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To *plurality*. The Association will encourage the free exploration of economic reality from any perspective that adds to the sum of our understanding. To this end it advocates plurality of thought, method and philosophy.

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Inside this issue:

In praise of Wesley Clair Mitchell, economist

By Merijn Knibbe

There are three main strands of Macro-Economic Measurement (MEM): National Accounting, the Flow of Funds and Business Cycle analysis. And one minor strand: the measurement of time poverty. This last strand is not yet macro but it does try to tack an estimated household production function to the national accounts data. Wesley Claire Mitchell, proud student of Thorstein Veblen and long-time head of the National Bureau of Economic Research is important for all of these and even stood at the cradle of two of them.

One pivotal publication of Mitchell is '*The backward art of spending money*' (Mitchell (1912)) which stresses the importance of the household and the difficult art of spending money wisely as well as the pivotal importance of the mistress of the house for the welfare of the household (and society). I'm not going into this discussion but to avoid misunderstandings: stressing the pivotal importance of the household and the housewife is feminist. This article is entirely consistent with the work of the Levy institute on 'time poverty'. It tries to estimate a kind of household production function and is based on the idea that "*The predominant framework for measuring poverty rests on an implicit assumption that everyone has enough time available to devote to household production or enough resources to compensate for deficits in household production by purchasing market substitutes. … Ajit Zacharias argues that this implicit bias in our official poverty statistics threatens to undermine the Sustainable Development Goals"*.

Another pivotal publication of Mitchell is '*The Role of Money in Economic Theory*' (Mitchell (1916)). In this article he praises Veblen, lambasts the Austrian school and people like Jevon and praises Alfred Marshall as Marshall tried to introduce real prices and real production into economic theory while the a-historical work of the 'marginalists' amounted to little more than fruitless speculation about the supposed psychological behavior of mankind. Mitchell put his money where his mouth was and in 1921 went on to publish, with King and Macaulay, the milestone '*Income in the United States: Its Amount and Distribution, 1909-1919*' which estimated time series of nominal and real income in the USA. Around 1920 there were quite some individual researchers working on this. In Bowley (1942) a very extensive literature overview can be found, subdivided by country. But this study (followed by much more detailed publications in 1922) sure was a step ahead and as such it was the foundation on which a student of Mitchell, Simon Kuznets, could work.

Next to this Mitchell worked on business cycles and published several works on this. Tjalling Koopmans criticized this as 'measurement without theory'. However, reading Koopmans nowadays gives one the impression of a statistician who had mastered multiple regression and thus wanted all economics articles to be stated in a 'multiple regression' format, but who had never heard of principle component analysis. Modern economists using principle component analysis like Andrle, Brůha and Solmaz are, contrary to Koopmans, very favorable about this kind work of Mitchell.

When it comes to the Flow of Funds we can cite from the annual NBER reports of 1944 and 1948 which assigned Morris Copeland with the task of establishing the Flow of Funds and Milton Friedman with the task of writing a monograph on money which would result in his 1963 Friedman and Schwarz 'A monetary history of the United States' book (much better than Friedman's work on permanent income and, as Rockoff argues, distinctly Mitchellian in approach):

In a 1945 NBER publication, *The National Bureau's First Quarter-Century*, on pp.61-62 Copeland's task (accomplished together with people from the Fed) was described as:

"*About the circuit flow of payments and its relation to national income and output, our knowledge is exceedingly vague. We do know, however, that the flow of payments does not adjust itself automatically to the flow of goods men are able to produce and need to consume. Indeed, several theorists have argued that cyclical fluctuations in business activity are due primarily to recurring changes in the relative size of these two flows. The findings this investigation promise should put us in a far better position to diagnose our recurrent chills and fevers, and to seek remedies*".

This present reason given by the British Office of National Statistics to integrate the Flow of Funds with the national accounts still neatly mirrors this assignment:

"*An understanding of the economic performance of the UK is especially important for effective policymaking and improving welfare. The non-financial accounts have long been extensively monitored as a health check for the economy, but they do not fully capture the build-up of financial risk. For instance, changes to the underlying resilience of the UK's source of funding can impact the economy in a way that is not obvious from studying fluctuations in income or output … We have partnered with the Bank of England to address this, by enhancing the coverage, quality and granularity of financial accounts statistics for the UK"* (ONS (2018)).

