# SOME OBSERVATIONS ON THE ERGODICITY OF ECONOMIC PROCESSES

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The paper reflects on the role of ergodicity in economics. The application of ergodicity in economics may be problematic, since economic processes are assumed to be more forecastable and stable than in the reality. As such, the theory of ergodicity may conclude to false practical conclusion.

*JEL codes:* A00, A10, A11

*Keywords:* ergodicity, forecastable, law of large numbers (LLN), Brexit

A recent paper by Iván Bélyácz entitled *"The Debated Role of Ergodicity in (Financial) Economics"*[1] raises numerous very important and fundamental questions. The most important observation in the study is that economic processes are not necessarily ergodic, so that the general statistical models applied in economics, often imported from other areas of science, cannot be used. As the study notes, experience shows that we always observe stochastic processes, and these processes always have two aspects. On the one hand there is the time aspect, when we examine the trajectories of a process, and on the other hand what we might term the random aspect, when we look at the possible random values at a fixed point in time. Using the terminology of statistics, we might thus speak of time series and cross-sectional data. In the case of both aspects, the question arises of whether there is an average, and if so what is its relevance. If we look at the process at a fixed point of time, then we are looking at a random variable and as such the law of large numbers ensures the existence of the average. If, however, we look at the process over time and we are looking at a current realization, then we are looking at the time average. In this case, if the time average exists, then we are dealing with ergodicity. In other words, in the case of a $X(T,\omega)$ stochastic process, if the time average $\lim_{T\to\infty} X(T,\omega)/T$ exists in some sense for every $\omega$, then we are dealing with an ergodic process.

Of course, ergodicity, like many other concepts in economics, was imported into economic thinking from the field of physics. The original goal of proving ergodicity was to explain the existence of macroscopically observable notions such as pressure or temperature; in other words, to explain how the chaotic behaviour of an essentially infinite number of particles existing in the microscopic world results in the stable values observable in the macroscopic world.

1 Iván Bélyácz (2017): The Debated Role of Ergodicity in (Financial) Economics. *Economy & Finance*, 4 (1), pp. 4–57.

Naturally the transplanting of this train of thought to economics is problematic indeed, since neither the essentially infinite number of individuals nor their essentially identical weight – i.e. the homogeneity of individuals – can be regarded as an economically realistic assumption. As the study emphasizes, this average is not to be confused with the separate $\mathbf{E}(X(t))$ expected value at each $t$ point in time, which by its nature is a weighted average according to the $\omega$ variable where the weights are provided by the corresponding probabilities. An expected value can only be taken according to a probability variable, so that the ergodic average cannot be regarded as an expected value. It follows from this that analysis of the ergodicity of a process is an important and relevant question. Numerous problems arising in practice are in reality connected not to the expected value, but to the ergodic average. While this distinction is very important and precise, I do not believe it expedient to separate the two questions too much. On the one hand, in actual statistical practice, the two kinds of average are often indistinguishable. Most frequently, it is assumed during the examination that the process is stationary, i.e. that its random character does not change in time, or changes only in a controlled manner, and hence the values belonging to various points in time can be regarded as a series of probability variables taken from some fixed distribution. In my opinion, it is not so much that expected value is an existing and rational concept, but rather that the ergodic average is a poor or non-existent concept. The more general question at hand, in my view, relates much more to the relevance of statistical models and the economic approach founded on probability calculation, as well as to the limits of this approach. I do not believe that the time average or averaging according to $\omega$ are radically different in terms of their usefulness. Both cases present the same fundamental problem.

The essence of the problem is that economics occupies a special place among the sciences. To put it somewhat theatrically, it lies between cosmology and the engineering sciences, and between the physics and chemistry on which they are based. I don't really feel qualified to form a definitive and irrevocable opinion on the modern science of cosmology, but I do often have the impression that, despite the very sophisticated set of mathematical tools applied, the relevance of the answers to the questions raised is not much more advanced than that of classical Greek mythology, where – if I remember correctly – *Kronos* devoured his own children. Every disputed contention in economics is better grounded and empirically better supported by an order of magnitude than the modern study of origins in the natural sciences. On the other side, of course, we have physics and the technical sciences built upon it, with their astonishing achievements beside which the empty chatter and nonsense of economics pale in comparison. Naturally, just as no one challenges the importance of cosmological research, since it seeks answers to the most fundamental human questions, so too is the importance of economic research beyond dispute. In both cases, we seek – based on the knowledge at our disposal – to provide a logically satisfying answer to what we regard as an important question. The simple fact is that we cannot answer every important question on an equally sound basis, partly because we lack the requisite data or observations, and partly because the complexity of systems surpasses all human understanding. The greatest trick of the human mind is simplification, to get to the crux of the matter. This is why we search for causes, why we believe that things have a purpose and meaning, and why we favour axiomatic systems proving that the world can be grasped by means of simple and self-evident underlying principles. This, however, does not always work. The accuracy of the answers we give to economic questions cannot compete with expectations in the engineering sciences. With so many parallels, economic systems are too complex. This is to say, in my view, that the question is not whether or not processes are ergodic, but rather how much we can validate them and how relevant is the time average arising from possible ergodicity. The main point of my observation is that even if the time average exists, this average carries no real relevance since we cannot estimate it on the relevant time horizons based on the data at our disposal.

Economic processes are built on extraordinarily stable cycles, and these cycles are of a fundamentally biological nature. There are stable daily or weekly cycles, and there are other cycles of longer or shorter duration. In winter it is cold, so we must turn the heating on, we must feed the children every day, they must go to school, we must go to work, and so forth. But in the lifestyles of individual generations there are also stable and predictable, longer cycles spanning decades. Young people want to date each other and seek partners, so external appearance is important to them and consequently they follow fashion. Older people suffer various ailments in a very predictable manner. In other words, to use another cliché, there really is nothing new under the sun. The same story repeats itself again and again, in space and time. Economic theory places individuals and their decisions at the focus of its sphere of thought. This is not a problem in itself, but what is highly misleading is the lack of emphasis on the way in which individual preferences are not individual at all, but are essentially, and to a very significant extent, biologically determined. These fundamental, clearly observable and very stable individual cycles blend together to ensure the extraordinary stability of societies. If anything can be learned from the history of the past few decades, it is how slowly things change in a country's social and economic life and how bewilderingly impotent economic processes are. Economic and technical endeavours all but fuse together in the operation of the systems that serve cycles which are governed by fundamental biological needs. It is difficult to decide where technical matters end and where purely economic matters begin. The border areas and interconnections are legion.

Of course we know that man does not live by bread alone. Other social, intellectual or spiritual needs appear beyond the biological level. Naturally, the higher we climb the levels of abstraction, the more dominant economic or social considerations become, but at the same time the sparser our knowledge becomes, and the harder it is to grasp or manage. As we ascend the hierarchy of needs on the steps of Maslow's pyramid, the processes generated by various needs become harder and harder to comprehend, increasingly unpredictable as they become increasingly abstract.

At the peak of abstraction are the financial markets, where tangible products disappear completely and only trading amid the bluffing and cloud cuckoo land of hopes and desires remains. Computers spew out data to no avail, as the data only reflect what the cloud cuckoo land builders happen to be thinking about cloud cuckoo land at any given time – be it their own, or other market players' versions of cloud cuckoo land; naturally at a specific given moment in time. Although the actual biological cycles that ensure economic stability are still present, they are very much pushed into the background; they are not even visible from the top floor of the skyscraper. While we often hear that the financial sphere is observable in a way well supported by data, one of the difficulties of financial modelling is that the process to be modelled is extremely unstable. To use the terminology applied by Iván Bélyácz, financial processes are in reality nonergodic. More accurately, this is not true either. If we can believe the work of *Thomas Piketty*, then on average over a longer period of time yields show stable averages, meaning that financial processes are ergodic on a historical scale. The only problem is that the historical scale ensuring ergodicity is in reality irrelevant. It is too long from the point of view of the present. Just as no one is really interested what yields were seen in *Napoleon*'s time, so no one really cares what yields will be seen 100 or 200 years from now either. What everyone does care about is what the yield will be in the next brief period to come. It is another question whether the coming period means a day from now, or a tenth of a second from now. Many signs point to a tenth of a second as the more relevant time horizon. But then why would processes be ergodic? What miracle would ensure the stable behaviour of a system in time? Nothing would, obviously. In other words, while numerous economic processes spring from stable, predictable and statistically easily measurable, simple cyclical processes, we do not actually encounter any kind of stabilizing cycle in the area of financial processes, since in this case we are dealing with processes that exist on a very high plateau of abstraction and desire. Financial markets attempt to determine the value of savings, which is to say that they endeavour, based on present knowledge, to make decisions – or rather estimates – pertaining to the future. At the same time, we must also see that the concept of savings is a social convention, or as one might also say, a logical fiction cast in a legal framework. Savings are always a promise. If you pay in your pension contributions, you will get a decent pension in future. Really? As we know, a promise is a beautiful thing if it is kept.

