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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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