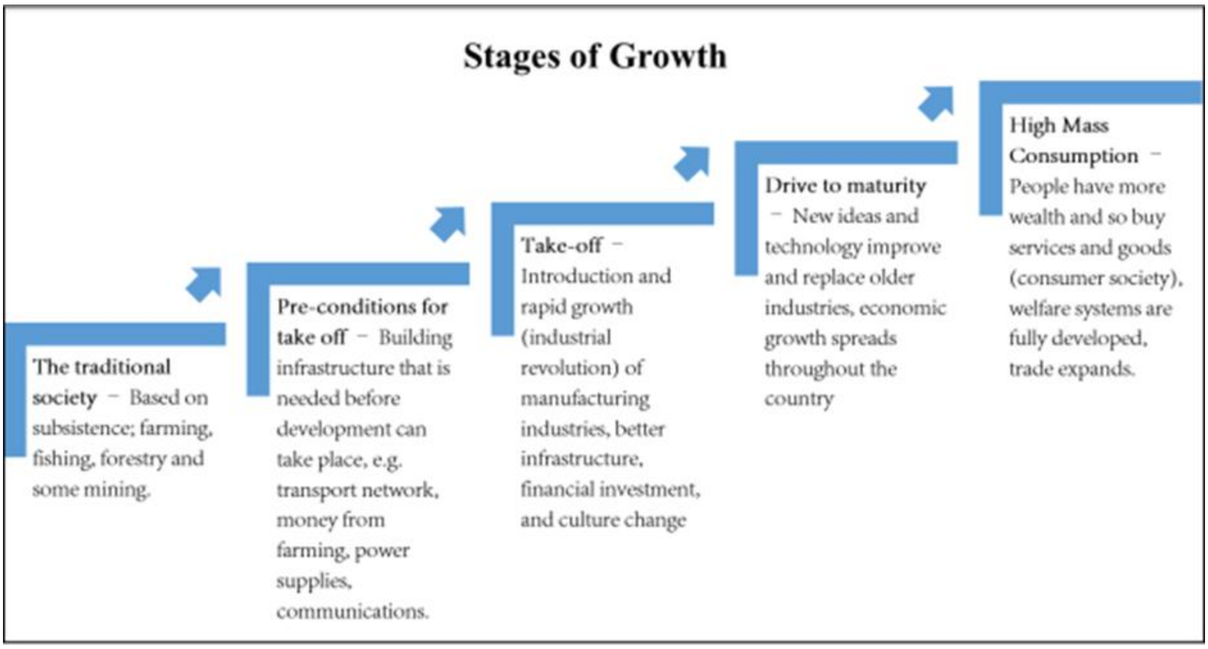
In the 1948 annual account of the NBER Friedman's task was described as:

"*Work on another monograph, dealing with the cyclical behavior of the money supply, its rate of turnover, and the condition of banks in different parts of the country as well as in the aggregate, will get actively under way this year. The study will go back to the Civil War, but will give special attention to developments in the sphere of money and banking since 1914 when the Federal Reserve System was instituted. Milton Friedman, Associate Professor of Economics at the University of Chicago, is in charge of the study*".

Friedman and Schwarz did not apply quadruple accounting methodology to their series and consequently, alas, did not manage to integrate their estimates of money with the Flow of Funds. This set the economics profession back by decades. Nowadays however, the Flow of Funds is the basis for the estimates of the amount of money as well as of flows of other money-like assets. They are also increasingly integrated with the National Accounts. While the work of Andrle, Brůha and Solmaz shows how methods for business cycle analysis can be applied to such integrated data. Data on the household production function as estimated by the authors of the studies on 'time poverty' can be added to this, to provide an overview of household prosperity consistent with the national accounts. It would not have surprised Mitchell. On the contrary. He would be amazed that this is not yet accomplished!

Utopia and Economic Development

By David Ruccio



From the very beginning, the area of mainstream economics devoted to Third World development has been imbued with a utopian impulse. The basic idea has been that traditional societies need to be transformed in order to pass through the various stages of growth and, if successful, they will eventually climb the ladder of progress and achieve modern economic and social development.

Perhaps the most famous theory of the stages of growth was elaborated by Walt Whitman Rostow in 1960, as an answer to the following questions:

Under what impulses did traditional, agricultural societies begin the process of their modernization? When and how did regular growth become a built-in feature of each society? What forces drove the process of sustained growth along and determined its contours? What common social and political features of the growth process may be discerned at each stage? What forces have determined relations between the more developed and less developed areas?

Rostow's model postulated that economic growth occurs in a linear path through five basic stages, of varying length—from traditional society through take-off and finally into a mature stage of high mass consumption.

While Rostow's model and much of mainstream development theory can trace its origins back to Adam Smith—through the emphasis on increasing productivity, the expansion of markets, and the definition of development as the growth in national income—the development models that were prevalent in the immediate postwar period presumed that the pre-conditions growth were not automatic, but would have to be engineered through government intervention and foreign aid.

Mainstream modernization theory was created in the 1950s—and thus after the first Great Depression and World War II, when world trade had been severely disrupted, and in the midst of decolonization and the rise of the Cold War, when socialism and communism were attractive alternatives to many of the national liberation movements in the Global South. It was a determined effort, on the part of academics and policymakers in the United States and Western Europe, to showcase capitalist development and make the economic and social changes necessary in the West's former colonies to initiate the transition to modern economic growth.*

The presumption was that government intervention was required to disrupt the economic and social institutions of so-called traditional society, in order to chart a path through the necessary steps to shift the balance from agriculture to industry, create national markets, build the appropriate physical and social infrastructure, generate a domestic entrepreneurial class, and eventually raise the level of investment and employ modern technologies to increase productivity in both rural and urban areas.

That was the time of the Big Push, Unbalanced Growth, and Import-Substitution Industrialization. Only later, during the 1980s, was development economics transformed by the successful pushback from the neoclassical wing of mainstream economics and free-market policymakers. The new orthodoxy, often referred to as the Washington Consensus, focused on privatizing public enterprises, eliminating government regulations, and the freeing-up of trade and capital flows.

Throughout the postwar period, mirroring the debates in mainstream microeconomic and macroeconomic theory, mainstream development theory has oscillated back and forth—within and across countries—between more public, government-oriented and more private, free-market forms of mainstream development theory and policy. And, of course, the ever-shifting middle ground. In fact, the latest fads within mainstream development theory combine an interest in government programs with micro-level decision-making. One of them focuses on local experiments—using either the randomized-control-trials approach elaborated by Abhijit Banerjee and Esther Duflo or the Millenium Villages Project pioneered by Jeffrey Sachs, which they use to test and implement strategies so that impoverished people in the Third World can find their own way out of poverty. The other is the discovery of the importance of "good" institutions—for example, by Daron Acemoglu—especially the delineation and defense of private-property rights, so that Rostow's modern entrepreneurs can, with public guarantees but minimal interference otherwise, be allowed to keep and utilize the proceeds of their private investments.

The debates among and between the various views within mainstream development economics have, of course, been intense. But underlying their sharp theoretical and policy-related differences has been a shared utopianism based on the idea that modern economic development is equivalent to and can be achieved as a result of the expansion of markets, the creation of a well-defined system of private property rights, and the growth of national income. In the end, it is the same utopianism that is both the premise and promise of a long line of contributions, from Smith's *Wealth of Nations*through Rostow's stages of growth to the experiments and institutions of today's mainstream development economists.

The alternatives to mainstream development also have a utopian horizon, which is grounded in a ruthless criticism of the theory and practice of the "development industry."

One part of that critique, pioneered by among others Arturo Escobar (e.g., in his *Encountering Development*), has taken on the whole edifice of western ideas that supported development, which he and other post-development thinkers and practitioners regard as a contradiction in terms.** For them, development has amounted to little more than the West's convenient "discovery" of poverty in the third world for the purposes of reasserting its moral and cultural superiority in supposedly post-colonial times. Their view is that development has been, unavoidably, both an ideological export (something Rostow would willingly have admitted) and a simultaneous act of economic and cultural imperialism (a claim Rostow rejected). With its highly technocratic language and forthright deployment of particular norms and value judgements, it has also been a form of cultural imperialism that poor countries have had little means of declining politely. That has been true even as the development industry claimed to be improving on past practice—as it has moved from anti-poverty and pro-growth to pro-poor and basic human needs approaches. It continued to fall into the serious trap of imposing a linear, western modernizing agenda on others. For post-development thinkers the alternative to mainstream development emerges from creating space for "local agency" to assert itself. In practice, this has meant encouraging local communities and traditions rooted in local identities to address their own problems and criticizing any existing distortions—both economic and political, national as well as international—that limit peoples' ability to imagine and create diverse paths of development.

The second moment of that critique challenges the notion—held by mainstream economists and often shared by post-development thinkers—that capitalism is the centered and centering essence of Third World development.

Moreover, such a "capitalocentric" vision of the economy has served to weaken or limit a radical rethinking of and beyond development.*** One way out of this dilemma is to recognize class diversity and the specificity of economic practices that coexist in the Third World and to show how modernization interventions have, themselves, created a variety of noncapitalist (as well as capitalist) class structures, thereby adding to the diversity of the economic landscape rather than reducing it to homogeneity. This is a discursive strategy aimed at rereading the economy outside the hold of capitalocentrism. The second strategy opens up the economy to new possibilities by theorizing a range of different and potential connections among and between diverse class processes. This forms part of a political project that can perhaps articulate with both old and new social movements in order to create new subjectivities and forge new economic and social futures in the Third World.

The combination of post-development and class-based anti-capitalocentric thinking refuses the utopianism of Third World development, as it constitutes a different utopian horizon—a critique of the naturalizing and normalizing strategies that are central to mainstream development theory and practice in the world today. It therefore leads in a radically different direction: to make noncapitalist class processes and projects more visible, less "unrealistic," as one step toward dethroning the "development industry" and invigorating an economic politics beyond development.

*At the same time, the Western Powers attempted to reconstruct the global institutions of capitalism, through the triumvirate of the World Bank, the International Monetary Fund, and the General Agreement on Tariffs and Trade (predecessor to the World Trade Organization) that was initially hammered out in 1944 in the Bretton-Woods Agreement.

**A short reading list for the post-development critique of mainstream development includes the following: Wolfgang Sachs, ed., *The Development Dictionary: A Guide to Knowledge As Power* (Zed, 1992); Arturo Escobar, *Encountering Development: The Making and Unmaking of the Third World* (Princeton, 1995); Gustavo Esteva et al., *The Future of Development: A Radical Manifesto* (Policy, 2013); and the recent special issue of *Third World Quarterly* (2017), "*The Development Dictionary* @25: Post-Development and Its Consequences."

***Building on a feminist definition of phallocentrism, I along with J.K. Gibson-Graham (in "'After' Development: Reimagining Economy and Class," an essay published in my *Development and Globalization: A Marxian Class Analysis*) identify capitalocentrism whenever noncapitalism is reduced to and seen merely as the same as, the opposite of, the complement to, or located inside capitalism itself.

The main reason why almost all econometric models are wrong

By Lars Syll

Since econometrics does not content itself with only making optimal predictions, but also aspires to explain things in terms of causes and effects, econometricians need loads of assumptions — most important of these are additivity and linearity. Important, simply because if they are not true, your model is invalid and descriptively incorrect. And when the model is wrong — well, then it is wrong.

Limiting model assumptions in economic 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 *a fortiori* only are of limited value to our understanding, explanations or predictions of real economic systems.

Our admiration for technical virtuosity should not blind us to the fact that we have to have a cautious attitude towards probabilistic inferences in economic contexts. We should look out for causal relations, but econometrics can never be more than a starting point in that endeavour since econometric (statistical) explanations are not explanations in terms of mechanisms, powers, capacities or causes. Firmly stuck in an empiricist tradition, econometrics is only concerned with the measurable aspects of reality. But there is always the possibility that there are other variables – of vital importance and although perhaps unobservable and non-additive, not necessarily epistemologically inaccessible – that were not considered for the model. Those which were can hence never be guaranteed to be more than potential causes, and not real causes. A rigorous application of econometric methods in economics really presupposes that the phenomena of our real world economies are ruled by stable causal relations between variables. A perusal of the leading econom(etr)ic journals shows that most econometricians still concentrate on fixed parameter models and that parameter-values estimated in specific spatio-temporal contexts are presupposed to be exportable to totally different contexts. To warrant this assumption one, however, has to convincingly establish that the targeted acting causes are stable and invariant so that they maintain their parametric status after the bridging. The endemic lack of predictive success of the econometric project indicates that this hope of finding fixed parameters is a hope for which there really is no other ground than hope itself.