It is worthwhile, however, refining the argument somewhat further here. The economic crisis of 2008 shook economic processes to their foundations and realigned the global economy. Every one of the political landslides occurring these days is a consequence of this crisis. Confidence in the stable value of savings has been fundamentally shaken as a result of the crisis, which in other words means that confidence in a secure future has been shaken. When someone took out a loan in Swiss francs, they thought – as did the lender – that the exchange rate would remain stable even in the longer term. When the exchange rate collapsed, the borrower's life fell apart as well. Explicitly or implicitly, they received a promise for which there was no guarantee. To once again cite the study by Iván Bélyácz, the borrower thought – because this is what was suggested to them – that the exchange rate has an ergodic or stable time average, like pressure or temperature. As a consequence, society's faith in competition as a social arrangement creating equilibrium and security was also shaken. The borrower received a promise, that their repayment instalments will be this or that much. But no one wants to keep this promise. Among human needs, the desire for security and stability plays an important role. But if market processes are nonergodic, then who will safeguard the desire for stability? Who will protect small investors, the man in the street, the hard-working little guy? In a very dangerous way, many people once more see the key to security in a caretaker state – if for no other reason than because it can fix prices. Protectionism, and the desire to bolt all the doors, is on the rise. Many see the future in the genuinely vast but impoverished East, and not in the rich and highly developed West. The West equals chaos, the East stability. Sooner or later, this increasing uncertainty in the economy and society must be reflected in economic theories. The questioning of ergodicity is part of this process.

An accusation sometimes levelled against economics is that it was unable to forecast the 2008 crisis. This, however, is a complete misinterpretation. By its nature and content, trading on financial markets is very far removed from the aforementioned biological or physical cycles. Accordingly, the so-called fundamentals of price movements do not assert themselves directly. To apply the reasoning outlined in the thinking of Thomas Piketty, let us take the rate of growth of the economy to be 1%. Market players see this as too low, wanting to see 5%. Whether or not there is any basis to this is beside the point. Investors expect 5%, and so the yield will be 5%. Or at least this is what will be declared, and investors will be satisfied in the relatively long term. But beware, as it is only a promise! And it is obvious that sooner or later a problem will emerge. In a system where resources expand by 1%, investors' yields cannot remain sustainable at 5% for long. Sooner or later, investors will want to make good on the promises and to realize yields at a lower level of abstraction. Pensions have to be paid, for example. Higher pensions were promised, and it's time to pay up. What's that, there's nothing to pay them from? Trees don't grow to the sky, and sooner or later every bubble must burst. Naturally, knowing precisely when the bubble will burst, if possible to the nearest minute or even to the nearest second, is a naïve and obviously untenable expectation. In other words, the fact that a financial crisis was coming was, in reality, known by everyone who wanted to know. In my view, it was much more to do with financial economists not wanting to predict the crisis, and being far more interested in seeing the bubble grow. Tied in with this were various mathematical and statistical theories. Of course this is not to suggest that, like some godfather, evil bankers stuffed money in their bags and bought the silence of leading mathematicians. It was much more a matter of giving employment to the scientific community, so to speak. Modern mathematics operates using exceptionally sophisticated ideas and is effective at a very high level of abstraction. Accordingly, it exists in a sphere firmly isolated from actual practice, or – one might also say – the real world. Every mathematician is fully aware of this. Consequently, they devote all their energies – with an almost childlike naivety – to seeking contact with the outside world beyond mathematics. There is no greater kudos for a mathematician than to see a theory they have researched or expounded put into practice. It is not therefore the case – as many believe – that mathematicians remain disdainful of applications of their theories in practice, but rather that they jejunely seek them out. Let us imagine just how gratifying it was to realize that the glittering world of money had need of modern mathematics. It is no wonder if many mathematicians gave up their scholarly careers, returning to visit their former monastic cells in red sports cars. The majority, however, were happy if students continued to turn up at their lectures.

To return to the question of relevance of statistical analyses, it is worth examining another type of scientific miscalculation that has received a lot of attention these days, namely the spectacular failure of public opinion surveys. In my judgement, the question is important because it sheds light on the most fundamental problems of statistical modelling, and from there directly the problem of ergodicity. The question is this: Why have public opinion pollsters failed so conspicuously? Why were they unable to predict the result in the two most significant political events of the past year, the American presidential elections and the British referendum on Brexit? In both instances, opinion pollsters confidently placed their bets on one outcome, but exactly the opposite occurred. As we know, the proof of the pudding is in the eating. And the proof of statistical methods lies in prediction. This now failed – twice. It should be emphasized that we are dealing with the simplest possible, mathematically most discussed and dissected of questions. