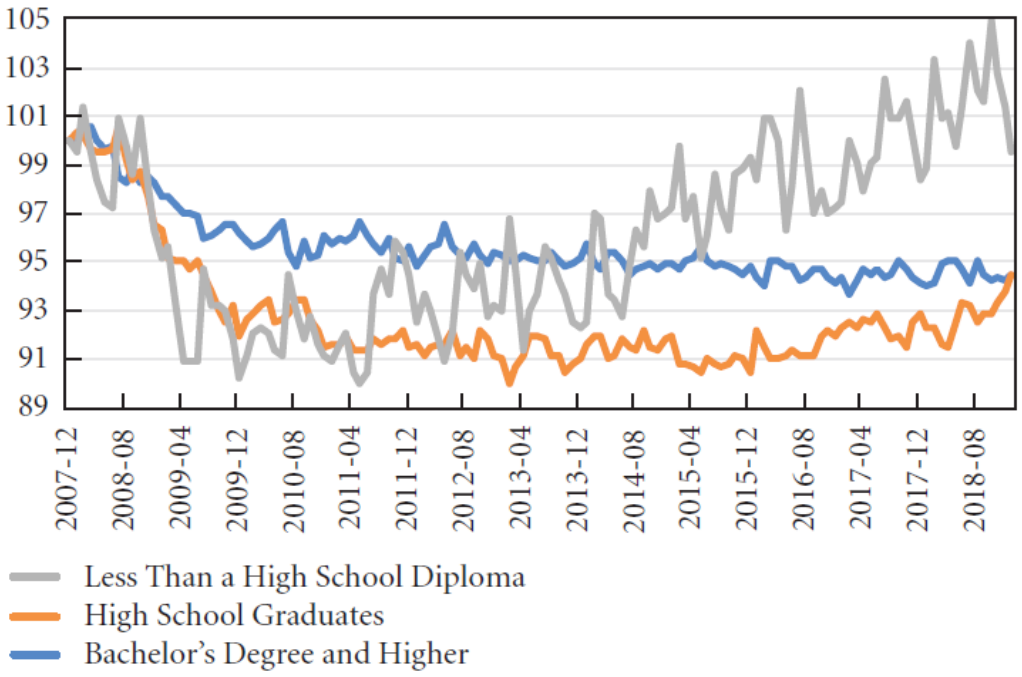
Despite the recent improvements in the labor market, the employment-to-population (E–P) ratio is less than halfway from returning to its pre-crisis levels. As Figure 2 shows, the E–P ratio was 60.7 percent in January 2019, up from 58.2 percent, which was its post-crisis low, but well below its pre-crisis peak of 63.4 percent in December 2006 and its historic peak of 64.7 percent in April 2000. These numbers show that the weak recovery of labor force participation is as much of a contributing factor to the fall in the unemployment rate as the employment gains. In other words, a significant number of employees have been discouraged and remained out of the labor force despite the recovery in output and employment.[1]

[1] For a more detailed discussion, see Papadimitriou, Hannsgen, and Nikiforos (2013), and Nikiforos (2013).

Figure 2 Civilian employment–population ratio

Source: BLS.

Figure 3 Employment-–population ratio, 25 years and older (Dec 2007=100)



Source: BLS.

A more worrisome feature of recent labor market trends, which usually passes unnoticed, is that the jobs that have been created are mostly low-productivity, low-paid jobs. As Figure 3 shows, it is only the E–P ratio of employees with less than a high school diploma that has increased significantly. When it comes to employees with a high school diploma, the increase is much smaller, while there has been no increase whatsoever in the E–P ratio of employees with a bachelor's degree and higher. Given these data, it is not surprising that, despite the low unemployment rates, there are no significant pressures on wage inflation.

The structure of the US economy

In order to understand the US economy – or for that matter any economy – we need to identify its structural characteristics. These characteristics will allow us to link its precrisis trajectory to the present relatively slow recovery and, most importantly, its future prospects. Through this prism, it is also easier to understand major policy debates and concerns regarding foreign competition, such as the recent much-discussed "trade wars".

In several previous reports we have identified four main structural problems afflicting the US economy: (1) the weak net export demand for US products; (2) the fiscal conservatism that has prevailed for most of the last three decades; (3) the increase in income inequality; and (4) the associated financial fragility.[2]

These issues are not independent of each other. An economy that faces weak net export demand from abroad tends to have high trade deficits. From the financial balances perspective, a trade deficit implies a negative balance (deficit) for the private sector, the public sector, or both. If trade deficits are accompanied by austerity, the burden of the adjustment falls on the private sector. Such an economy faces the choice between growth accompanied by trade and private deficits – essentially growth fueled by private indebtedness – or a recession that will dampen output and reduce imports, thus reducing trade and private deficits. In the former case, private deficits accumulate into higher stocks of debt and make the financial position of the private sector more fragile.

Increasing income inequality makes the situation worse because households at the bottom and middle of the income distribution have higher propensities to consume than households at the top of the distribution. Therefore, a redistribution of income toward the top, as has happened in the United States over the last four decades, has a negative effect on consumption, demand, and growth. In such a situation, for the economy to keep growing it is necessary that poor and middle-class households finance part of their consumption by borrowing. Hence, income inequality adds another layer of instability, as the balance sheets of most households become more fragile (Papadimitriou et al. 2014; Nikiforos 2016).

Finally, such a situation is facilitated by asset inflation, for an increase in asset prices increases the value of the asset side of balance sheets and masks potential vulnerabilities on the liabilities side. As a result, asset inflation contributes to an increase in both the demand for and supply of new liabilities, as both households or other agents are more willing to increase their indebtedness (e.g., loans) and the banks or other institutions are ready to accommodate them. Asset inflation can also have some direct wealth effects on private expenditure, although according to our estimates for the US economy these are relatively small. This analysis shows the connection that oftentimes exists between the two Minskyan processes of fragile balance sheets, on the one hand, and asset inflation, on the other.

The identification of these four structural characteristics of the US economy allows us to understand the factors that led to the crisis of 2007–09 as well as why the recovery that followed has been so slow. In the decades before the crisis, the growth of the US economy (in the face of increasing trade deficits and strict fiscal policies) was largely based on private indebtedness. Due to widening income inequality, the increase in indebtedness was

[2] The reader can refer to Godley (1999), Papadimitriou, Hannsgen, and Nikiforos (2013), Papadimitriou et al. (2014, 2015), Papadimitriou, Nikiforos, and Zezza (2016), and Nikiforos and Zezza (2017, 2018).

especially problematic for households at the bottom of the income distribution. This process was facilitated by the stock market inflation and the increase in real estate prices, especially after 2000. The crisis ensued when – in the face of high indebtedness – the Fed increased the interest rate and households increased their saving rates; this led to a decrease in growth rates and triggered the financial crisis, which then further reduced growth and employment.

In the period after the crisis, the slow GDP growth rate can be attributed to the same structural factors. Net export demand was weak (with the significant exception of petroleum products) and fiscal policy was constrained (until last year). Inequality also kept increasing. The major difference with the precrisis period is that the household sector has not increased its indebtedness, hence consumption has grown very slowly. Since most components of demand grew slowly (if they grew at all), it is only natural that the economy as a whole also stagnated.

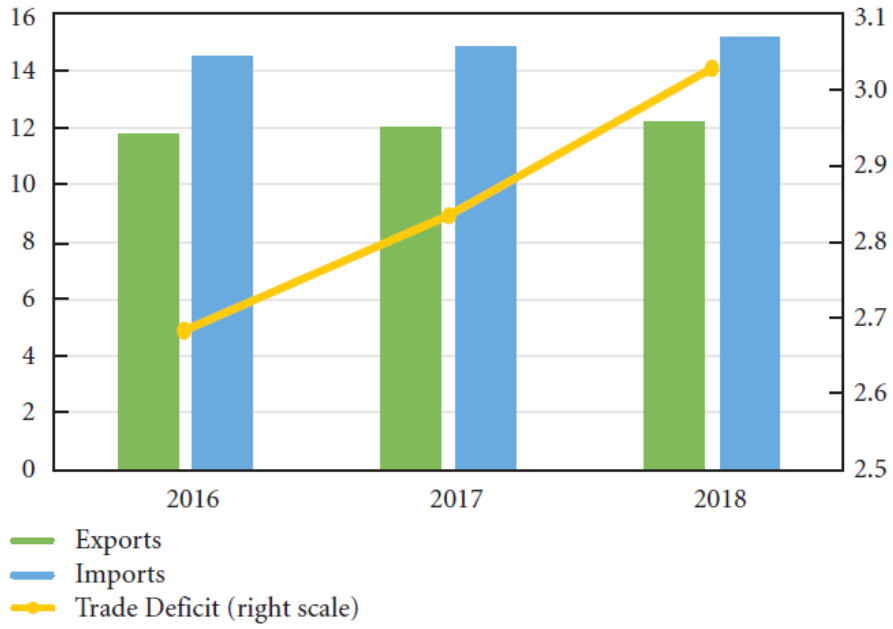
These four structural characteristics are very important for the present and the future prospects of the US economy, warranting more detailed discussion.

The foreign sector

The performance of net exports has been at the center of US policy debates over the last year because of the tariffs imposed by the United States on several imported products and the counter-tariffs introduced by some US trading partners. The last year has also seen intense trade negotiations with Canada, Mexico, and Europe, which have concluded with an agreement, and ongoing negotiations with China.

Despite the current administration's focus on trade issues, the US trade deficit has been increasing over the last two years. In 2018, the trade deficit increased – in nominal terms – to its highest level in history. Even as a percentage of GDP, the trade deficit has been increasing in the last two years. In 2018, it slightly exceeded 3 percent (Figure 4a).

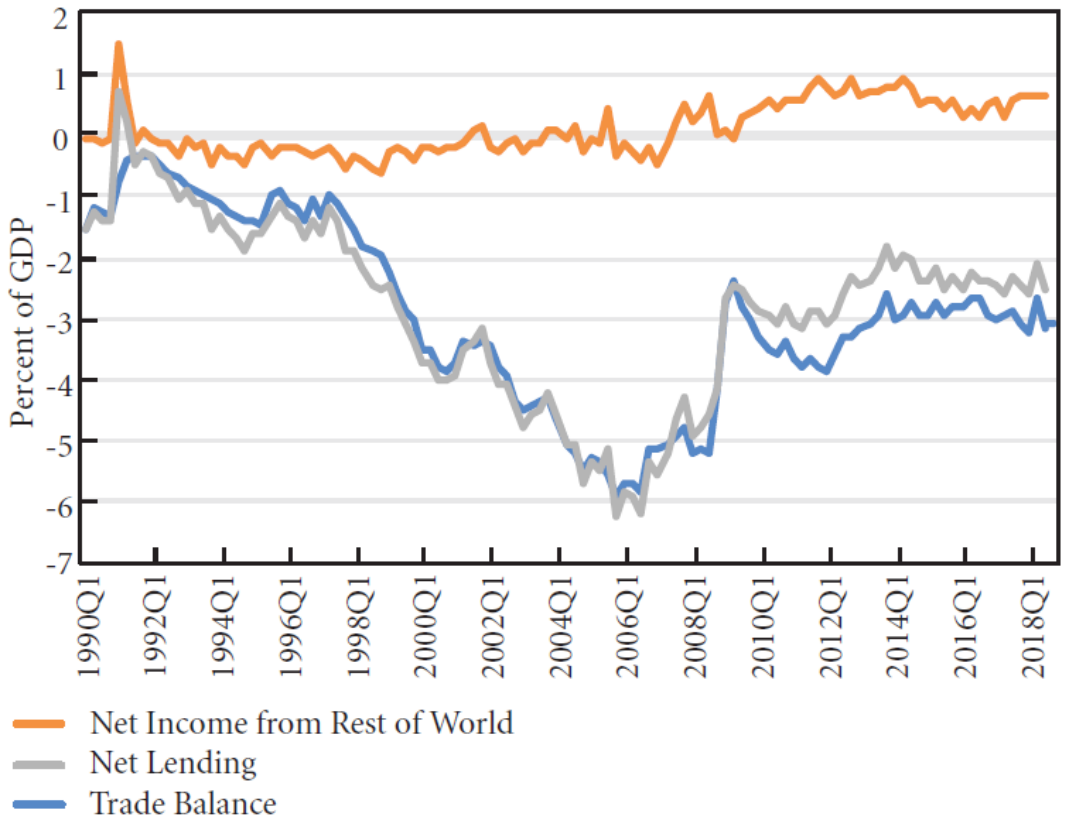
Figure 4a US international trade, 2016–18 (percent of GDP)



Sources: US Census; BEA.

To get a better idea of the foreign sector, we can decompose the current account balance into the trade balance and net income from the rest of the world. As Figure 4b shows, net income receipts have slightly increased over the last two years. This increase has counterbalanced the deterioration of the trade deficit, so the current account has been relatively stable.

Figure 4b Current account balance



Source: BEA

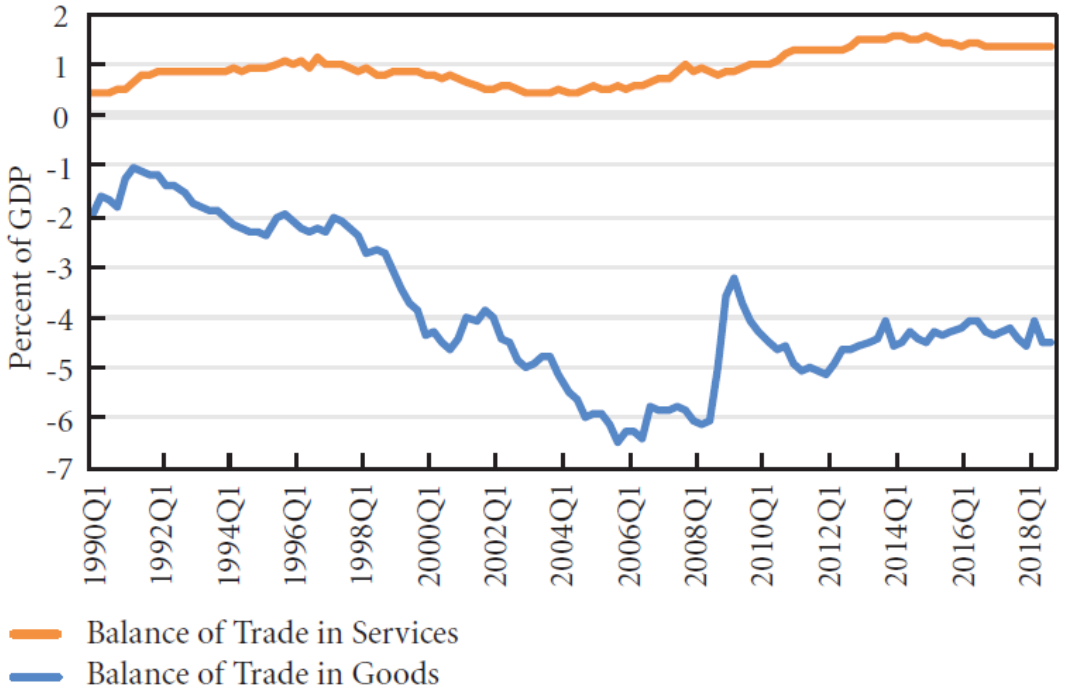
If we go one step further and decompose the trade balance into the respective balances of goods and services, we see that the trade surplus in services has been relatively stable over the last several years. Hence, the worsening of the trade position comes from the goods side (Figure 4c).

The main reason for the relative stability of the trade and current account balances is presented in Figure 4d. Since the beginning of the recovery, the trade deficit in goods *except for petroleum products* has been following its precrisis trend.[3] At the end of 2018 it reached its precrisis peak – and for that matter its historical peak – of around 4.4 percent. However, at the same time this increase has been counteracted by the improvement in the trade balance of petroleum goods, related to shale gas extraction. The trade deficit of petroleum goods is now close to zero, compared to 2.2 percent of GDP when shale gas extraction started in 2011 and 3 percent before the crisis. It is not then hard to calculate that, had it not been for this

[3] To be more precise, the trade balance of non-petroleum goods started slowly improving in 2006, more than a year before the economy officially entered the recession. This improvement had to do with two main factors: (1) the slowdown of the US economy that had started already in 2006, and (2) the significant depreciation of the dollar that started in 2002 and continued up until 2008.

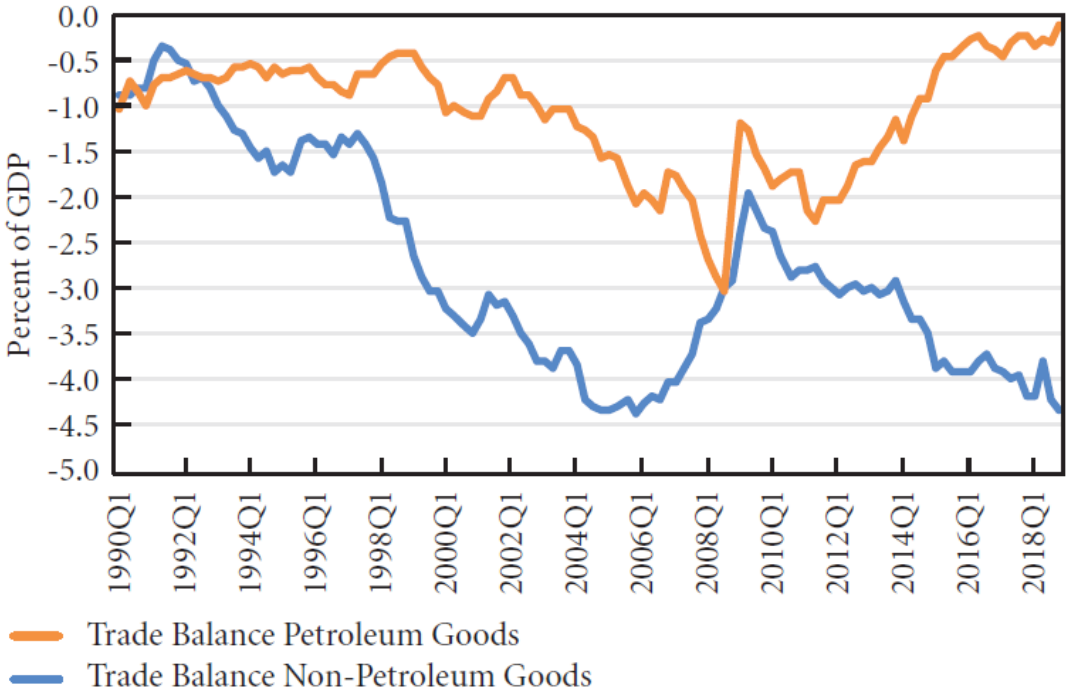
improvement in the petroleum products trade balance, the overall trade deficit of the US economy would be close to 7 percent, or more.

Figure 4c Trade balance in goods and services



Source: BEA

Figure 4d Trade balance in goods



Source: BEA

Two points are important here. First, Figure 4d shows that the underlying causes of the increase in the trade deficit remain in place. This has important macroeconomic, but also (even more significant) political, ramifications. Second, the future path of the trade balance of petroleum products is not clear. However, over the last three years, it has been stable at slightly below zero. If this stability continues and the trade deficit of non-petroleum goods keeps increasing, the overall trade and current account balances are bound to worsen.

Finally, the demand for US products is vulnerable to the weakening of the economies of the main US trading partners. The eurozone economy, as well as those of Canada, China, and, to a smaller degree, Mexico, are expected to slow down in 2019. This will have a significant effect on US exports and the more general macroeconomic performance of the US economy. Besides the direct demand effect for US products, a slowdown among US trading partners can lead to a worsening of the terms of trade (as long as weaker growth is accompanied by lower inflation), as well as to an appreciation of the dollar if there is an inflow of capital to the United States, or because of monetary policy differences.[4]

In previous reports (Papadimitriou et al. 2015; Papadimitriou, Nikiforos, and Zezza 2016), we have estimated that the combination of a slowdown in the growth rate of US trading partners by 1 percent, a decrease in their inflation rates, and dollar appreciation can lead to a decrease in the growth rate of the US economy of close to 1 percent in each year of our projection period. These dangers still remain.

Fiscal policy

One of the main reasons for the slow recovery is that the US economy has been operating under austerity for most of the time since 2011. The 2011 Budget Control Act (BCA) and its later amendments imposed caps on discretionary spending, which led to a significant reduction in government outlays. Figure 5 shows that as of the last quarter of 2018, real government expenditure was almost 4 percent below its level at the beginning of the recovery, 10 years earlier. This is a remarkable feature of the current recovery, unique among all the postwar business cycles of the US economy.

In the period from 2011 until the end of 2017, government expenditure either decreased or remained stable. The exception to this postcrisis pattern was the period from the second half of 2014 through 2015 – this is related mostly to the Affordable Care Act, whose major provisions came into force at that time.

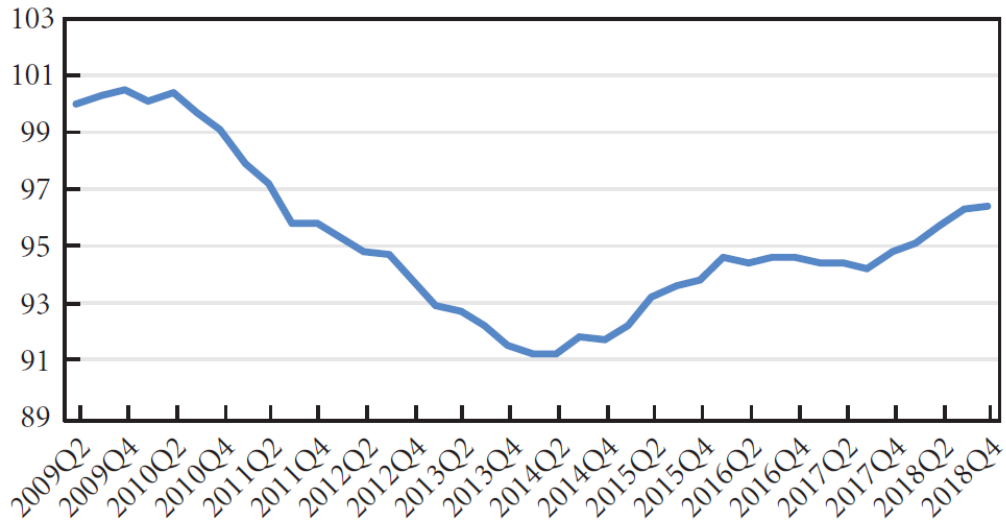
A significant change in the direction of fiscal policy took place last year. The new tax law adopted in December 2017 introduced a wide array of tax cuts, and two bills passed in 2018 – the Bipartisan Budget Act (February) and the Consolidated Appropriations Act (March) – raised the spending caps for the 2018 and 2019 fiscal years by $143 billion and $153 billion, respectively. The increase in spending during 2018 is evident in Figure 5.

In our report last year (Nikiforos and Zezza 2018), we argued that it was unlikely that the tax cuts would have significant effects because their benefits were heavily skewed in favor of high-income households and large corporations. Rich households have low marginal propensities to consume, hence the consumption effects are small. Also, for various reasons,

[4] The recent (March 7, 2019) European Central Bank announcement that it will keep its interest rate low led to a quick appreciation of the dollar.

large corporations' investment decisions have been decoupled from cash flows. Thus, the tax law supporters' main argument – that it would lead to an investment boom – seemed unlikely. It was more likely, we argued, that the increase in cash flows would lead to higher dividends or more stock buybacks.

Figure 5 Index of government consumption expenditures and gross investment (2009Q2=100)



Source: BEA.

A year later, these predictions are confirmed by the evidence. According to various sources, the corporate tax cuts had an insignificant effect on investment. For example, a recent *Business Conditions Survey* of the National Association for Business Economics finds exactly that (NABE 2018). The *Financial Times* summarized the evidence of the first 10 months of tax cuts in an article with the following title: "US Tax Cut Said to Have Little Impact on Investment: Survey Adds to the Evidence That Much of the Windfall Was Used for Share Buybacks" (Edgecliffe-Johnson and Crooks 2018).

For these reasons, the tax law has been very ineffective. By comparison, a similar increase in the government deficit could finance very ambitious projects that would make a big difference. For example, a large infrastructure plan with a similar ex ante budget impact would not only have a significant demand effect, but also important externalities in terms of productivity increases and improvements in the living standards of the average US citizen (who today has to endure the inconvenience of the country's decaying infrastructure).

As a side note, it is important to mention that the tax changes move the United States toward a territorial tax system that creates incentives for corporations to produce and be taxed in other countries. This is contradictory to the administration's effort to reduce trade deficits. By contrast, the productivity externalities of a large infrastructure plan would help the United States regain some of the ground lost to its main trade rivals.

As opposed to the tax cuts, the increase in the spending caps was expected to have more significant demand and growth effects. In last year's report, we projected that the growth rate of the US economy would increase to 3.1 percent in 2018. The latest Bureau of Economic Analysis estimate is 2.9 percent, which is close enough.

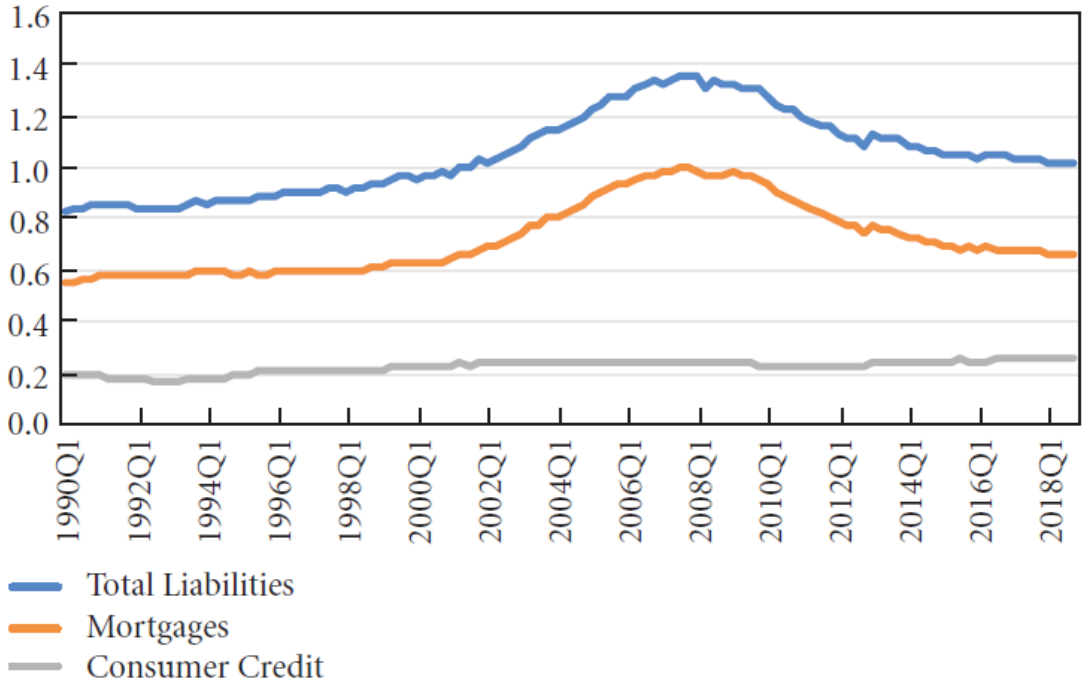
The immediate GDP growth response to last year's fiscal boost confirms the criticism of the austerity of the last decade – and the argument that so-called "secular stagnation" comes to a large extent from the demand side, as we have repeatedly argued over the last several years. Also, the fact that there was no discernible effect on wage and price inflation shows that the US economy is far from full employment, despite the low unemployment rates.

Looking into the future, the important question related to fiscal policy is whether there is going to be a return to austerity or not. For the moment, last year's agreement provides only for a small increase in fiscal expenditure this year, so the benefits will be lower. Given the political deadlock in Washington that became evident with the recent government shutdown, an agreement for further fiscal expansion seems unlikely. If anything, many lawmakers, after voting for last year's tax cuts, have expressed worries about the high fiscal deficit and proposed a revision of social provision programs – such as Social Security, Medicare, and Medicaid – to deal with it. Such a scenario, if it ever managed to make its way through the split Congress, would exert a dual negative effect: through fiscal consolidation and a further worsening of income inequality.

Financial conditions

In the aftermath of the crisis, according to the Federal Reserve's Financial Accounts, the household sector has consolidated its balance sheets. The overall ratio of liabilities to disposable income has decreased from 130 percent on the eve of the crisis to slightly above 100 percent at the end of 2018 (Figure 6). Nevertheless, this ratio is still elevated by historical standards.

Figure 6 Ratio of household liabilities to disposable income

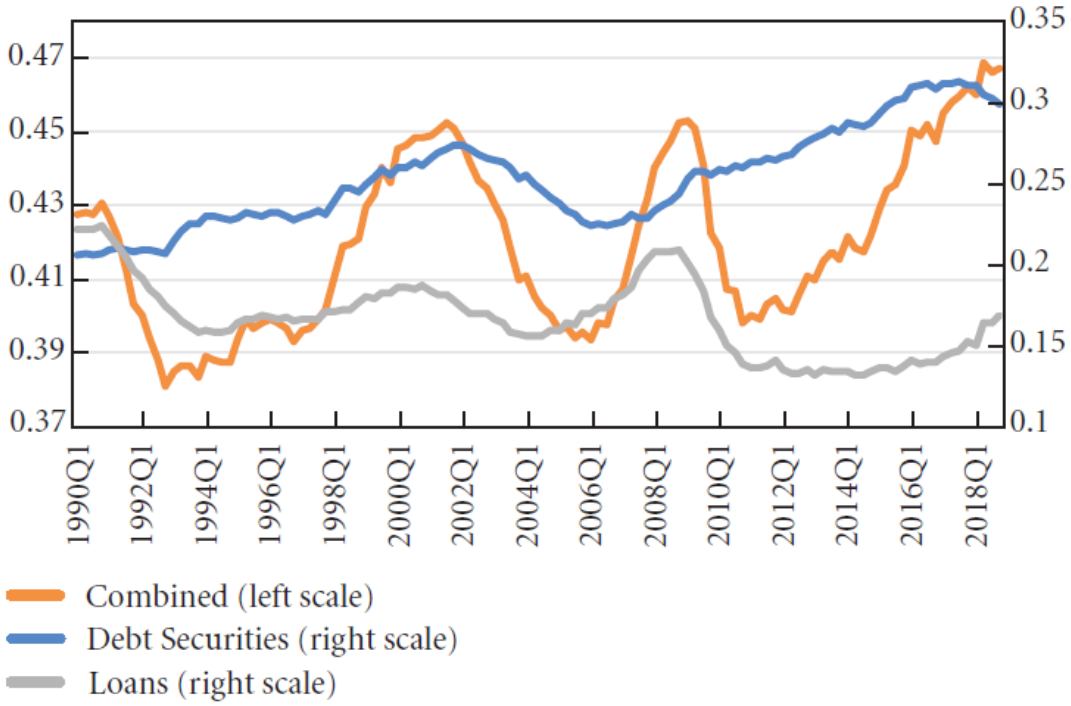


Source: Federal Reserve.

The adjustment is mostly due to the decrease in household mortgages. At the same time, consumer credit has slightly increased as a share of disposable income. In an economy whose growth before the crisis was based to a large extent on the increase in household indebtedness, this continuous deleveraging of the household sector is a major drag on aggregate demand, and one of the main reasons for the sluggish recovery.

The situation is different in the corporate sector. As Figure 7 shows, the debt liabilities (and the sum of debt and loans) of nonfinancial corporations are at an all-time high. The important related question is: To what extent are these liabilities mirrored by high-quality, liquid assets on the other side of the balance sheets? For example, it is well-known that the largest corporate debt issuer is Apple. However, Apple at the same time has a lot of liquidity and its balance sheet should be robust (Apple has a AA+ rating).

Figure 7 Ratio of nonfinancial corporate sector liabilities to GDP



Source: Federal Reserve.

Several indicators show that a significant number of firms find themselves in a situation very different from that of Apple. To begin with, data from various sources show that the share of so-called zombie firms has increased. Various studies use different definitions of zombie firms, but they are essentially a variation of what Hyman Minksy (1992) called Ponzi firms: firms whose profits are not sufficient to cover the interest payments on their debt. The increase in the number of zombie firms has taken place despite the very low interest rates of late.[5] Other data show that the share of corporate bond issuers with a BBB investment

[5]. The term "zombie firms" was coined by Caballero, Hoshi, and Kashyap (2008) in a paper on Japan, without any reference to Minsky. Recent related studies for the United States and other economies include BIS (2017), Banerjee and Hofmann (2018), Adalet McGowan, Andrews, and Millot (2018), and Acharya et al. (2018). Caballero, Hoshi, and Kashyap (2008) and Acharya et al. (2018) define a zombie firm as a firm that received subsidized credit. Banerjee and Hofmann (2018) and Adalet McGowan, Andrews, and Millot (2018) identify zombie firms as firms that are at least 10 years old and have had an interest coverage ratio of less than one for at least three consecutive years. Banerjee and Hofmann

grading – the lowest grading above junk status – has risen in recent years. As of 2018, the share of BBB issuers was around one-third (BIS 2019). Meanwhile, the share of the market capitalization with a credit rating above BBB has fallen to 50 percent, below its level in the late 1990s and before the crisis.[6]

The increase in interest rates over the last two years – albeit small – poses some challenges for the firms that already have overstretched balance sheets, and they will need to refinance their debt at a higher interest rate. Relatedly, another source of potential instability is the vulnerability of these corporations to even small decreases in earnings stemming from a possible macroeconomic shock. Right now, most analysts expect that there will be a decrease in earnings in the upcoming quarters. Moreover, given that so many bonds are just above junk status, a potential downgrade of a significant number of firms with these kinds of bonds can trigger a fire sale whose consequences could ripple through the financial markets and the real economy.

It is also important to note that the picture that emerges when looking at the balance sheets of firms is significantly bleaker than the one portrayed by the aggregated data of the Federal Reserve represented in Figure 7, which show a relatively mild increase in corporate liabilities. Finally, the fragility of corporate sector balance sheets is accompanied by an obvious overvaluation of the stock market. As we can see in Figure 8a, despite the correction in the last quarter of 2018, the ratio of market capitalization to GDP (or to total profits) is still at historically high levels. Similarly, Figure 8b shows that the cyclically adjusted price-to-earnings ratio is still way above its historical average – it is higher than the level it reached in the early fall of 1929 and surpassed only by the level of the late 1990s.

The stock market overvaluation is another potential source of instability, especially in conjunction with the fragility of private sector balance sheets. According to simulations that we have presented in previous reports (Nikiforos and Zezza 2017; 2018), a stock market correction accompanied by a deleveraging of the private sector could have very severe consequences for the economy. For example, in last year's report we estimated that, under relatively conservative assumptions, such a scenario would lead to a cumulative loss of 7 percentage points of real GDP compared to the baseline scenario.[7]

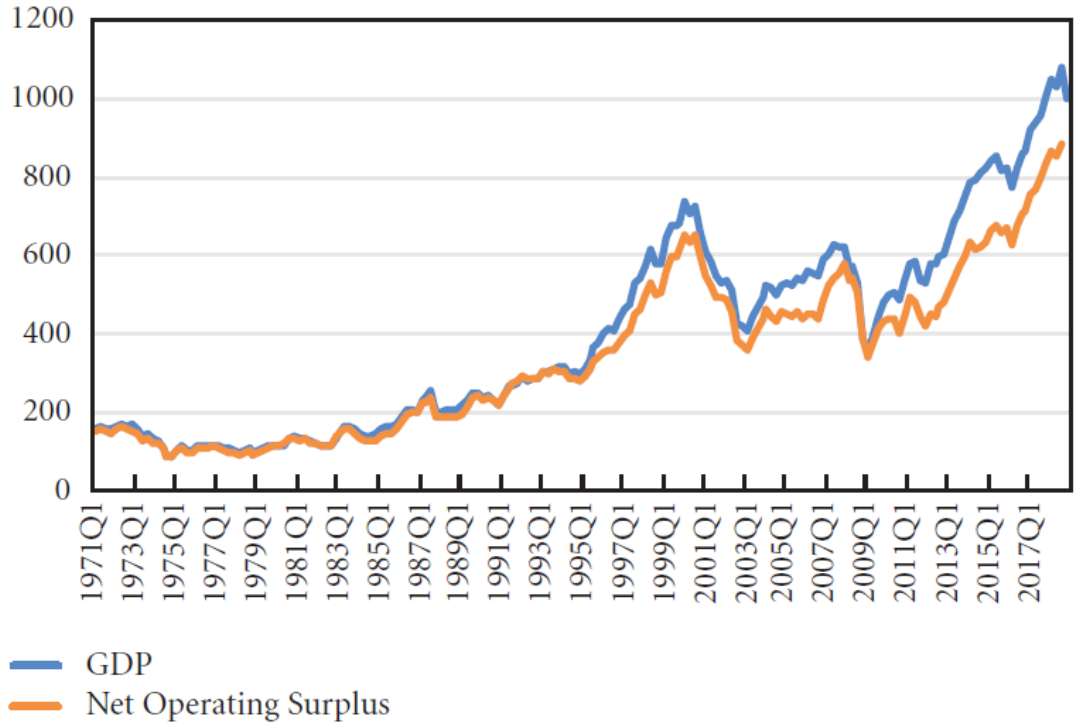
Obviously, such a scenario is purely conjectural. Processes like these, unsustainable as they may be, can continue for a long time, especially when monetary policy is as accommodating as it is right now. In fact, the drop in the stock market in the last quarter of 2018 was a sign of the market's realization of the fragility of balance sheets in the face of tightening monetary policy. In turn, the slowdown in the pace of the Fed's interest rate increases seems to indicate policymakers' recognition of the same fragility.

(2018) also introduce a narrower definition: firms with a ratio of their assets' market value to their replacement cost (Tobin's q) that is below the median within their sector in any given year. These studies attribute the rise of the share of zombie firms to easy monetary policy that did not enforce the "creative-destruction" process of the market rigorously enough.

[6] Henderson (2019) reports related data.

[7] More specifically, the assumption of this scenario was a fall in the stock market of around 35 percent, which would induce a second round of deleveraging lasting until the end of the projection period, with the debt-to-income ratios of households and firms falling to their early-2000s levels.

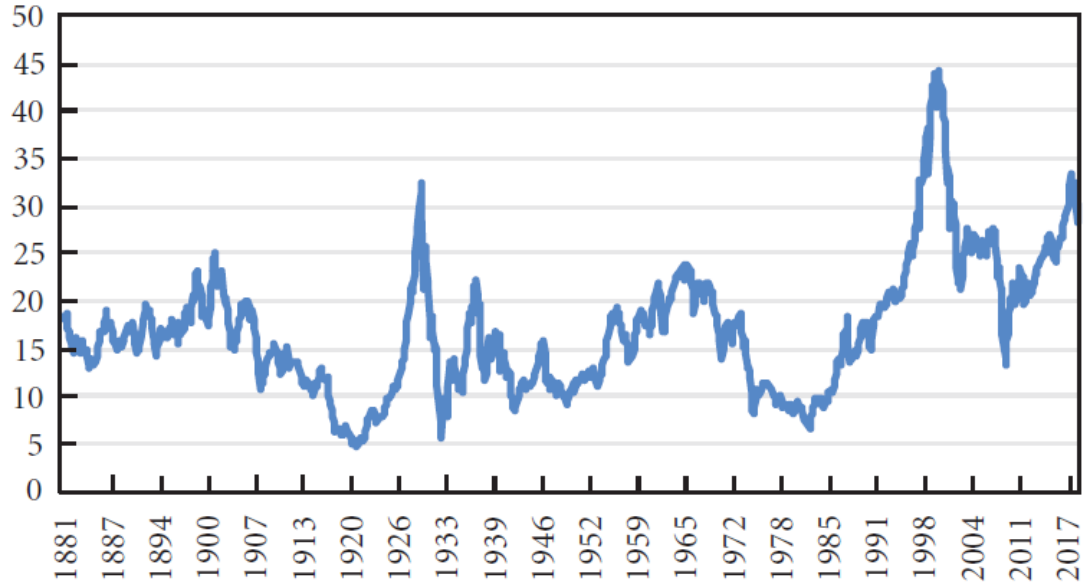
Figure 8a Ratio of market capitalization to GDP and net operating surplus, 1971Q1–2018Q4 (1975Q1=100)



Note: The index is calculated as the ratio of end-of-period Wilshire 5000 index to GDP and net operating surplus, respectively.

Sources: BEA; Wilshire Associates; authors' calculations.

Figure 8b Shiller cyclically adjusted price–earnings ratio P/E 10, 1881 – March 2019

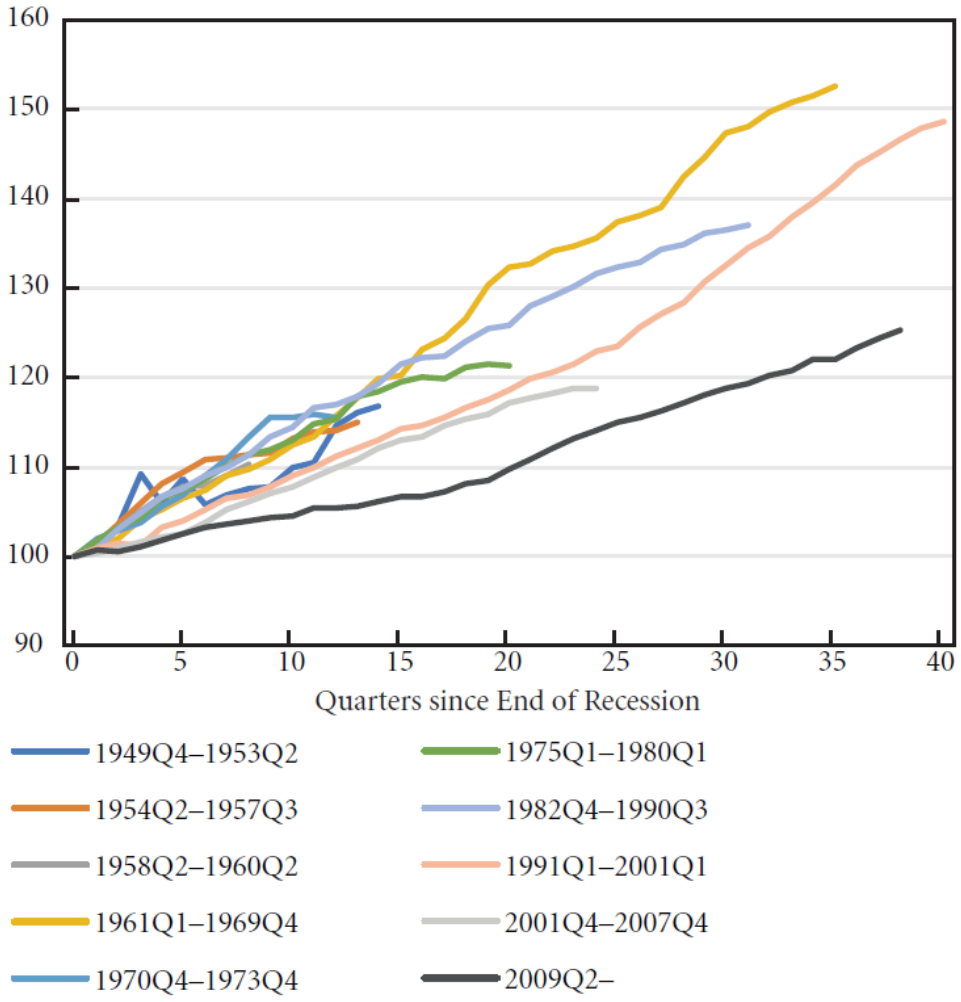


Source: econ.yale.edu/~shiller/data.htm.

Income inequality

The fourth major structural problem of the US economy is the very high level of income inequality. Various studies using different methodologies have shown that since the late 1970s, the income of poor and middle-class households has stagnated. As a result, almost all the benefits from the growth of the US economy over that period have accrued to the top income brackets (Piketty 2014; Galbraith 2012; Wolff 2017). This increase in inequality is self-reinforcing. As the top income brackets capture a higher share of income, they are able to tilt the distribution further in their favor.

Figure 9 Index of Consumption in US Recoveries (trough=100)



Sources: BEA; authors' calculations.

The most serious consequences of these extreme levels of inequality are political.[8] Nevertheless, they also have some important economic repercussions. The most straightforward effect of this redistribution is that the income of households with a high propensity to consume stagnates, while at the same time the income of rich households – with a lower propensity to consume – increases. This exerts a strong negative pressure on

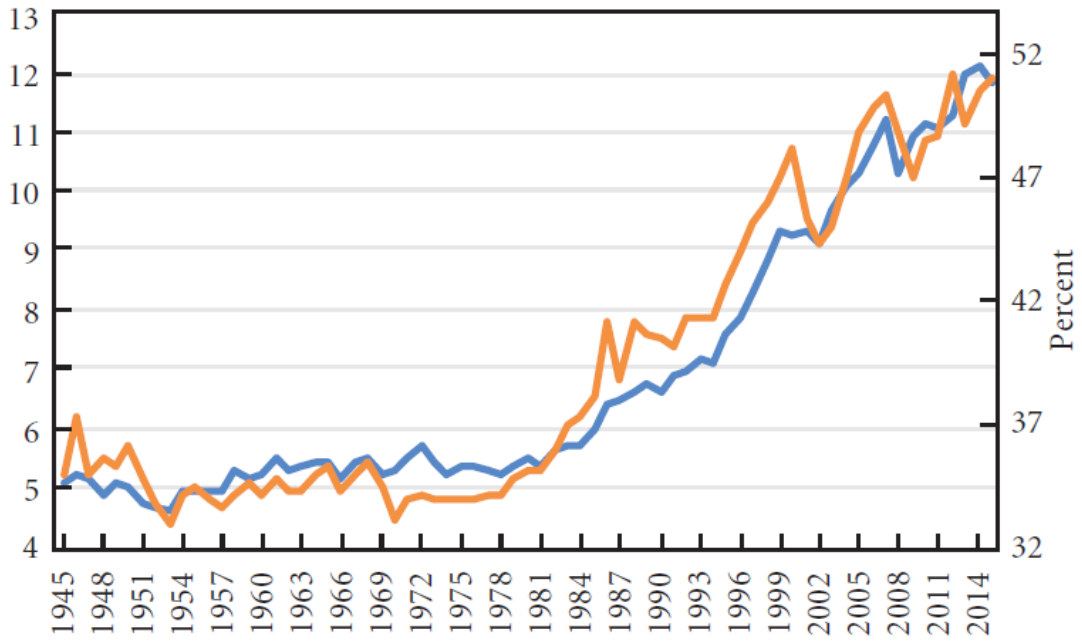
[8] The extreme concentration of income and wealth at the top – combined with the lack of control of political money – leads to the disproportionate influence of high-income households and corporations on the political decision-making process and undermines the democratic institutions of the country.

consumption. As we can see in Figure 9, the trajectories of consumption in postwar US recoveries are very similar to those of total GDP (Figure 1). Given that consumption is by far the largest component of GDP, increasing income inequality is one of the major sources of the poor GDP performance.

Moreover, according to the theory of induced technical change, as a factor of production becomes more expensive, firms are motivated to introduce new technologies that use less of this factor. The stagnation of the real wage over the last four decades has weakened this motivation to advance technical change through labor-saving technologies. This is a major source of the slowdown of productivity growth.

Finally, the increase in inequality is related to the financial instability discussed in the previous section. Figure 10 shows that there is a strong correlation between the financialization of the economy – captured here by the ratio of total financial assets to GDP – and income inequality. The increase in the income share of the top 10 percent, depicted in the figure, implies an increase of between $3 trillion and $4 trillion in the total income of households in this top income bracket, compared to a counterfactual in which there was no increase in inequality. Since the saving rate of these households is high, the resulting increase in liquidity has been a major contributing factor to the instability of the financial markets.

Figure 10 Financialization and inequality, 1945–2015



Sources: BEA; Federal Reserve; World Inequality Database; authors' calculations.

On the other side of the coin, the households at the bottom of the distribution, whose incomes stagnated, had to borrow in order to finance their normal consumption expenditures, including some services – like education or health care – that became increasingly expensive. This rising indebtedness of middle-class households managed to support growth in the pre-crisis period. The inability or unwillingness of these households to continue increasing their debt-financed spending in the post-crisis period is a major drag on the economy, as is apparent in Figure 9.

The macroeconomic effects of taxing the rich

The discussion so far has suggested that the US economy has some serious structural problems that impede a sustainable recovery and growth in the future. Obviously, these issues are not easy to address, and there is no silver bullet for all of them. A concerted effort must be made to deal with these challenges in a holistic way – an effort that begins with a recognition of the structural weaknesses and their interrelationships and requires experimentation with various policies.

One policy that moves in this direction is an increase in the tax rate of the very rich. Such an increase could tackle – at least partially – the problem of increasing inequality. Also, as will be shown in what follows, if the resulting increase in the federal government's revenues is accompanied by an increase in its outlays of the same size, this policy could have some significant positive macroeconomic demand effects.

Three related proposals have been made so far. First, Elizabeth Warren, the senior senator from Massachusetts and contender for the Democratic presidential nomination, proposed a progressive annual wealth tax on households with high net worth. More precisely, the proposal is an annual 2 percent wealth tax on household net worth above $50 million, with an additional 1 percent tax on net worth above $1 billion.[9] According to the estimates of the economists Emmanuel Saez and Gabriel Zucman that accompany the proposal, the tax would affect fewer than 1 percent of American households (around 75,000) and would generate around $2.75 trillion over the 10-year period 2019–28, or roughly 1 percent of GDP per year.[10]

Another proposal has been advanced by Bernie Sanders, junior senator from Vermont and another candidate for the Democratic nomination. His proposal suggests the establishment of an estate tax for estates worth more than $3.5 million. The tax would be progressive and would increase from 45 percent for estates worth between $3.5 million and $10 million to 50 percent for estates worth between $10 million and $50 million, 55 percent for estates worth more than $50 million, and finally 77 percent for estates valued in excess of $1 billion. The plan would also end the tax break for dynasty funds and close other potential loopholes currently in existence.[11] The proposal does not have estimates of how much revenue it would raise in total. It mentions that the families of the 588 billionaires in the United States, under the current valuation of their wealth, would eventually have to pay $2.2 trillion. Since this is an estate tax, the timing of these revenues is uncertain.

Finally, Alexandria Ocasio-Cortez, the newly elected representative for New York's 14th congressional district, recently suggested increasing the top marginal tax rate by creating a new 70 percent tax bracket for incomes above $10 million a year. According to some rough estimates, such a tax would raise around $300 billion over a period of 10 years.

In what follows, we simulate two related scenarios. Scenario 1 simulates the macroeconomic effects of the wealth tax proposed by Senator Warren. For the purposes of our simulations,

[9] The proposal can be found here: https://www.warren.senate.gov/newsroom/press-releases/senator-warren-unveils-proposal-to-tax-wealth-of-ultra-rich-americans
[10] The details of Saez and Zucman's calculations are here: https://www.warren.senate.gov/imo/media/doc/saez-zucman-wealthtax.pdf
[11] The proposal can be found here: https://www.sanders.senate.gov/download/estate-tax-one-pager?id=DE8AEADA-A3F5-4D26-8517-F6730F161E29&download=1&inline=file

this proposal has the benefit of a precise timeline that we can implement in our model. However, from a macroeconomic point of view, the results would be similar to those of an estate tax if the latter generated the same amount of revenue.

Scenario 2 simulates a 10 percentage point increase in the average tax rate paid by the top 1 percent of the income distribution. As mentioned, the proposal to introduce a top marginal tax rate of 70 percent on annual incomes above $10 million would raise an average of $30 billion per year. From a macroeconomic point of view, this amount is very small (around 0.15 percent of 2018 GDP) and applies to a tiny fraction of the population: the top 0.01 percent.[12] It is worth mentioning that the tax increase simulated here is less than the "optimal" tax rate that is proposed in the recent related public economics literature (e.g., Diamond and Saez 2011; Romer and Romer 2014).

For both scenarios, the running assumption is that the increase in tax revenues is compensated for by an equivalent increase in government outlays. This assumption allows us to isolate the macroeconomic effects of redistribution.

For Scenario 1, we use the aforementioned calculations by Saez and Zucman. After adjusting for potential tax avoidance and tax evasion, they estimate total revenues of $2.75 trillion over 10 years, or roughly 1 percent of GDP per year. On top of these calculations, we assume a marginal propensity to consume for these very rich households of 0.2. Therefore, we adjust the potential effects downward by the related loss of consumption.

For Scenario 2, we use information from the Congressional Budget Office's *The Distribution of Household Income, 2015* (CBO 2018). According to these data, the average pre-tax income of the households in the top 1 percent in 2015 was $1.764 million, and those households paid an average tax rate of 33 percent, which results in an average post-tax income of $1.178 million. This rate is lower today, after last year's tax cuts. For the purposes of our simulations we age the data, assuming that total market income of the top 1 percent for the period 2019–23 grows at the rate of growth of nominal income. We also assume, in line with the related literature, that higher taxation acts as a disincentive to generate and/or report more income. This disincentive is captured by the elasticity of top incomes with respect to the net-of-tax rate (if the tax rate is τ, then the net-of-tax rate is $1-\tau$). More precisely, the elasticity measures the percent increase in average reported income when the net-of-tax rate increases by 1 percent. For our calculation, we use a value of 0.25 for this elasticity, which is the average estimated value in the literature. The total revenues from such a tax are around 1.3 percent of GDP. Finally, as in Scenario 1, we adjust for a marginal propensity to consume of 0.2 for the top 1 percent.

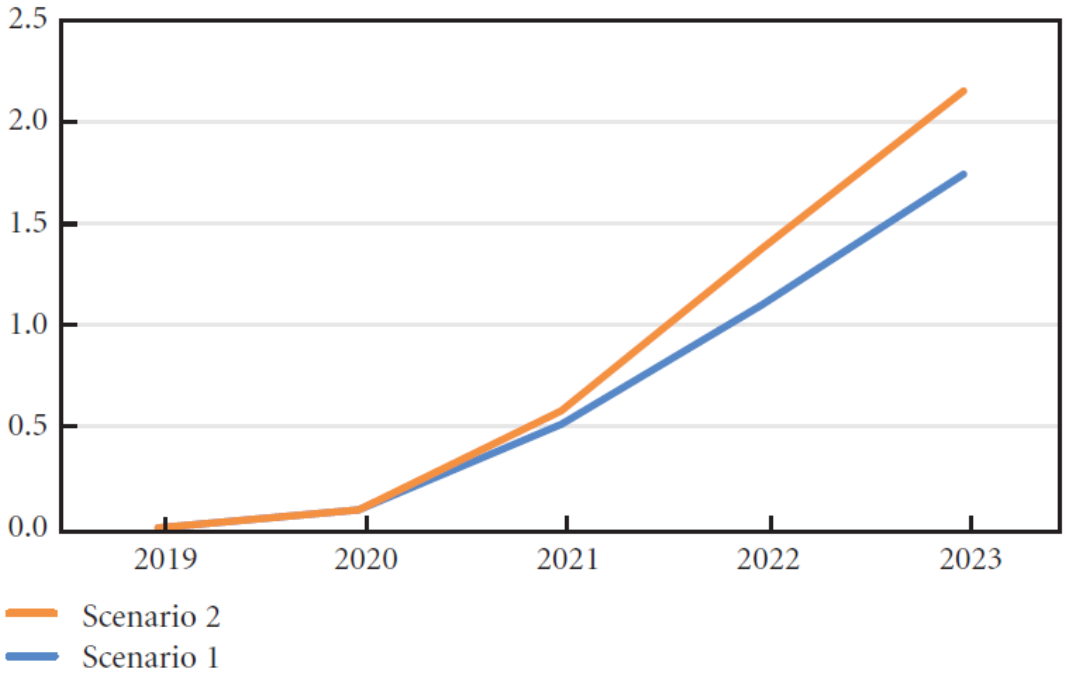
The two scenarios are implemented on top of a "business as usual" baseline scenario, where we use as a point of reference the projections of the CBO in its recent *Budget and Economic Outlook: 2019–2029*. A detailed description of the baseline scenario is provided in the appendix. The projection period for the baseline is the five-year period 2019–23. The two scenarios are implemented for the years 2020–23.

The macroeconomic effects of these policies are summarized in Figure 11. As we can see, GDP under Scenario 1 will be around 1.7 percent above the baseline by the end of 2023. In

[12] According to the World Inequality Database, the threshold income of the top 0.01 percent was $9.56 million in 2014.

Scenario 2, GDP will be around 2.2 percent above the baseline. Hence, in both scenarios the overall multiplier of the proposed polices is around 1.7 – that is, increasing the tax revenues from the very rich by 1 percent of GDP, if spent by the government, leads to a 1.7 percent increase in GDP.

Figure 11 Percentage difference in real GDP compared to baseline



Source: authors' calculations based on the Levy Macroeconomic Model.

As mentioned, the assumption of the simulations is an ex ante balanced budget. As one would expect under these assumptions, the boost in demand and output eventually leads to an improvement in the ex post balance. At the same time, there is a deterioration of the private sector balance – due to the increase in taxation (which decreases disposable income) – and the current account balance, because of the increase in imports due to the boost in economic activity.

Some final remarks are in order here. There is only so much of the increase in income inequality that can be reversed through taxation. A more important part of this process would be a change in the primary distribution of income. This would require a variety of policies that would change the structure of the labor and product markets. That discussion goes beyond the scope of this paper.

Second, our simulations do not mean to suggest that government should follow balanced budget policies. On the contrary, as emphasized earlier, the abandonment of treating the government as if it were a big household that faces a strict budget constraint is one of the required policy changes for sustainable recovery. A growing majority of macroeconomists, including ourselves, are in agreement with such policy change.

Finally, our simulations suggest that, given the current configuration of the US economy, a redistribution of income toward middle-class and poor households can have a significant positive macroeconomic effect, in the form of a boost in aggregate demand. Some positive

side effects, which were not explicitly treated here, would include a greater incentive for productivity-enhancing technical change and a taming of financial instability. Important as all these effects may be, the main case for redistribution is not economic.

Conclusion

This report analyzed the recent trajectory, the present state, and the future prospects of the US economy. We pointed to four main structural problems: weak net export demand, fiscal conservatism, increasing income inequality, and financial fragility. These problems can explain how we arrived at the crisis of 2007–09 and the weak recovery that has followed, as well as why the prospect of a recession is increasingly likely.

The US economy is in dire need of deep structural reforms that will deal with these problems and propel it toward a sustainable future. In this report, we analyzed a pair of policies that move in that direction, both involving an increase in the tax rate for high-income and high-net-worth households. Even if the primary justification for such policies is not economic, we show that if this increase in taxes is accompanied by an equivalent increase in government outlays, the redistributive impact will have a positive macroeconomic effect: a 1 percent of GDP increase in tax revenues from the richest households would lead to a 1.7 percent increase in GDP, while a 1.3 percent increase in such revenues would result in a 2.2 percent boost to GDP (again, if matched by a rise in public spending in each case). Moreover, although a more wide-ranging policy effort is required to significantly reduce income and wealth inequality – particularly by addressing pre-tax inequality – the tax policies considered in this report would represent a step toward building a more stable US economy.

Appendix

For our baseline simulations, we follow our usual procedure of anchoring them to the CBO's most recent *Budget and Economic Outlook* for the years 2019–29 (CBO 2019). The baseline evaluates a "business as usual" scenario. The growth rate is assumed to be slightly above 2 percent for the first two years and converges toward 1.5 percent by the end of our projection period in 2023. Meanwhile, the primary government deficit remains relatively stable.

The simulations make assumptions that are as "neutral" as possible: a low level of inflation around 2 percent and a constant nominal exchange rate. US trading partners have the growth and inflation rates projected by the International Monetary Fund's October 2018 *World Economic Outlook* (IMF 2018) and its recent January update (IMF 2019). Equity and real estate market prices are assumed to increase mildly, and the effective federal funds rate grows according to the median projection of the Federal Open Market Committee. Finally, during the projection period the debt-to-disposable-income ratio of the household sector is assumed to remain stationary, in line with its behavior over the last few years, while the debt-to-income ratio of firms increases along its postcrisis trend.

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What can economists and energy engineers learn from thermodynamics beyond the technical aspects?

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Abstract
For over half a century, progress in non-equilibrium thermodynamics, in particular with the emergence of the theory of dissipative structures, has inevitable implications for the self-organization of human societies and biodiversity whose losses affect directly subsistence and daily life. Seen from this angle, the thermodynamics of human societies resulting from that of living organisms, developed at the end of nineteenth century, suggests a likely collapse of societies that dissipate the most energy. In the unbalanced pursuit of economic growth, for the sake of competitiveness, economists and energy engineers must take into account this risk seriously before the situation becomes more critical. This awareness, resulting from the study of complex non-linear systems, can help build better energy solutions based on the energy efficiency and renewable energies, which must replace progressively fossil fuels to keep to a minimum their use.

Key-words economics, thermodynamics, energy engineering, efficiency, renewable energies

Introduction

Thermodynamics as a science was born from the idea that “heat can be the cause of movement, it even has a great motive power, the conversion of heat to power: steam engines” (Carnot, 1824, p. 1). In this sense, the practice preceded, by more than a century, the theory since the first steam engine was built by the engineer Thomas Newcomen, in 1712, to pump water from the coal mine of Dudley in England. Thermodynamics is therefore a branch of physics whose fundamental object is the study of the properties of systems where the notion of heat intervenes.

The choice of the expression “heat can be the cause of movement”, by the initiator of thermodynamics, is not insignificant since it is not limited to engines. This opens the field to all the structures involved in the dissipation of heat, including living beings and human societies, which constantly interact with their respective environments, and offers new perspectives for energy engineering in order to develop human-centered innovative solutions that benefit all without harming the environment, and without compromising the ability of future generations to innovate solutions that best respond to their needs and aspirations.

In this perspective, economic life is not a closed and autonomous universe governed by independent laws as stipulated by the mainstream theory. This leads to the following questions: How did the idea of thermodynamics of living organisms come to be? How did it open the way to the thermodynamics of human societies? What is the impact of this evolution on energy engineering, which concerns optimization of energy systems interacting with their respective environments?

After reviewing the literature and clarifying the epistemological posture, this paper deals with the thermodynamics of living organisms, then that of human societies to explore the impact of this evolution on energy engineering, before revealing some global guidelines that might interest economists and energy engineers who aspire, despite multiple constraints, to build a better world for all.

Literature review and epistemological posture

Thermodynamics has aroused, since 1824, the interest of researchers in physics, chemistry, mathematics, medicine, sociology, anthropology, history, and more recently algorithmologists who perceive the second law of thermodynamics as a law of information (Béranger, 2018, p. 62). Energy is everywhere, to succeed in using it, organisms and devices need information.

One of the first scientists to use the law of entropy in economic analysis is the physician Sergei Podolinsky. In his work published in Ukrainian, at the end of the nineteenth century, under the title 'Труд человека и его отношение к распределению энергии' (Podolinsky, 1880), which means *'Human work and its relation to the distribution of energy*', he proposes a new definition of human work based on the second law of thermodynamics (Vozna, 2016, p. 2). After studying entropy in biological processes, he worked on economics as an open system of energy flows (Alier, 2008, p. 115).

Economists have ignored for a long time the laws of thermodynamics (Burley and Foster, 1994, p. 1). When they are interested, they often remain prisoners of their economocentrism, i.e. the tendency to use the contributions of other sciences to further refine their art of persuasion. As noted by Robert Gibrat (1936, p. 25), some economists have argued that economics needs principles at the height of those of thermodynamics. To this end, they imagine that humanity as a whole is a closed system, living on itself. This system includes, in reference to what is commonly known as the law of supply and demand, producers who are at the same time consumers. The problem is to establish the equilibrium equations of all the exchanges, starting from these two principles: conservation of the masses and conservation of values. In this perspective, Marc Lichnerowicz (1970, p. 159) proposed to develop a model of economic exchange whose principles are analogous to those of thermodynamics or inspired by them. For their part, Mark Glucina and Kozo Mayumi (2010) consider that thermodynamics seem more relevant for constructing a descriptive model, or pre-analytical vision of the economy, because they involve physical constraints on production and consumption. However, they do not seem to facilitate mathematical modeling in economics. As if the latter was an end in itself. The only usefulness of mathematics, as we learned during our engineering curricula, is to give greater clarity to a subject, which allows debate, because people can only listen to each other if they have a common way of posing problems, as Gilles Deleuze (1983) taught us during one of his fascinating conferences available today to the general public.

Inspired by the second principle of thermodynamics, the economist Nicholas Georgescu-Roegen (1971) introduced in economics the notion of entropy: human production, which is only a transformation of matter, induces an irreversible upheaval in the state of the world. In the same way that energy is degraded during a transformation, the materials used inevitably degrade in economic growth: an important part of the resources is lost forever. He thus calls into question the possibility of recycling and provides for forced degrowth, in the

long term, by the creation of entropy induced by human activity. Unfortunately, his approach is based entirely on the thermodynamics of the nineteenth century in terms of a closed system. Most of the work that proclaims, in one way or another, the importance of entropy in economics (McMahon & Mrozek, 1997; Ayres , 1998; Bryant, 2007; González, 2009; Kummel, 2011; Avery, 2012; Kovalev, 2016) are unaware of the advances in research in this area because they seem to be struggling to get out of their box, and realize that in the face of global perils, it is necessary to open up to other sciences in a serious and assiduous way to understand how the return effects, the permanent interactions, the chaotic accelerations, the unexpected bifurcations, the entropy, and the arrow of time affect the phenomena, both physicochemical and human. In the face of perils lying in wait for humanity, it is necessary to link the specialties to better understand the phenomena.

This epistemological posture, which is mine in this chapter, allows to explore new avenues of research so that the random, the irreversible, the unpredictable occupy an increasingly important place in economics and energy engineering, especially to better refine the decision-support, environmental impact assessment and modeling applied to climate change mitigation, the energy transition. In other words, the phasing out of energy sources based on the fossil carbon in favor of renewable energies, and the preservation of the biodiversity that is essential to ensure the future of humanity and a healthier way of life. The advances of research in thermodynamics can help to raise awareness on the relativity of economic thought, to better understand the economic evolution, and the risks of disaster that it incurs to the Earth. It is not a question of considering economics as a branch of thermodynamics (Roddier, 2015, p. 2), nor of substituting the laws of thermodynamics for the so-called law of economics, knowing that the word law does not have the same connotation in the literature on thermodynamics as in mainstream economics, as we will see later. In addition, energy engineering programs must place greater emphasis on the philosophy of science, sociology of knowledge, ethics and history of science and technology so that innovation does not become an end in itself in the competitiveness race.

Thermodynamics of living organisms

The idea of thermodynamics of living organisms had been debated at the end of the 19th century (Moret, 1884, p. 18; Hirn, 1887, p. 673; Gertsen, 1887, p. 3; Chauveau, 1888, p. 32). Generally speaking, a living organism, be it an animal or a plant, is a being who is born, grows up, eats, rejects waste, reproduces and dies. Certain unicellular organisms, i.e. living beings that are composed of a single cell such as bacteria, yeasts and plankton, can survive without fulfilling some of these conditions. If the laws of thermodynamics apply to living beings, we must have, according to Maignon (1907, p. 661), the following formula:

$$\text{Chemical Expense} = \text{Heat} \pm \text{Mechanical Work}$$

This idea of applying the laws of thermodynamics to living beings stems from a reasoning which joins to sources of heat of mechanical or chemical origin other sources which are peculiar to living beings. It is well known that the temperature of certain animals is kept several degrees higher than the ambient temperature and remains fairly constant despite variations of it. Some plants are in similar conditions. Hence the need to investigate how this vital heat in living beings is related to known sources, whether it obeys the same laws or if the exercise of life would not in any way change the essential conditions (Jamin & Bouty, 1906, pp. 118-119).

If some researchers have assimilated, under the prism of engineering, the living being to a living engine, and imagined that there could be applied to living beings a special thermodynamics where the principle of energy conservation would be preserved, but where the principle of Carnot would not be (Berthelot, 1917, p. 127), others say that living engines, more complex than common engines, have also their laws. These systems of rules are at the same time thermodynamic and physiological: thermodynamic because the animal engine is material and cannot escape the laws of matter, physiological, because this animal engine must present the laws proper to living beings, that the object of physiology is the study of the functions and properties of their organs and their tissues. It would be reductive to analyze living engines by extending the energetic rules of the material systems common to living beings, without taking into account the special conditions imposed by life (Lefèvre, 1912, p. 301). This refutes the idea that it is the interpretation of thermodynamics that was incompatible with experimental physiology and not thermodynamics in itself (Ameline, 1898, p. 17).

Advances in research have led to the idea that the second principle of thermodynamics is applicable to a living organism only in the very general form proposed by Ludwig Boltzmann in the context of the kinetic theory of gases. In this respect, Louis de Broglie (1953: 60) explains that according to the static interpretation of thermodynamics, the entropy S of a system appears as directly:

$$S = k\ log\ W$$

Where 'k' denotes the Boltzmann constant and 'W' the thermodynamic probability or statistical weight.

Soon after, Josiah Willard Gibbs generalizes this equation to the out-of-equilibrium case through the following equation:

$$S = k \sum p_i . log\ p_i$$

Where p_i is the probability of the system being in a particular microscopic state. At equilibrium, all microscopic states are equiprobable so that $p_i = 1/W$ which restores the Boltzmann expression.

The second principle of thermodynamics or the increase of entropy, adds de Broglie, then receives an almost intuitive interpretation: "It expresses the tendency for any system to evolve towards states of greater probability". In this approach, probability is not a non-knowledge, but a way of explaining chaotic and irreversible phenomena evolving out of equilibrium far from any determinism. This interpretation leads to the exploration of thermodynamics of human societies whose future is not written, but remains to be built day by day.

Thermodynamics of human societies

In his lessons on thermodynamics professed during the first semester of 1888-1889, Henri Poincaré (1908a, pp. xii) notes that "the law of energy conservation can have only one meaning, it is that there has a property common to all possibilities; but, in the deterministic hypothesis, there is only one possible, and then the law makes no sense". In this, the

famous mathematician stands out from the determinism that finds its origin in the thought of Claude Bernard ([1865]1984, p. 109) as the basis of any experimental scientific method. The famous mathematician also writes in his '*Last Thoughts*':

> "But we are in the presence of a fact; science, rightly or wrongly, is deterministic; wherever it enters, it brings in determinism. As long as it is only physics or even biology it does not matter; the domain of consciousness remains inviolate; what will happen the day when morality becomes in turn object of science? It will necessarily become impregnated with determinism and it will undoubtedly be its ruin" (Poincaré, 1920, p. 245).

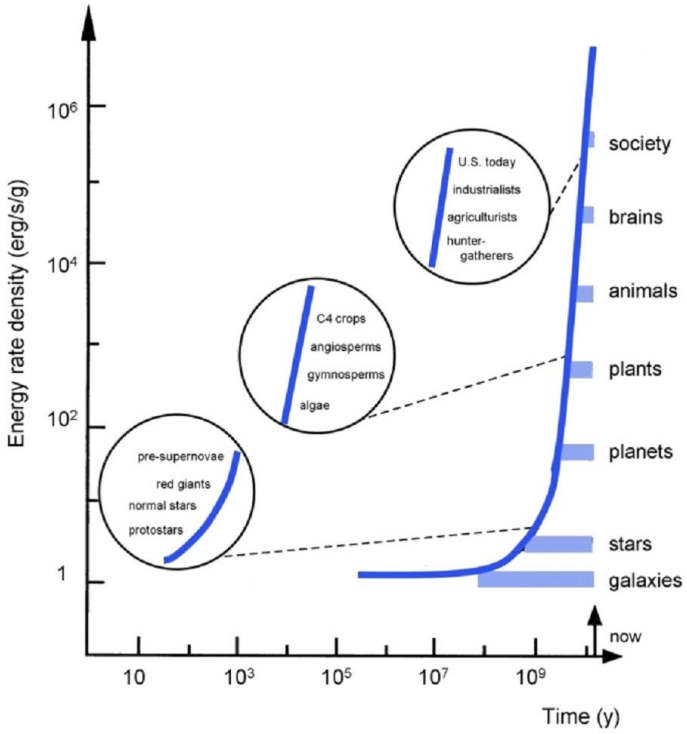
The existence of a law in thermodynamics, unlike economics, does not mean that it has only one way of acting. Does not the dominant discourse often repeat that we must adapt or disappear!

In a conference that opened the way to the thermodynamics of human societies, the sociologist Maurice Hauriou (1899, p. 5) took up this idea by considering that only the "thermodynamic laws shed some light on the possibilities of freedom". This presupposes a permanent interaction between the human and his environment and overlaps with the formulation of Douglas Hugh Everett, in his "Introduction to the Study of Chemical Thermodynamics" (1959), according to which "a particular proportion of the Universe is called the 'system' while the rest of the Universe is called 'the outside' or 'the environment" (Rybac, 1968, p. 137).

This conceptualization has allowed researchers to develop the thermodynamics of open systems, traversed by a flow of matter and energy, whereas the classical conception of thermodynamics considers closed systems, whose exchanges with the external environment are null or limited and tightly controlled. From this angle, the new thermodynamics gives a major importance to the phenomenon of irreversibility, where the old is placed in the vicinity of equilibrium, in the reversibility zone, which makes the human world appear to be subject to its potential momentum and not just the laws of thermodynamics in their traditional meaning. In this context, the appearance of the notion of dissipative structure (Prigogine, 1967, p. 371), which applies to phenomena as different as cyclones or living species, seems particularly interesting because it applies to human societies. Cyclones, living species, human societies, are famous for the unpredictability of their evolution.

Starting from the idea that to move, work, communicate, it is necessary to be constantly supplied with energy, that the natural selection favors the living organism which dissipates the energy most quickly (Lotka, 1922, p. 149), and that the basis of the national economy is the struggle for energy (Soddy, [1933]2014, p. 63), the astronomer Eric Chaisson (2001, p. 17) drew a curve that shows the energy dissipated per unit of mass (figure 1), and reveals the emergence of structures capable of dissipating more and more energy over the history of the universe. Human societies are at the top, since human beings are the only ones to have industries, services and all kinds of products that dissipate a lot of energy.

Figure 1 Increase of energy dissipation



Source: Chaisson, Eric (2001) *Cosmic Evolution: The Rise of Complexity in Nature.* Cambridge: Harvard University Press, p. 17.

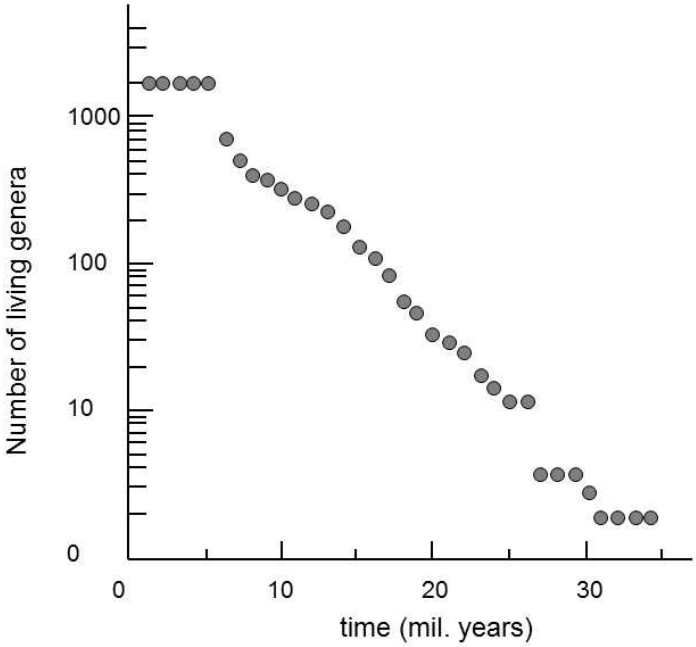
As astrophysicist François Roddier (2014, pp. 2-4) notes, dissipative structures memorize information about their environment. The more a dissipative structure memorizes information, the more it dissipates energy. But the faster it dissipates energy, the faster it changes its environment, so that the information it memorizes quickly becomes obsolete. The dissipative structure then has more and more difficulty dissipating energy. To be able to continue to do this, it must constantly restructure itself in order to finally reach a critical point. In this sense, the more a human society seeks to adapt itself to an evolving environment, the more it dissipates energy, therefore more it makes it evolve. Each structure will seek to adapt itself faster and faster until where the time of adaptation can no longer decreases. In this context, its vitality takes a hit and goes off gradually.

If the implications of out-of-equilibrium thermodynamics suggest a likely collapse of the most energy-dissipating human societies, the question then is if it is possible to avoid or at least delay collapse of such societies. To do this, the dynamics of the process needs to be understood in order to evolve slowly enough to continually have the time to adapt itself far from the obsession with competitiveness that feed a frantic race that have no other aim than to keep a market share.

This questioning is based on the Red Queen's Hypothesis, proposed by the biologist Leigh Van Valen (1973, p. 17), inspired by an episode of the famous novel Lewis Carroll's Alice's Adventures in Wonderland published in 1865: the young Alice meets a queen dressed in red, and is soon thrown into a frantic race. Alice runs a moment with her, then, surprised, tells her: "Well, in our country, you'd generally get to somewhere else – if you run very fast

for a long time, as we've been doing?" And the Queen answers: "Now, here, you see, it takes all the running you can do, to keep in the same place. If you want to get somewhere else, you must run at least twice as fast as that!" (Carroll, 2015, p. 206.) When the environment evolves faster than a living species can adapt to it, this species is doomed to stand still. However, nothing is moving forward, and this inevitably leads to the near extinction of the species as shown in figure 2 which represents the gradual decrease in the number of mammalian genera over approximately thirty-six million years.

Figure 2 Survival of the individual genera of mammals.



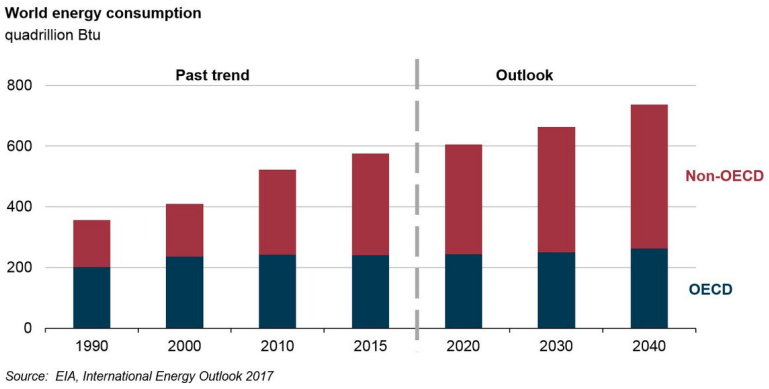
Source: *Evoluční biologie*, 2. vydání (*Evolutionary biology*, 2nd edition), J. Flegr, Academia Prague 2009, According to Van Valen, Leigh (1973). A New Evolutionary Law, *Evolutionary Theory*, 1, July, p. 5.

In this context, a restriction of energy dissipation is needed to oscillate around critical instabilities that are not static critical points but dynamic critical points. It is necessary to cooperate in this collective awareness in a multidisciplinary perspective (thermodynamics, energy engineering, environmental science, economics) in order to take action in time to escape from the principle of energy dissipation of Dewar Law (2003): A structure decreases its entropy (organizes itself) to maximize the entropy flow (the rate of energy dissipation). This principle, has up to now, took the Humanity of renewable energies to fossil fuels and to a globalization focused on an unrestrained race for competitiveness (Groupe de Lisbonne, 1995). This reflection which deals with the field of energy (conversion and use) in relation to environmental and economic issues, also deals with the current fundamental aspects related to the evolution of our world (earth, sky, water, human life), to rebuild it on a new basis. It stands at the antipodes of competitivism, the ideology of competitiveness, whose success never ceases to surprise, every day, despite the lack of standard definition, robust indicators and a rigorous theory (Belabes, 2001). The impact of advances in thermodynamics research on energy engineering should be explored.

Impact on energy engineering

Energy engineering is a diverse field that deals with energy efficiency and services, facility management, environmental friendliness and alternative energy technologies. Global energy consumption is steadily increasing across the globe and the sector will be affected by an increasingly complex set of economic, geopolitical, and environmental challenges. In its latest *'Global Energy Transformation report*: *A Roadmap to 2050'*, the International Renewable Energy Agency (IRENA, 2018, p. 16) believes that the global energy system needs to be transformed according to the two pillars of energy transition that are energy efficiency and renewable energy. As shown in Figure 3, global energy consumption could increase by 28% between 2015 and 2040 according to the latest EIA (2017, p. 6) report "International Energy Outlook 2017".

Figure 3 World energy consumption



Under the pressure of environmental activists and the growing resistance of consumers, especially via social networks, manufacturers are called to invest in cleaner energy sources, optimize their assets and customer interaction, and capture the value of data from the digital world using recent advances in algorithms and artificial intelligence. They must design energy systems and processes by anticipating the strategic stakes of the development of new energies, understanding the prospects of economic and managerial change raised by industrial ecology in the product life cycle project, from its conception to its recycling.

In this context, energy engineers must receive training that is based on a multidisciplinary and transversal approach, allowing to match the different stages of the energy chain, from its production process, to the optimization of the procedures of rational use, considered in the perspective of environmental respect, devoting a large part to the phenomenon of irreversibility and to the notion of entropy, by developing analysis methods to deal more effectively with the very current issues of management and economics related to energy engineering, energy mastery, and intelligent use of thermal / electrical renewable energies: calorimeters, evaporators, condensers, flow meters, pumps, turbines, compressors, combustion chambers, boilers, cooling towers, heat engines, turbojets, thermal power chimneys, fuel cells, heat pumps, air conditioners, refrigerators, thermoelectric generators. The knowledge and skills of engineers must be up to date to meet the challenges of this new millennium and they must cultivate a commitment and a passion for lifelong learning, which will generate new useful solutions that make life easier and more durable (Riley, 2011).

The energy strategy of some countries, with an environmentally friendly culture and a consciousness of the risks related to the maximum energy dissipation, tends to accentuate the role of local communities to build a more viable future. The Energy productions are moving towards decentralization, and promoting renewable energies often anchored in territories that mobilize local resources. This orientation complicates the management and operation of energy systems, spatially and temporally, particularly at level of production and consumption.

This orientation, that comes under the local governance in virtue of the principle of subsidiarity, leads to the development of territorial energy systems consisting of an integrated and simultaneous analysis of all energy aspects of a territory (needs, resources, conversion and storage technologies), considering a range of different solutions at the level of production, distribution, consumption, conversion and storage of energy, that requires the collection of a large amount of data and complex analysis tools (Cherix *et al*., 2015).

The emergence of Big Data and the multiplication of increasingly powerful data processing techniques, notably machine learning and deep learning, open the door to a multitude of applications in the energy sector. The growth of the Internet of Things can, moreover, become a key lever for adapting production to consumption, optimizing the performance of energy equipment and their maintenance and providing, with the help of sensors, valuable information and in real time on the operating status.

Beyond the deontology of engineers, historically related to the practice of consulting engineering, and the reflection on the social, environmental and cultural impact of technological development, and the risks inherent to a technical society, the ethics of energy engineering must pay particular attention to the performative nature of the technique that requires human control, and the participation of all segments of society in the technical choices and their governance, to initiate a process with the ability to regulate its internal environment, and to maintain a constant equilibrium in the face of an ever-changing external world (self-regulating process of homeostasis), driving a struggle against the maximal dissipation of energy and the process of self-organized criticality that initiates enormous transformations on very short time scales (Bak *et al*., 1987).

Faced with this major challenge that must mobilize collective intelligence and wisdom, a better conceptualization of energy engineering innovation is proving more necessary than ever beyond the slogan that innovation is an idea that creates market. Does not higher education institutions, specialized in the field of science and technology (Aulet, 2013), teach the equation:

$$\text{Innovation} = \text{Invention} \times \text{commercialization?}$$

In the broad sense, an innovation is a change that responds to a need for improvement (Conseil de la Science et de la Technologie, 2000, p. 5). Innovation is a varied and complex object. Hence the need to distinguish between incremental innovation, radical innovation, disruptive innovation and frugal innovation (Barnu, 2012), just to mention a few of the most used in the specialized literature. Future research will undoubtedly bring forth other varieties.

The substitution of the notion of “market-agencement” to the that of market, to address products as processes (Callon, 2013), is in line with such approach, following on from earlier work (Polanyi, [1944]2001), although there is still much to do. This will provide a better

understanding of the energy market-agencement so that it can be changed to a way of life that is more respectful of human beings, nature and all forms of life. As noted by Jean-Pierre Dupuy (2011), “the greatest threats today come less from the bad guys than from the industrialists of the good”.

Conclusion

Among the main lessons of thermodynamics useful to economists and energy engineers, it should be remembered that:

- The interest of thermodynamics laws, under the prism of the philosophy of science, lies fundamentally in the fact that they open the field to the possibilities of freedom in the broadest sense of the word that is not centered on the market and non-intervention of the state in business. Free market ideology does not lead to the freedom it promises, but reduces it to a consumerist freedom basically framed by the market.
- The existence of a law in thermodynamics does not mean that there is only one way to act and develop solutions. This opens the field, at least, to three strategies: to adapt to an environment, to co-construct an environment, to influence an environment.
- Complexity and chaos incite awareness essential to building a better world. The ethics of energy engineering, previously developed, can play a constructive role in this process to improve the quality of life and bring happiness to people far from the frenzied race for competitiveness that has proven to be a dangerous obsession pulling down and inducing, in the end, a dynamic zero-sum game.

Thus, the conception of laws in thermodynamics is different from that commonly accepted in economics where, ultimately, there is no choice but to adapt or disappear; it is synonymous with common property and, more generally, with trend or regularity empirically observable. In other words, it is not performative. Most economists forget or are unaware that the answers provided by models are valid only in a given context: there are no universal economic laws valid at all times and in all places.

Moreover, the non-equilibrium thermodynamics, which postulates the existence of a local thermodynamic equilibrium for each of the elementary subsystems associated with an element of space-time, opens the field to diversity and sensitivity to initial conditions. As Henri Poincaré (1908b, p. 72), points out, “if it may happen that small differences in the initial conditions generate very large differences in the final phenomena; a small mistake on the first would produce a huge mistake on the last ones. Prediction becomes impossible”.

The facts of everyday life in the field of alternative and renewable energy, which are generally ignored in economics, become worthy objects of study that need to be carefully studied by energy engineers, which undermines any fixed point, any formula that is ready, any step that starts with some certainty or leads to certainty. In the world of certainty, there is no room for questioning, nor for substantive debate. There are only answers, ready-made solutions, denying time, space, and local cultural heritage. However, as Ilya Prigogine (1998) pointed out in an interview, thirty years ago, “complexity is when the truth is no longer certain, and uncertainty is not more ignorance”. Moreover, the enemy of complexity, noted Edgar Morin (1990, p. 254), “*is not the simplicity, it is the mutilation. Mutilation can take the form of one-dimensional conceptions or reductive conceptions*”. The preliminary

study of social structures and their attributes, both material and immaterial, becomes crucial.

The notion of *wujūh ma'āsh al-'umrān al-basharī* of Ibn Khaldun ([1376]2004, 2, p. 68), literally livelihood opportunities in cities, that explores the way in which human societies that have entered a state of decadence are trying to survive, seems very timely, even if the energy of that time was essentially firewood, charcoal, oil, and dried cow dung that our grandmothers used to care for in the rural areas, about forty years ago, although electricity and gas were available to them. They had a sense of respect for the environment by giving priority to natural solutions in their wisdom putting each thing in the right place. This opened the way for an approach of history "from below" turning away from the chronicle of institutions and great men to focus on reporting micro-resistances (Thompson, [1963]1988).

From this perspective, the progress of research in thermodynamics may, moreover, incite to rediscover those who had been relegated to the pantheon of ignored or misunderstood authors despite the quotations that fail to exceed "the semantic level" (what is he talking about?, i.e. the form) for the benefit of the "critical level" (how did he work?, i.e. the structure not just the form), to quote a distinction dear to Umberto Eco (2013) who opened our minds, while we were students in engineering, in reference to semiology after reading *In the Name of the Rose*, on the need to take an interest in the relationship between technology and progress. A bit like "too much tax kills the tax", too much technology would kill technology. The practical applications of scientific knowledge, especially in industry, have no meaning except from the persons whom they should serve.

This led us, subsequently, to the writings of Ivan Illich ([1973] 2005, p. 19), who drew up a critique of the industrial society by showing that its service structures go beyond a certain threshold, against the objectives assigned to them. In this sense, technology could create more problems than it solves. But the most serious risk is that these institutionalized services make the use of expert knowledge unavoidable, which leads to freezing the imagination and to depriving individuals of simple means and their scope, to manage their lives or solve their problems as our grandmothers used cow dung to bake bread whose flavor is invaluable. This leads to the following major methodological issue: We cannot study a phenomenon with fixed images like our grandmothers used only cow dung or only electricity. The reality is much more complex and infinitely more interesting.

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Metaphors for the evolution of the American economy: progressing from the invisible and visible hands to the humanistic hand

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Abstract
The first part of this paper considers how three important metaphors can help us understand the past progress of the American economy. First is Adam Smith's "invisible hand" metaphor that helps explain how the market economy functioned in the mid to late 18th century. Second is Alfred Chandler's "visible hand" metaphor that explains about the relatively decreased importance of markets and the increased importance of managerial decision making in the allocation of economic resources during the late 19th century and the first half of the 20th century. Third is Richard Langlois' "vanishing hand" metaphor that he used to characterize the significant changes occurring in the American economy's functioning from the late 20th to the early 21st century. Taken together these three metaphors help explain how the American economy's predominant functioning has changed as the economy has transitioned from one stage to another.

The second part of this paper proposes that the next economic stage will be associated with a metaphor called the "humanistic hand". Whereas the American economy's overall economic and technological performance has been relatively high, its human performance has generally not been favorable, especially in recent decades. That assessment refers to the economy's high human costs related to chronic ailments and other ways that Americans have been casualties of the socio-economy's dysfunction. The essential nature of the humanistic hand stage is its orientation to remedying the adverse human consequences of the socio-economic dysfunctions of modern capitalism.

To explain how a humanistic hand stage might function and come into existence, this paper has drawn on the research of Elinor Ostrom and David Wilson. Ostrom understood that the economy's productive groups can become dysfunctional when a group's participants are overly self-interested, and therefore, seek to gain at the expense of others. Such a result ultimately may cause the group to cease functioning and to fail. Ostrom and Wilson recognized that what is needed are functional organizations in which group participants communicate, trust each other, and become committed to common purposes. The humanistic hand economy is one in which the predominant economic organizations are functional organizations. There is strong reason to believe that in these functional organizations, the human costs associated with economic activity will be dramatically lower than what occurs in the typical enterprises of American style modern capitalism. Lastly, Wilson has explained how the functional organization of the humanistic hand stage might come about through a process of cultural evolution.

Introduction

The purpose of this paper is to consider how a selection of significant metaphors relating to the economy can help us understand the past progress of the American economy as well as a scenario for future progress. Carefully considering these metaphors can also help us explore important socio-economic issues and help economic policymakers make decisions. Consider three very important metaphors. First, in his *Wealth of Nations* book, Adam Smith (1776) used the metaphor of the "invisible hand" to explain his view of the functioning of the market economy in the mid to late 18th century. Second, in explaining how the American economy of the late 19th century and at least the first half of the 20th century functioned quite differently from the predominantly market economies of earlier times, Alfred Chandler (1977) used the

metaphor of the "visible hand" to emphasize the relatively decreased importance of markets and the increased importance of human (or managerial) decision making in the allocation of resources. Third, Richard Langlois (2003) used the metaphor of the "vanishing hand" to characterize the significant changes occurring in the American economy's functioning in the late 20th century and early 21st century. These three metaphors relating to different economic stages have helped economists and other observers of the American economy appreciate many important aspects of the economy and how its predominant functioning has changed as the economy has transitioned from one stage to another over the years.

While it is impossible to predict the future, much less the character of future economic stages, this paper proposes an economic stage that could conceivably be the next stage. This stage is called the "humanistic hand". Whereas the American economy's overall economic and technological performance has been relatively high, the American economy's human performance has generally not been good. That assessment refers to the economy's high human costs related to chronic ailments and the other ways that Americans have been casualties of the socio-economy's dysfunction. The essence of the proposed humanistic hand stage is that to a great extent it would be oriented to remedying the adverse human consequences of the socio-economic dysfunctions of modern capitalism. This stage would be thus focused on raising the level of human well-being in the American society, a level that has generally suffered in recent decades despite the economy's rising productivity and income.

There is reason to believe that a humanistic hand economic stage could come into existence through a process of cultural evolution (Wilson, 2015). Further, based on the work of Elinor Ostrom (2009) and her colleagues on the functioning of common pool resources (CPRs), there is further reason to believe that contrary to conventional economic wisdom, destructive overuse of the resources involved can be avoided. What Ostrom learned is that users of the common resources can learn to cooperate and commit themselves to shared purposes, creating functional organizations in the process. In essence, based on the findings of Ostrom, Wilson, and others, the core principles related to successfully organizing and managing CPRs can be generalized to other contexts, thereby allowing successful large scale operation of many kinds of institutions without a tendency for the kind of dysfunction and market failure that has led to many of the societal human costs plaguing modern capitalist economies.

Economic stages, metaphors and progress

It seems fair to say that a nation's economy (or socio-economy) is always changing and evolving. Obviously some of these changes are more important than others, and some are more associated with progress than others. As indicated in the previous section, through the writings of leading authors, it is possible to identify important stages in a country's economic development and associate them with a metaphor. These metaphors along with short pertinent analyses may enable us to obtain a reasonable understanding of how important aspects of a nation's economy were developing and functioning during a particular stage as well as getting an idea about the importance of that economic stage with respect to its contribution to progress, i.e., adding to the well-being of the country's inhabitants during that stage. Although these metaphors/analyses are lacking in some ways, they nevertheless can be useful in shedding light on questions such as the following: To what extent did what was happening during that stage represent economic and social progress? How did the functioning in that economic stage lead to the succeeding stage(s)? How does this particular stage fit into the long arc of economic history? To what extent does the advent of the next

economic stage displace the immediately preceding stage? In any case, it seems that much insight into a country's socio-economic path can be obtained via this kind of inquiry even though the analysis here may be considered lacking from the standpoint of a complete historical analysis. Further, this mode of inquiry can presumably be helpful to a country's policymakers insofar as they need to take appropriate actions to guide the economy, thereby fostering progressive trends and retarding retrogressive ones in an emerging economic stage.

According to Donald McCloskey (1985), metaphors are very important to economic explanation and advocacy. Metaphors are the language that economists use: they are not mere ornaments (pp. 74-76). They have much to do with determining our economic and philosophical convictions. According to the philosopher Max Black, "a memorable metaphor has the power to bring two separate domains into cognitive and emotional relation by using language directly appropriate to the one as a lens for seeing the other" (as quoted by McCloskey (1985, p. 76). Metaphors are a "distinctive mode of achieving insight"; they have a "capacity to astonish us with implications once unseen" (p. 77). Moreover, "what is successful in economic metaphor is what is successful in poetry" (p. 78).

The invisible hand metaphor

The original version of the invisible hand metaphor can be found in Adam Smith's *The Wealth of Nations* first published in 1776. According to Smith (1937, pp. 422-423), although

> "every individual who employs capital necessarily labors to render the annual revenue of the society as great as he can. He generally … neither intends to promote the public interest, nor knows how much he is promoting it …. He intends only his own gain; … he is [in effect] … led by an invisible hand to produce an end which was not part of his intention …. [Thus] by pursuing his own interest, he frequently promotes that of the society more effectually than when he really intends to promote it."

Accordingly, voluntary, self-interested trading by business people in a free market is believed to produce unintentional and widespread economic benefits for the society. And that free trade creates signals about the value of and costs of the exchanged goods that spontaneously directs competing consumers and producers to fulfill the needs and desires of others. These market signals are understood to contribute strongly to the efficient functioning of markets. Thus, when changes in the demand for goods occur, this automatically results in price and profit adjustments that lead to beneficial, equilibrating changes in supply.

The invisible hand metaphor was used by Adam Smith, and many others over the years, to support the view that free trade motivated by the self-interests of producers and consumers creates a self-regulating system that functions better than when governments regulate the economy. The advocates of free, unregulated trade are said to support laissez faire (French for "leave it alone") policies. In time the invisible hand idea became one of the primary justifications for free market forms of the capitalist system. Further, it is important to note that the invisible hand metaphor has represented what is understood to be the essential nature of how early market oriented capitalist economies have functioned in America (not to mention a number of European economies) in the 18th century and a good part of the 19th century. The invisible hand metaphor is henceforth used in this paper to represent the early market-oriented stage of capitalism. The metaphor is meant to evoke various aspects of not only how

the early market economy functioned but how people in these economies thought about and experienced their economies.

It should be noted that the use of the invisible hand metaphor by Adam Smith represents in part how he understood markets to function, in part how he thought it should be functioning. In the latter sense, the invisible hand was an ideal. He emphasized how markets could function well with little or no government regulation and could contribute to business specialization, greater productivity, and economic growth. In other parts of his *Wealth of Nations* book, he was critical of some aspects of the market economy. However, insofar as Smith used the invisible hand metaphor, he can be faulted for insufficiently explaining about the flaws of markets, i.e., about the extent to which markets can be dysfunctional.

The visible hand metaphor

The next important economic stage takes its name from the title of Alfred Chandler's (1977) book entitled *The Visible* Hand. It is a metaphor that obviously contrasts sharply with the invisible hand metaphor. Note that this stage has also been called managerial capitalism. According to Chandler (p. 1), in mid19th century America, the "modern business enterprise took the place of market mechanisms in coordinating the activities of the economy and allocating its resources." It was the visible hand of management that "took over the functions of coordinating the flows of goods through… processes of production and distribution and of allocating funds and personnel for future production and distribution)" (p. 1). Accordingly, "modern business enterprise… became the most powerful institution in the American economy and its managers the most influential group of economic decision makers" (p. 1).

According to Chandler (1977, pp. 1-3), by the late 19th century, the large modern business enterprise typically had a fair number of business units or divisions handling its different lines of goods and services. In other words, corporations had become not only large but multidivisional enterprises. Among the important large-scale business enterprises that Chandler studied were E. I. DuPont; Standard Oil of New Jersey; General Motors; and Sears, Roebuck and Company. The activities of these companies' divisions and the transactions between them were coordinated by a hierarchy of middle and top salaried managers, not by market mechanisms. This was in contrast to the early (or traditional) American business firm that was a single unit enterprise that handled a single product and which performed a single economic function. "As late as 1840 there were no middle managers in the United States – that is, there were no managers who supervised the work of other managers and in turn reported to senior executives who themselves were salaried managers" (p. 3). Before 1840, all top managers were owners. "By World War I this [multidivisional] type of firm had become the dominant business institution in many sectors of the economy" (p. 3). "What the new enterprises did do was to take over from the market the coordination and integration of the flow of goods and services from the production of the raw materials through the several processes of production to the sale to the ultimate consumer" (p. 11).

> "The visible hand of management replaced the invisible hand of market forces where and when new technology and expanded markets permitted a historically unprecedented high volume and speed of materials through the processes of production and distribution. Modern business enterprise was thus the institutional response to the rapid pace of technological innovation

> and increasing consumer demand in the United States during the second half of the nineteenth century" (p. 12).

They were the critical engines of economic growth in the U.S. (McCraw, 2008, p. 215) not only because their operations were generally more productive than market oriented small firms but because they enabled economies of scale (Usselman, 2006, p. 596). They also contributed to growth because they were the core around which medium and small supplier firms grew (Wikipedia, p. 2).

It should be noted that the success of large multidivisional firms required the creation of a managerial class that was needed to coordinate the increasingly complex and interdependent system. The growth in the size of the managerial class by the middle of the 20th century was extraordinary. By then, "these [large] enterprises employed… thousands of middle and top managers who supervised the work of dozens and often hundreds of operating units employing tens and often hundreds of thousands or workers" (Chandler, 1977, p. 3).

"Whereas the activities of single-unit traditional enterprises were monitored and coordinated by market mechanisms, the producing and distributing units within a modern business enterprise are monitored and coordinated by middle managers. Top managers, in addition to evaluating and coordinating the work of middle managers, took the place of the market in allocating resources for future production and distribution. In order to carry out these functions, the managers had to invent new practices and procedures which in time became standard operating methods in managing American production and distribution" (p. 7).

As time went on, these managers typically pursued careers that became increasingly technical and professional (p. 8). Because of the specialized skills needed, their selection and promotion required consideration of their training, experience, and performance. No longer were family relationships and money the key factors (pp. 8-9). Managers increasingly aspired to lifelong careers involving "climbing up the hierarchical ladder" (p. 9). Along with these changes in the nature of management came the increasing separation of management from ownership.

The vanishing hand

In the view of Richard Langlois (2003), the economic stage of capitalism that Chandler had labeled the visible hand had to a considerable extent run its course by the mid20th century. In Langlois' view, the vertical integration characteristic of the large multidivisional corporations began to be replaced by enterprises that favored vertical disintegration or at least less vertical integration. According to Langlois, the large multidivisional enterprises "are an increasingly small part of the landscape that [now] features a wide variety of market and network [organizational] forms (p. 353). Langlois uses the metaphor of a vanishing hand to refer to this relatively recent stage of industrial capitalism. The change from visible hand to the vanishing hand stage, as Langlois admits, is not as dramatic as the change from invisible to visible hand (pp. 352-353).

In the vanishing hand economic stage, innovative technological change has often involved simplifying and reducing scale. This has been true in the electricity generation field where new technology resulted in reducing the minimum efficient scale of new electric capacity (p. 370). A similar pattern occurred with the rise of semiconductor technology in telephony (p.

371). In these and related businesses, new profit opportunities "could be seized only by breaking down or 'unbundling' the vertical structure of the managerial corporation" especially in regulated utilities (p. 371).

A particularly interesting development during the 1960s involved corporations that used their management skills to diversify excessively such as ITT did. These corporations became conglomerates as they assembled new divisions by purchasing firms or divisions of other firms that were unrelated to their existing businesses. This pattern was aided by innovations in the securities markets that made it easier to do leveraged buyouts (pp. 371-372). Because of the generally poor performance of the conglomerates, these acquisitions eventually contributed to deconglomeration as the acquired divisions were sold off. Such activities were in effect a return to corporate specialization, the strategic catchword of the 1990s (p. 372). Companies were advised to return to their "core competences" (p. 372). Vertical disintegration and specialization became "perhaps the most significant organizational development of the 1990s" (p. 373). Companies were "finding it profitable to outsource increasing amounts of the production process" both domestically and abroad (p. 373). This was especially true of pharmaceuticals who often outsourced manufacturing, marketing, and clinical trials. In semiconductor manufacturing, firms were retaining their design, development, and marketing functions but did not own their manufacturing plants (p. 373). US auto manufacturers modularized their product design and supply chain strategies and relied heavily on subcontractors (374). These subcontractors were specialists in a sense but generalists in other ways; they had general purpose technologies or capabilities. This kind of decentralization made sense when the companies could avoid the high costs of coordination (p. 374). Various other developments such as visible design rules contributed to this type of specialization. Dell computer company is notable for "selling PCs to order by assembling them like Lego from a set of standardized components" often produced by other firms (p. 375). Further, these developments benefited from the evolving "external capabilities of the entire economy" (p. 375). One interesting example of the competitive pattern of the time period involved the steps in the home mortgage lending process, originating, underwriting, holding, and servicing the loan; these steps were typically being undertaken by different organizations (p. 376).

There are many factors that have led businesses in the late 20th century and early 21st century to increasingly use markets to obtain needed inputs rather than obtaining these from internal organizational units. One of these factors is the growth of coordinating technologies such as personal computers and broadband communication networks that can lower the cost of coordination across markets relative to internal sourcing. In Langlois' (2003, p. 377) view, it was "not just by changes in coordination technology but also by changes in the extent of markets – increasing population and income but also by the globalization of markets." The latter no doubt was related to the "reduction of political barriers around the world" (p. 377). In the late 19th century, the growth of internal sourcing from internal units of the large multidivisional corporations led to greater division of labor leading to higher productivity. In contrast to this, in the mid to late 20th century, productive changes in the division of labor were advanced to a greater degree by enterprises that took advantage of increasingly efficient suppliers around the world (pp. 378-379).

The noneconomic motivations of decision makers

The previous three sections of the paper have characterized three important stages of American economic development, the invisible hand, the visible hand, and the vanishing hand stages. These characterizations have focused to a great degree on the evolution of important structural aspects of the economy such as the main features of markets, organizations, and management. Little has been said about the motivations of the important economic decision makers. Given the capitalist nature of the economy, one can confidently assume that the owners of capital (and other resources) have been motivated largely by the prospect of profits. This is true both for the owners of small market-oriented firms as well as the stockholders of large organizations which are directed by multilevel managerial hierarchies. The importance of the profit motive and how it functions would presumably depend on circumstances such as the role of the company's founder(s), the firm's need for external equity financing, and whether the firm was a subcontractor or a large corporation with a dominant market position.

This is not to say, however, that individual owners and stockholders have not had other important decision-making motivations. There are many notable, interesting cases in which owners have been strongly motivated by noneconomic considerations, albeit these seem to be the exception rather than the rule. Let's consider three interesting cases from the 1980s and 1990s in which the owners' noneconomic motivations and behavior have been documented in book form. The three books are: Tom Chappell's *The Soul of a Business* (1993), Anita Roddick's *Body and Soul* (1991), and Fred Lager's *Ben and Jerry's: The Inside Scoop* (1994).

The first book concerns the story of Tom Chappell (1993) who was the founder and president of Tom's of Maine, a company that has produced a variety of toothpastes made wholly from natural ingredients. A number of years after founding the company, Chappell found that his "everyday business life had gone stale; his work had become an unfulfilling exercise" (p. xi). In his search for meaning, he decided to enroll in divinity school in order "to get back in touch with his original sense of purpose… and commitment to creating good products" (p. xi). He wound up finding "answers in the writings of the great philosophers" (p. xii). He discovered that he "could actually find a way to manage for profit and for the common good" (p. xii). He was able not only to rediscover his business vision, but to connect to his core values and spirit. He learned from studying religions that he needed to balance the spiritual and the practical and that his leadership required both his head and heart (p. xiv). The result was that he was able to lead his company in a way that brought to bear his beliefs and values. He developed a business strategy and mission that drew on his deep ethical principles and soul (pp. xiv-xv).

Anita Roddick (1991) opened the first Body Shop store in 1976. The Body Shop is in the cosmetics business, also known as the beauty business. Roddick had a personal vision and passionate belief that her business could be fun if it were conducted with love, and that it could be a powerful force for good (p. 7). She understood that the beauty business has typically taken advantage of women, selling them "totally frivolous and worthless products … [and] failing to take into account [their] real needs" (p. 9). She understood that it is "immoral constantly to make women feel dissatisfied with their bodies… by making miracle claims for a [beauty] product" (p. 15). She was determined that her business would be guided by feminine principles as well as by qualities like love, care, and intuition (p. 17). Anita's company aspired to ennoble the spirit "by creating a sense of holism, of spiritual development, of feeling

connected to the workplace, the environment, and relationships" (p. 22). Commercial considerations were secondary for her. She aspired to have a business with a human face and a social conscience (p. 24). There is no doubt that The Body Shop has been a socially responsible company. Moreover, she wanted her company to be part of an enlightened capitalism (p. 27). A key to Body Shop's success was "balancing profit with principles" (p. 154). In line with this, an important part of Body Shop's operation was supporting community projects.

Ben and Jerry's ice cream company was started by Ben Cohen and Jerry Greenfield in Burlington, Vermont in 1978. In some ways, Ben and Jerry's business culture is similar to that of the Body Shop. Its style has been to bring some of the counter culture values of the 1960s to the business world (Lager 1994, p. xi). The company's unofficial motto was "If it's not fun, why do it?" Ben and Jerry

> "strongly believed that [their] business should give something back to the community that supports it. They started by giving away free ice cream and sponsoring local festivals, but as the company grew, their efforts became more ambitious, and Ben and Jerry's was soon recognized as one of the most progressive, socially active corporations in America" (p. 243).

Their mission statement clearly articulated their values. In Ben and Jerry's view, operating the company responsibly and joyfully was more important for them than making a profit.[1]

The transition from one economic stage to another

It is important to consider what happens when an economy makes a transition from one economic stage to the next. When the economy changes from one stage to another, the functioning of previous stage(s) do not disappear. The manifestations of previous stages just become less prominent and dominant. With the advent of the visible hand stage, coordination of the economy by markets (the invisible hand stage) did not cease to exist. What happened is that much more of economic coordination activity became taken over by large managerial enterprises. With the advent of the vanishing hand stage, large multi-divisional corporations did not disappear. They did, however, gradually become less dominant and prevalent. More of American economic activity was then taking place in more specialized companies, often ones that were supplied by a great variety of subcontractors rather than relying on supply from organizational units located within the vertically integrated enterprise.

What's next? An economy with a humanistic hand

Based on the preceding discussion of important economic stages in the history of American capitalism, it makes sense to inquire about the nature of the next economic stage. One could

[1] I have not studied the history of American business owners, and I cannot speak from knowledge or experience about owners who have been strongly motivated by noneconomic goals. My expectation is that there always has been a minority of owners who have been motivated by strong noneconomic convictions. But I don't expect that the particular noneconomic motivations of owners in the 18th century would be the same as those of owners in the 19th or 20th centuries. However, I do suspect there is a similarity in the way that some fraction of owners have had a passion for and wisdom related to their business' purpose that transcends pecuniary considerations.

presumably attempt to forecast the nature of the next stage based on an analysis of emerging trends. I will leave that approach to social scientists who have experience doing socio-economic forecasting. The approach utilized here is to develop a scenario for an economic stage that could be expected to lead to improved socio-economic performance for the American economy. My proposal for the next economic stage is one that would enable very substantial improvements in the kind of functioning that would provide remedies for the important ways in which the economy has been dysfunctional. Although the overall performance of the American economy has in many respects been dynamic and highly productive, its human performance has too often been unfavorable. In other words, there has been a high human cost associated with the American economy's typical functioning. The human economic casualties include the many people suffering from chronic ailments such as obesity and type 2 diabetes, not to mention cancer, heart disease, and many others. Too many suffer from poverty and addictions. Further, people both suffer from and have very serious concerns about environmental degradation. These human costs can usually be traced directly or indirectly to socio-economic dysfunctions. Market failure in one sense or another is usually involved.

Because the economy's functioning can usually be implicated in America's low human performance, and because there are an increasing number of people that are very concerned about these matters, my proposal is for a fourth economic stage known as the *humanistic hand*. The essence is that people participating in an economy in the humanistic hand stage would have strong aspirations for remedying the dysfunctions associated with the kind of modern capitalism that exists in contemporary America. In the humanistic hand stage, people would be focused on achieving the kind of socio-economic performance that can be expected to yield high human well-being. That is, they would be focused on finding and implementing remedies for many of the human costs associated with existing socio-economic dysfunctions (and market failures). It is beyond the scope of this paper to explain in detail how all the different dysfunctions would be remedied. However, the next section will explain how the thinking of Elinor Ostrom can provide a number of the essentials needed to illuminate the path toward an economy with a humanistic hand.

In what follows my plan is to start with a relatively brief explanation of Ostrom's institutional economic contribution in which she explains about the features of common pool resources that are associated with both successful and failed institutional functioning. Building on Ostrom's work, it is possible to gain important insights into what a modern capitalist economy needs to do to improve its human functioning without sacrificing economic functioning in the usual sense (output, productivity, technological innovation, etc.). The ideal is that an economy that reached the humanistic hand stage would experience much higher performance in mental and physical health, equality, social harmony, and related considerations than an economy at an earlier economic stage.

It should be noted that Ostrom's work applies most readily to small scale situations. To better understand how Ostrom's thinking can apply to large scale situations and at the societal level, I have drawn on David S. Wilson's interesting research that combines economics with evolutionary biology. Using insights from Ostrom and Wilson one should be able to understand how a socio-economy's humanistic handed functioning can be a substantial improvement over the functioning of earlier economic stages. Arguably, the American economy in the humanistic hand stage could be able to achieve substantially greater human performance than what is typically achieved by other modern capitalist economies in the world.

Elinor Ostrom and common pool resources

Elinor Ostrom's (1990; 2009) research focuses on common pool resources (CPR) and the dilemmas they have posed for their users and society. A CPR is a resource such as a fishing ground, an irrigation system, ground water, pasture land for grazing animals, etc. that jointly benefits a group of people (the users) but which provides diminished benefits to the users involved if each individual pursues his or her narrow self-interest without considering other users. The CPR has a definite capacity. The problem is that each individual user has an incentive to overuse the resource. As authors such as Garret Harden (1968) have pointed out, when each user single-mindedly and independently follows the incentives, that will cause depletion of the CPR's capacity, possibly creating a tragic overuse of the resource.

In the view of conventional economic theory, there are only two ways to deal with this overuse problem. The first is to have government impose rules and/or taxes forcing the self-interested individuals to refrain from the destructive overuse of the CPR. The second is to privatize the CPR, making it a private, marketable, excludable good (Ostrom 2009, p. 409). Ostrom and her colleagues recognize that this standard dichotomous way of understanding the options for dealing with CPRs is not adequate. They studied many CPRs around the world (see Ostrom 1990). They learned that the overharvesting can be eliminated or reduced by, for example, encouraging communication among the people in the user group, developing trust among them, thereby fostering cooperation among the group's members (Ostrom, 2009, p. 409). They further learned how CPR users can develop credible commitments among themselves in effect creating valuable social capital. What the researches came to appreciate was that the individuals and groups involved with a CPR are not hopelessly trapped; they can make fruitful efforts to organize and solve their social dilemmas (p. 416). It turns out that there are typically many elements of any CPR situation that can be modified. Ideas for such changes can come from individuals within the CPR who rely on self-reflection and creativity to develop novel patterns of interaction that restructure the interactions among the CPR's users (p. 417). Further, Ostrom's research found that groups that attempt to organize and effectively manage their CPR are most likely to succeed if they follow eight core design principles.

The eight core design principles are (Wilson, 2015, pp. 12-13; Ostrom, 2009, p. 422):

1. *Strong group identity and understanding of purpose*. The identity of the group, the shared resources, and the need to manage the resource must be clearly delineated.
2. *Proportional benefits and costs*. Members of the group must negotiate a system that rewards members for their contributions.
3. *Collective-choice arrangements.* Decision making should be by consensus or another process recognized to be fair.
4. *Monitoring*. To prevent free-riding and exploitation, monitoring should be used to detect violations.
5. *Graduated sanctions*. Transgressions need not require heavy-handed punishment, at least initially. More severe punishment can be waiting in the wings.
6. *Conflict recognition mechanisms*. Conflicts should be resolved quickly in ways perceived as fair.
7. *Minimal recognition of rights to organize*. Groups must have the authority to conduct their own affairs.
8. *For groups that are part of larger social systems, there must be appropriate coordination among relevant groups*.

Ostrom (2009, p. 419) found that "the capacity to overcome dilemmas and create effective governance occurred far more frequently than expected and depended upon the structure of the resource … and the rules-in-use developed by users." Further, the success of these CPR collaborations depended on the capability of boundedly rational individuals to acquire fully reliable information in situations where dependable feedback was present (p.430).

The findings of Ostrom's (2009) research leads to the following overall conclusion. Individuals in CPR groups who are faced with incentives to cheat at the expense of others can overcome these disincentives and learn to work together. They do this by talking face to face with each other, trusting each other, forging good cooperative human relations, and committing themselves to their common purposes. Further, they also need to face the facts and complexity of their situation and negotiate in good faith. What does this imply for public policy? The "core goal of public policy should be to facilitate the development of institutions that bring out the best in humans" (pp. 435-436). In Ostrom's view, well designed institutions can nudge individuals to behave successfully in CPRs and other challenging social dilemma situations (p. 435).

At the heart of what Ostrom and her colleagues have discovered is that determined, cooperative, purposeful individuals can produce better socio-economic outcomes, ones involving better functioning, than capitalist economies ordinarily give rise to. Of course, it should be noted that Ostrom's CPR research applies strictly to relatively small scale situations. However, there is reason to believe that similar favorable outcomes can be expected for much larger scale situations. Let's consider David Wilson's thinking on this subject.

Generalizing the principles of common pool resources

David Wilson is an evolutionary biologist. So it is not surprising that one of his research interests has been Darwin's natural selection theory relating to the evolution of all kinds of living organisms. More recently, Wilson's interests have turned toward the cultural evolution of human societies, particularly the kind of cultural evolution that can lead to improving socio-economies and, thereby, make the world and its societies a better place to live (Wilson 2015, p. 6).

For Wilson the term *functional organization* is a key concept that is very useful for understanding the positive role that cultural evolution can have with respect to socio-economic change. From a biological standpoint,

> "something is *functionally organized* when its parts work together in a coordinated fashion to achieve a given end. The organelles of a cell and the organs of a multicellular organism are miracles of functional organization designed by natural selection to enhance survival and production" (Wilson, 2015, p. 9).
>
> "When a group of organisms is functionally organized, its members coordinate their activities for a common purpose just like the organs of an organism and the parts of a can opener. A group that is extremely well organized could even be called a *superorganism*…" (p. 9).

Recall Ostrom's analysis of what happens when users of a CPR are able to be successful in organizing their CPR such that it overcomes the dysfunctional behavioral pattern(s) that otherwise would have caused the overuse and depletion of the CPR. Presumably, the result of such a CPR organizing activity is that the organization will then embody more of the features such as cooperative relationships that make it a functional organization. In the functionally organized CPR, all the members are coordinating their actions to achieve their common purpose. By overcoming the tendency to dysfunction, it has also become an organization whose members are working for the common good of society (Wilson, 2015, pp. 9, 29).

It should be noted that the negative tendencies of CPRs are very similar to the tendencies that exist in many ordinary competitive markets. In the latter, the market is likely to fail when its businesses behave in a purely self-serving way by, for example, using manipulation, deception, and trickery that cause consumers to pay too much for products they do not need (Tomer 2017, p. 77). Arguably, if instead these markets were functionally organized a la CPRs, consumers and businesses would be communicating and cooperating with each other. They would be coordinating their actions and presumably finding a common purpose. And the businesses would be attempting to behave in a socially responsible manner. That is, the businesses would be learning about the true preferences of their customers and attempting to supply goods that are really and truly right for them rather than seeking to gain at their expense (p. 89).

According to Wilson, Ostrom, and Cox (2013) (henceforth WOC), the core design principles associated with successful CPRs can be generalized so that they are useful in other contexts such as in improving the functioning of markets. WOC argue that these "principles have a wider range of application than CPR groups and are relevant to nearly any situation where people must cooperate and coordinate to achieve shared goals… The principles can be used as a practical guide for increasing the efficacy of groups" (p. S22).

Also according to WOC, groups and societies can benefit greatly by using the core design principles in situations where there are possibilities for some group members to opportunistically benefit at the expense of others (WOC, 2013, p. S26). These principles which are required to accomplish shared objectives "apply to most human groups whose members are not close relatives, proven friends, or coping with a dire emergency" (p. S26). One might think that such groups would be inclined to spontaneously adopt these principles, but that is not so (p. S27).

Education is one of the contexts that can benefit significantly by applying the core design principles because many classroom situations are deficient in the use of these principles (WOC, 2013, p. S27). There is evidence that educational applications of these principles "can have a transformative effect on classroom behavior" not only in the short-term but in the long-term (p. S27). The Sudbury Valley School in Massachusetts provides a radical educational success story. In this school which has a strong democratic and normative environment and which emphasizes self-motivated learning, the core design principles have contributed to an environment that prevents bullying and other harmful behaviors (p. S28).

Another important context that can benefit from the core principles is neighborhoods. Two case studies have examined use of the principles as a framework for improving the quality of neighborhoods. The first is the West Side Community Collaborative in Buffalo, New York. The second is a program called Design Your Own Park in Binghamton, New York. The first

focused on a neighborhood "characterized by substantial deterioration of the housing stock and the invasion of drug dealers, prostitutes, and substantial decline of property values" (WOC, 2013, p. S29). In this case, adopting most of the principles led to reversing the tragedy of the commons over a period of years. In the second case, the principles were used in connection with the opportunity to design and create a neighborhood park (p. S29). The groups working to create the park were coached to adopt the principles. Over time they had success in creating "a safe, secure and esthetically pleasing environment."

The upshot of these and other examples of applying the core design principles is that a generalization of the principles is possible because the principles "follow not only from political theory but from the evolutionary dynamics of cooperation in all species and the biocultural evolution of our own species" (WOC, 2013, p. S30).

"Our examination of a few case studies … only scratches the surface, but supports the notion that the core design principles can be generalized. In addition, an extensive literature on human social behavior in the laboratory can be related to the core design principles, if only in retrospect, and broadly supports their generality" (p. S30).

Toward a humanistic hand economy

If, as it seems, the American economy is performing relatively well in quite a few purely economic senses, but relatively poorly in that the socio-economy's dysfunctions are causing it to perform relatively poorly in a significant number of human senses, how can this situation be changed? That is, how is it possible to make a transition to the humanistic hand stage? The key to the answer is the understanding that a generalized version of the core principles that are useful for overcoming CPR dysfunctions can be useful for overcoming many of the economy's human related dysfunctions. Conceivably, appropriate application of the generalized principles can enable the economy to overcome its dysfunctions related to the problems of obesity, poverty, addictions, etc. To move in this direction, what is needed is a process of cultural evolution oriented to overcoming the human related dysfunctions. Cultural evolution, as Wilson (2015, pp. 19-21) explains, is like natural selection based on relative individual fitness except that it is instead based on "natural selection between groups." Cultural evolution involves much social experimentation, some of which involves conscious invention, and some of which is inadvertent (Wilson 2011, pp. 353-354). Some of the cultural evolution might be considered to be "guided mutation" (p. 354). As Wilson points out,

> "Life is complex, and our understanding is severely limited. At the end of the day, we need to try out multiple solutions, designing them as best we can, and select the ones that work based on a careful evaluation of their consequences. We need to manage the process of cultural evolution" (p. 354).

The cultural evolution process can lead to desired outcomes with the necessary scale; it is, however, not a sure thing like an engineered process. Some kind of cultural evolution is presumably always occurring but not necessarily the kind that will lead to the desired functional organization. Things can easily go wrong, and there are many unforeseen consequences (Wilson, 2011, p. 354). There always seems to be a substantial group of people that will oppose any good idea. The best that we can hope for are good leaders and a strong positive sense of purpose among the country's citizens. Nevertheless, knowing that

there are core principles that, if followed diligently, can bring about favorable human results is an important reason for optimism. There is good reason to believe that guided cultural evolution can bring about economy-wide functional organization. The goal of overcoming major socio-economic dysfunctions surely is one that many will find to be a source of strong motivation. The reward of a transition to a humanistic hand economy involving positive institutional transformation would no doubt be worth the costs.

Conclusions

It is interesting to note that a number of leading authors (namely Adam Smith, Alfred Chandler, and Richard Langlois) who had a great amount of understanding about the economic stage of the American economy that existed during the time that they were actively writing and researching utilized metaphors as a short hand way to refer to the key characteristics of the functioning of the economy at that time. This paper has provided relatively short explanations of the important economic stages that the metaphors (invisible hand, visible hand, and vanishing hand) refer to. No doubt there are economic historians who could identify other economic stages in the development of the American economy. Nevertheless, the stages identified in this paper seem particularly important, and they provide a useful entry point to thinking about the nature of what might be the next economic stage (human hand).

The later part of this paper has drawn on the research of Elinor Ostrom and David Wilson to explain about the qualities of functional organizations, the kinds of qualities that a humanistic hand stage would embody. Ostrom understood that groups typically become dysfunctional when group participants are overly self-interested, and therefore, seek to gain at the expense of others, ultimately causing the group to cease functioning and to fail. Ostrom and Wilson recognized that what is needed are functional organizations in which group participants communicate, trust each other, and become committed to common purposes. The humanistic hand economy is one in which the predominant economic organizations are functional organizations. There is strong reason to believe that in these functional organizations, the human costs of economic activity will be dramatically lower than is the case in the typical economic enterprises of American style modern capitalism. Arguably, because of its high level of functioning, its emphasis on noneconomic motivation, its low human costs, and its high overall level of well-being, we need a humanistic hand economy. Can we expect cultural evolution to produce a humanistic hand economy? The answer is not knowable at this point. However, there are some signs that at least a certain significant and growing fraction of the American population would favor a transition to an economy with a humanistic hand.

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Permanent fiscal deficits are desirable for the high income countries: a note[1]

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1. Budget deficits are a natural consequence of excess private savings

Despite the austerity efforts of fiscal authorities around the world, public financial balances almost always and everywhere end up in deficits. This is no accident: the fundamental macroeconomic identity states that the private sector's excess saving equals the net external lending plus the financial deficit of the public sector. Given the private sectors' preference for running positive excess savings (and the relative unimportance of external imbalances, at least in the longer run) the public sectors are likely to display fiscal deficits secularly.

The public financial deficit and private excess saving are two sides of the same coin. Abstracting from the external imbalances, the public sector financial deficit represents the net (i.e. not covered by taxes levied on the private sector) value of goods and services *acquired* by the public sector from the private sector. Thereby, the private sector earns additional *income* (equal to the net value of goods and services sold to the government). That additional private sector income is neither consumed nor invested. Thus, it constitutes the excess private saving.

The positive private excess saving emerges only because the private sector desires to hold such a debt (e.g. taking the form of stocks of government-issued fiat money). In a closed economy the *consolidated* net financial wealth of the private sector *must* consist, exclusively, of government debt. In such an economy, public debt *is* the financial wealth of the private sector, consolidated. The private sector would not supply the government with goods and services in exchange for the government debt (starting with the government-issued currency) should that debt be considered worthless.

The conventional wisdom underlying the fiscal policies in most high-income countries stresses the need to restrict public-sector financial deficits. This is particularly the case with the European Union. The EU Growth and Stability Pact "lays down the obligation for Member States to adhere to the medium-term objective for their budgetary positions of close to balance or in surplus".[2] The Fiscal Compact agreed upon by the majority of EU leaders is designed to strengthen "fiscal discipline" across the Euro area (and beyond). It also imposes the obligation to reduce public sector debt/GDP ratios. Given the sluggish pace of nominal GDP growth, that requirement actually imposes the obligation to run budgetary *surpluses*. Thus the taxation of the private sector (net of transfers to the same) should be persistently higher than the income earned by the private sector on sales of goods and services to the public sector. The private sector would then have to "bleed" for many years to come – for the sake of "sound public finances". (The "sound public finances" are deemed however indispensable for the long-term dynamism of the private sector itself).

[1] This note summarises the argument developed in the author's article "The private saving glut and the developed countries' government financial balance" (*Review of Keynesian Economics*, no. 1, 2019).
[2] See Council Regulation No. 1055/2005 amending the Growth and Stability Pact (EU 2005). The same requirement features in the more recent (2012) Fiscal Compact (Article 3, point 1(a)).

The active policy aiming at balanced public finances implies that the private sector's excess saving is not allowed to materialise (e.g. being "pre-emptively" taxed away). But this outcome is hard to achieve: the private sector is unlikely to completely part with an excess of saving over investment. In rather extreme cases the excess savings would all assume the form of reserves of government-issued cash – thus remaining positive after all. Of course, it is rather unreasonable to expect that under a confiscatory fiscal policy seeking to wipe out the potential excess private saving, the private sector would be induced to increase its investment (or even consumption) spending. In effect, a fiscal policy seeking active consolidation of public finances is doomed to fail. The fiscal deficit will not be eliminated even if the economy is forced into stagnation (or recession).

Conversely, the public sector's financial consolidation can be achieved quite automatically and painlessly whenever the private sector is inclined to expand its investment and/or consumption (thus also reducing excess saving) – as was commonly observed throughout the high-income countries over a couple of years prior to 2000 and (to a lesser extent) prior to 2007.

Admittedly, the developments culminating in the years 2000 and 2007 were "unsustainable". Much of the private investment went into risky (or speculative) activities (e.g. residential construction) that failed to pay off, leaving large segments of the private sector deeply indebted to other private sector segments. Similarly, the expanding private consumption was disproportionately driven by debt owed to other parts of the private sector (rather than being backed by rising wages and other regular household incomes). The *internal* private-sector debt/credit excesses were followed by the painful private sector "deleveraging" (or "balance-sheet recessions") characterised by depressed private investment, increased private saving (depressed consumption out of the disposable income) and – consequently – increased excess saving of the private sector (the latter equal to the increased public sector financial deficits) reaching its (local) peaks in 2003 and 2010.

2. Fiscal surpluses and "beggar-thy-neighbour" policies

It is worth noticing the fact that some OECD countries (Germany in the first place) have for quite some time run fiscal surpluses, not deficits. However, the fiscal surpluses of those countries appear to have been smaller in absolute numbers (usually by far) than the fiscal deficits of others. Also, the fiscal-surplus countries tend to run external surpluses *larger* than their excess private savings. Their external surpluses – essentially equal to the external *deficits* of the partner countries – contribute to the fiscal deficits in the latter. Fiscal deficits disappearing in some (growing) countries do not vanish without trace. Private excess saving in such countries must all come from a rising surplus against foreign countries. They must be reflected (even if not one-for-one) in *higher* fiscal deficits of the foreign countries.

Even if it were in the best long-term interest of the population majorities in each and all of the high-income countries to avoid large and persistent external imbalances (and rely instead on large and persistent fiscal deficits), it is only realistic to expect that in some countries the authorities will choose to behave opportunistically, resorting to beggar-thy-neighbour tactics. However, the reliance on external surpluses substituting domestic fiscal deficits cannot work globally (or for the developed countries collectively) or indefinitely. Sooner or later, growth led by high export surpluses must come to an end either on account of excessively high foreign debt accumulated by the net-importer countries and/or on account of the recurring

protectionist sentiments in the net importing countries. US President Trump's ideas about international trade do not come from nowhere. In either case the policy of basing domestic growth on the beggar-thy-neighbour tactics must end at some point – at least for sufficiently large countries. This policy may work indefinitely for Luxembourg, but not for Germany.
It seems legitimate to assume that in the longer run the (larger) high-income countries individually (and thus also collectively) will not be in a position to "export" their excess private savings in sufficient quantities. In the last instance, the excess private savings can only be absorbed by (and emerge with) properly accommodative and cooperative fiscal policies across a sufficiently large number of high-income countries.

For about 50 years the US public sector has had the privilege of being the principal "absorber" of private excess savings globally. Of course, the *global* private excess saving could materialise because the US has been running external deficits – thus supplying the external surplus countries with additional incomes (in the form of additional dollar balances representing the US *public debt*). The fact that the US dollar has been *the* prime reserve currency certainly makes the sustained US external deficits fairly easy to "finance". Under more balanced international trade other high-income countries could be expected to share the responsibility for absorbing (and generating) private excess savings by running properly accommodative fiscal policies themselves.

3. Private excess savings likely to increase in the future

Whether or not there will be a genuine reason to run such accommodative fiscal policies in the future depends on the tendencies with respect to private saving and private investment. As already suggested there are pretty good grounds to expect a continuation of the past tendencies: a further fall in the investment shares concomitant with the saving propensity rising (or stagnating at best).

The deep ("systemic") tendencies underlying the behaviour of private sector saving and investment are likely to strengthen in the future. In the high-income countries, it is difficult to envision either a decisive rise in the wage share or a decline in income inequality. If anything, the combined effects of progressing globalisation (outsourcing production to low-wage and low-tax countries) and technological change (expansion of "intelligent machines" which will reduce demand for human labour, including high-skill occupations) are likely to support falling investment shares and rising income inequality that increases saving rates. Excess private saving will then increase in tandem with increased income inequality.

Whether such private excess saving materialises will depend on the course taken by the fiscal policies in the high-income countries. With fiscal policies consistently hostile to deficit spending, the private sector would be unable to work out saving in excess of investment. In other words, the private sector's disposable income would not be allowed to rise. In effect, the falling (or stagnant) private saving would be driven to a level consistent with the falling (or stagnant) investment. Under such conditions the real output would remain stagnant, at best.

Alternatively, accommodative fiscal policies would support the private sectors excess savings via matching public financial deficits. The additional demand for private output (equal additional private sector income and equal private excess saving) would support real output growth. Growth driven by rising public debts might continue – as long as the private sectors remained desirous of newly issued public debts. Should, at some stage, the private sectors

become "satisfied" with the quantity of its financial wealth (in the form of public debts held) they might become unresponsive to the public demand for more privately-produced goods and services. At such a stage, deficit-spending fiscal policies would no longer be effective (and the fiscal balance would be automatically restored).

Should one be concerned with a prospect of the private sectors in the high-income countries being finally satiated with their financial wealth? That outcome is rather hard to imagine, and the empirical evidence (the experience of Japan) suggests there is still a long way to go.

In conclusion, should the past (and current) tendencies underlying private sectors' saving and investment continue, one must expect the emergence of large potential private excess saving across the high-income part of the global economy. If the fiscal policies attempt to prevent the materialisation of public sector deficits, real economic growth will likely come to a halt. In other words, continuing output growth of the high-income countries requires cooperative fiscal policies that support the private sectors with income injections financed by rising public debts. This conclusion is a version of the "classical" functional finance principle. However, in contrast to the latter, our conclusion is that public debt must grow more or less permanently – and not only in response to "cyclical" growth slowdowns or occasional recessions. Additionally, whereas the functional finance principle applied in any single country is likely to be impractical (on account of the complications posed by external trade, capital movements and exchange rates), internationally cooperative and accommodative fiscal policies precluding major external imbalances are likely to fare better in practice. Clearly, even if run cooperatively, large functional finance deficits would not be free of potential problems (and thus managing to mitigate the scale of external imbalances). Consideration of those problems goes beyond the scope of this note. In any case, it is worth remembering that if large fiscal deficits become problematic (e.g. when either the private sectors no longer consider the public debt or currency worthy of accumulation or when the mistaken views on the dangers of growing public debt prevail), growth will likely come to a standstill.

Fiscal deficits serving as permanent substitutes for dwindling (for whatever reason) private investment and stagnant private consumption can support continuing overall growth. However, the nature of the economy will undergo gradual evolution. While production (and profits) would remain private, the public sector would become an increasingly important "customer" of the private sector. The public sector would be commissioning from the latter growing supplies of goods and services (to be paid for with public debt). That offers an opportunity for meeting important social goals (e.g. with regard to environmental protection) which the private profit-oriented sector is not inclined to consider on its own.

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