Real-world social systems are not governed by stable causal mechanisms or capacities. The kinds of 'laws' and relations that econometrics has established, are laws and relations about entities in models that presuppose causal mechanisms being atomistic and additive. When causal mechanisms operate in real-world systems they 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 it (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.

Even in statistics, the researcher has many degrees of freedom. In statistics — as in economics and econometrics — the results we get depend on the assumptions we make in our models. Changing those assumptions — playing a more important role than the data we feed into our models — leads to far-reaching changes in our conclusions. Using statistics is no guarantee we get at any 'objective truth.'

On the limits of 'statistical causality'

Causality in social sciences — and economics — can never solely be a question of statistical inference. Causality entails more than predictability, and to really in depth explain social phenomena requires theory. Analysis of variation — the foundation of all econometrics — can never in itself reveal how these variations are brought about. First, when we are able to tie actions, processes or structures to the statistical relations detected, can we say that we are getting at relevant explanations of causation?

Most facts have many different, possible, alternative explanations, but we want to find the best of all contrastive (since all real explanation takes place relative to a set of alternatives) explanations. So which is the best explanation? Many scientists, influenced by statistical reasoning, think that the likeliest explanation is the best explanation. But the likelihood of x is not in itself a strong argument for thinking it explains y. I would rather argue that what makes one explanation better than another are things like aiming for and finding powerful, deep, causal, features and mechanisms that we have warranted and justified reasons to believe in. Statistical — especially the variety based on a Bayesian epistemology — reasoning generally has no room for these kinds of explanatory considerations. The only thing that matters is the probabilistic relation between evidence and hypothesis. That is also one of the main reasons I find abduction — inference to the best explanation — a better description and account of what constitute actual scientific reasoning and inferences.

> *In the social sciences … regression is used to discover relationships or to disentangle cause and effect. However, investigators have only vague ideas as to the relevant variables and their causal order … I see no cases in which regression equations, let alone the more complex methods, have succeeded as engines for discovering causal relationships.*
>
> David Freedman (1997:60)

Since statisticians and econometricians have not been able to convincingly warrant their assumptions of homogeneity, stability, invariance, independence, additivity as being ontologically isomorphic to real-world economic systems, there are still strong reasons to be critical of the econometric project. There are deep epistemological and ontological problems of applying statistical methods to a basically unpredictable, uncertain, complex, unstable, interdependent, and ever-changing social reality. Methods designed to analyze repeated sampling in controlled experiments under fixed conditions are not easily extended to an organic and non-atomistic world where time and history play decisive roles.

> *If contributions made by statisticians to the understanding of causation are to be taken over with advantage in any specific field of inquiry, then what is crucial is that the right relationship should exist between statistical and subject-matter concerns … The idea of causation as consequential manipulation is apt to research that can be undertaken primarily through experimental methods … However, the extension of the manipulative approach into sociology would not appear promising, other than in rather special circumstances … The more fundamental difficulty is that under the — highly anthropocentric — principle of 'no causation without manipulation,' the recognition that can be given to the action of individuals as having causal force is in fact peculiarly limited.*
>
> John H Goldthorpe (2000:159)

Why statistics and econometrics are not very helpful for understanding economies

As social researchers, we should never equate science with mathematics and statistical calculation. All science entail human judgement, and using mathematical and statistical models do not relieve us of that necessity. They are no substitutes for thinking and doing real science.

Most work in econometrics is made on the assumption that the researcher has a theoretical model that is 'true.' But — to think that we are being able to construct a model where all relevant variables are included and correctly specify the functional relationships that exist between them, is not only a belief without support, it is a belief *impossible* to support.

The theories we work with when building our econometric regression models are insufficient. No matter what we study, there are always some variables missing, and we do not know the correct way to functionally specify the relationships between the variables.

Every econometric model constructed is miss-specified. There is always an endless list of possible variables to include, and endless possible ways to specify the relationships between them. So every applied econometrician comes up with his own specification and 'parameter' estimates. The econometric Holy Grail of consistent and stable parameter-values is nothing but a dream.

The theoretical conditions that have to be fulfilled for econometrics to really work are nowhere even closely met in reality. Making outlandish statistical assumptions do not provide a solid ground for doing relevant social science and economics. Although econometrics has become the most used quantitative method in economics today, it is still a fact that the inferences made are as a rule invalid.

Econometrics is basically a deductive method. Given the assumptions, it delivers deductive inferences. The problem, of course, is that we will never completely know when the assumptions are right. Conclusions can only be as certain as their premises — and that also applies to econometrics.

On randomness and probability

Modern mainstream economics relies to a large degree on the notion of probability. To at all be amenable to applied economic analysis, economic observations have to be conceived as random events that are analyzable within a probabilistic framework. But is it really necessary to model the economic system as a system where randomness can only be analyzed and understood when based on an *a priori* notion of probability?

When attempting to convince us of the necessity of founding empirical economic analysis on probability models, neoclassical economics actually forces us to (implicitly) interpret events as random variables generated by an underlying probability density function.

This is at odds with reality. Randomness obviously is a fact of the real world. Probability, on the other hand, attaches (if at all) to the world via intellectually constructed models, and *a fortiori* is only a fact of a probability generating (nomological) machine or a well-constructed experimental arrangement or 'chance set-up.'

Just as there is no such thing as a 'free lunch,' there is no such thing as a 'free probability.'

To be able at all to talk about probabilities, you have to specify a model. If there is no chance set-up or model that generates the probabilistic outcomes or events – in statistics one refers to any process where you observe or measure as an experiment (rolling a die) and the results obtained as the *outcomes* or *events* (number of points rolled with the die, being e. g. 3 or 5) of the experiment – there strictly seen is no event at all.

Probability is a relational element. It always must come with a specification of the model from which it is calculated. And then to be of any empirical scientific value it has to be *shown* to coincide with (or at least converge to) real data generating processes or structures – something seldom or never done.

And this is the basic problem with economic data. If you have a fair roulette-wheel, you can arguably specify probabilities and probability density distributions. But how do you conceive of the analogous nomological machines for prices, gross domestic product, income distribution etc? Only by a leap of faith. And that does not suffice. You have to come up with some really good arguments if you want to persuade people into believing in the existence of socio-economic structures that generate data with characteristics conceivable as stochastic events portrayed by probabilistic density distributions.

We simply have to admit that the socio-economic states of nature that we talk of in most social sciences – and certainly in economics – are not amenable to analyze as probabilities, simply because in the real world open systems there are no probabilities to be had!

The processes that generate socio-economic data in the real world cannot just be assumed to always be adequately captured by a probability measure. And, so, it cannot be maintained that it even should be mandatory to treat observations and data – whether cross-section, time series or panel data – as events generated by some probability model. The important activities of most economic agents do not usually include throwing dice or spinning roulette-wheels. Data generating processes – at least outside of nomological machines like dice and roulette-wheels – are not self-evidently best modelled with probability measures.

When economists and econometricians – often uncritically and without arguments — simply assume that one can apply probability distributions from statistical theory on their own area of research, they are really skating on thin ice. If you cannot show that data satisfies *all* the conditions of the probabilistic nomological machine, then the statistical inferences made in mainstream economics lack sound foundations.

Statistical — and econometric — patterns should never be seen as anything other than possible clues to follow. Behind observable data, there are real structures and mechanisms operating, things that are -- if we really want to understand, explain and (possibly) predict things in the real world -- more important to get hold of than to simply correlate and regress observable variables.

Statistics cannot establish the truth value of a fact. Never has. Never will.

Sometimes we do not know because we cannot know

To understand real world 'non-routine' decisions and unforeseeable changes in behaviour, ergodic probability distributions are of no avail. In a world full of genuine uncertainty – where real historical time rules the roost – the probabilities that ruled the past are not those that will rule the future.

Time is what prevents everything from happening at once. To simply assume that economic processes are ergodic and concentrate on ensemble averages – and *a fortiori* in any relevant sense timeless – is not a sensible way for dealing with the kind of genuine uncertainty that permeates open systems such as economies.

When you assume the economic processes to be ergodic, ensemble and time averages are identical. Let me give an example: Assume we have a market with an asset priced at 100 €. Then imagine the price first goes up by 50% and then later falls by 50%. The ensemble average for this asset would be 100 € -- because we here envision two parallel universes (markets) where the asset-price falls in one universe (market) with 50% to 50 €, and in another universe (market) it goes up with 50% to 150 €, giving an average of 100 € ((150+50)/2). The time average for this asset would be 75 € – because we here envision one universe (market) where the asset-price first rises by 50% to 150 €, and then falls by 50% to 75 € (0.5*150).

From the ensemble perspective nothing really, on average, happens. From the time perspective lots of things really, on average, happen.

Assuming ergodicity there would have been no difference at all. What is important with the fact that real social and economic processes are nonergodic is the fact that uncertainty – not risk – rules the roost. That was something both Keynes and Knight basically said in their 1921 books. Thinking about uncertainty in terms of 'rational expectations' and 'ensemble averages' has had seriously bad repercussions on the financial system.

Knight's uncertainty concept has an epistemological founding and Keynes' definitely an ontological founding. Of course, this also has repercussions on the issue of ergodicity in a strict methodological and mathematical-statistical sense.

The most interesting and far-reaching difference between the epistemological and the ontological view is that if one subscribes to the former -- Knightian – view, you open up for the mistaken belief that with better information and greater computer-power we somehow should always be able to calculate probabilities and describe the world as an ergodic universe. As Keynes convincingly argued, that is ontologically just not possible.

To Keynes, the source of uncertainty was in the nature of the real — nonergodic — world. It had to do, not only — or primarily — with the epistemological fact of us not knowing the things that today are unknown, but rather with the much deeper and far-reaching ontological fact that there often is no firm basis on which we can form quantifiable probabilities and expectations at all.

Sometimes we *do not* know because we *cannot* know.

Keynes' critique of econometrics — still valid after all these years

To apply statistical and mathematical methods to the real-world economy, the econometrician, as we have seen, has to make some quite strong assumptions. In a review of Tinbergen's econometric work — published in *The Economic Journal* in 1939 — John Maynard Keynes gave a comprehensive critique of Tinbergen's work, focusing on the limiting and unreal character of the assumptions that econometric analyzes build on:

(1) Completeness: Where Tinbergen attempts to specify and quantify which different factors influence the business cycle, Keynes maintains there has to be a complete list of *all* the relevant factors to avoid misspecification and spurious causal claims. Usually, this problem is 'solved' by econometricians assuming that they somehow have a 'correct' model specification. Keynes (1940:155) is, to put it mildly, unconvinced:

> *It will be remembered that the seventy translators of the Septuagint were shut up in seventy separate rooms with the Hebrew text and brought out with them, when they emerged, seventy identical translations. Would the same miracle be vouchsafed if seventy multiple correlators were shut up with the same statistical material? And anyhow, I suppose, if each had a different economist perched on his a priori, that would make a difference to the outcome.*

(2) Homogeneity: To make inductive inferences possible — and being able to apply econometrics — the system we try to analyze has to have a large degree of 'homogeneity.' According to Keynes most social and economic systems — especially from the perspective of real historical time — lack that 'homogeneity.' It is not always possible to take repeated samples from a fixed population when we were analyzing real-world economies. In many cases, there simply are no reasons at all to assume the samples to be homogenous.

*(3) Stability***:** Tinbergen assumes there is a stable spatio

-temporal relationship between the variables his econometric models analyze. But Keynes argued that it was not really possible to make inductive generalisations based on correlations in one sample. As later studies of 'regime shifts' and 'structural breaks' have shown us, it is exceedingly difficult to find and establish the existence of stable econometric parameters for anything but rather short time series.

(4) Measurability: Tinbergen's model assumes that all relevant factors are measurable. Keynes questions if it is possible to adequately quantify and measure things like expectations and political and psychological factors. And more than anything, he questioned — both on epistemological and ontological grounds — that it was always and everywhere possible to measure real-world uncertainty with the help of probabilistic risk measures. Thinking otherwise can, as Keynes wrote, 'only lead to error and delusion.'

(5) Independence: Tinbergen assumes that the variables he treats are independent (still a standard assumption in econometrics). Keynes argues that in such a complex, organic and evolutionary system as an economy, independence is a deeply unrealistic assumption to make. Building econometric models from that kind of simplistic and unrealistic assumptions risk producing nothing but spurious correlations and causalities. Real-world economies are organic systems for which the statistical methods used in econometrics are ill-suited, or even, strictly seen, inapplicable. Mechanical probabilistic models have little leverage when applied to non-atomic evolving organic systems — such as economies.

Building econometric models can't be a goal in itself. Good econometric models are means that make it possible for us to infer things about the real-world systems they 'represent.' If we cannot show that the mechanisms or causes that we isolate and handle in our econometric models are 'exportable' to the real-world, they are of limited value to our understanding, explanations or predictions of real-world economic systems.

(6) Linearity: To make his models tractable, Tinbergen assumes the relationships between the variables he study to be linear. This is still standard procedure today, but as Keynes (1939:564) writes:

> *It is a very drastic and usually improbable postulate to suppose that all economic forces are of this character, producing independent changes in the phenomenon under investigation which are directly proportional to the changes in themselves; indeed, it is ridiculous.*

To Keynes, it was a 'fallacy of reification' to assume that all quantities are additive (an assumption closely linked to independence and linearity).

Econometric modelling should never be a substitute for thinking. From that perspective, it is really depressing to see how much of Keynes' critique of the pioneering econometrics is still relevant today.

The limits of probabilistic reasoning

Probabilistic reasoning in science — especially Bayesianism — reduces questions of rationality to questions of internal consistency (coherence) of beliefs, but, even granted this questionable reductionism, it is not self-evident that rational agents really have to be probabilistically consistent. There is no strong warrant for believing so. Rather, there is strong evidence for us encountering huge problems if we let probabilistic reasoning become the dominant method for doing research in social sciences on problems that involve risk and uncertainty.

In many of the situations that are relevant to economics, one could argue that there is simply not enough of adequate and relevant information to ground beliefs of a probabilistic kind and that in those situations it is not possible, in any relevant way, to represent an individual's beliefs in a single probability measure.

Say you have come to learn (based on own experience and tons of data) that the probability of you becoming unemployed in Sweden is 10%. Having moved to another country (where you have no own experience and no data) you have no information on unemployment and a fortiori nothing to help you construct any probability estimate on. A Bayesian would, however, argue that you would have to assign probabilities to the mutually exclusive alternative outcomes and that these have to add up to 1 if you are rational. That is, in this case – and based on symmetry – a rational individual would have to assign probability 10% to become unemployed and 90% to become employed.

That feels intuitively wrong though, and I guess most people would agree. Bayesianism cannot distinguish between symmetry-based probabilities from information and symmetry-based probabilities from an absence of information. In these kinds of situations, most of us would rather say that it is simply irrational to be a Bayesian and better instead to admit that we 'simply do not know' or that we feel ambiguous and undecided. Arbitrary and ungrounded probability claims are more irrational than being undecided in the face of genuine uncertainty, so if there is not sufficient information to ground a probability distribution it is better to acknowledge that simpliciter, rather than pretending to possess a certitude that we simply do not possess.

We live in a world permeated by unmeasurable uncertainty – not quantifiable stochastic risk – which often forces us to make decisions based on anything but rational expectations. Sometimes we 'simply do not know.' According to Bayesian economists, expectations tend to be distributed as predicted by theory.' I rather think, as did Keynes, that we base our expectations on the confidence or 'weight' we put on different events and alternatives. Expectations are a question of weighing probabilities by 'degrees of belief,' beliefs that have preciously little to do with the kind of stochastic probabilistic calculations made by the rational agents modelled by probabilistically reasoning Bayesian economists.

We always have to remember that economics and statistics are two quite different things, and as long as economists cannot identify their statistical theories with real-world phenomena there is no real warrant for taking their statistical inferences seriously.

If you have a fair roulette-wheel, you can arguably specify probabilities and probability density distributions. But how do you conceive of the analogous 'nomological machines' for prices, gross domestic product, income distribution etc? Only by a leap of faith. And that does not suffice in science. You have to come up with some really good arguments if you want to persuade people to believe in the existence of socio-economic structures that generate data with characteristics conceivable as stochastic events portrayed by probabilistic density distributions! Not doing that, you simply conflate statistical and economic inferences.

The present 'machine learning' and 'big data' hype shows that many social scientists — falsely — think that they can get away with analysing real-world phenomena without any (commitment to) theory. But — data never speaks for itself. Without a prior statistical set-up, there actually are no data at all to process. And — using a machine learning algorithm will only produce what you are looking for. Theory matters.

Some economists using statistical methods think that algorithmic formalisms somehow give them access to causality. That is, however, simply not true. Assuming 'convenient' things like 'faithfulness' or 'stability' is to assume what has to be proven. Deductive-axiomatic methods used in statistics do no produce evidence for causal inferences. The real causality we are searching for is the one existing in the real world around us. If there is no warranted connection between axiomatically derived statistical theorems and the real-world, well, then we have not really obtained the causation we are looking for.

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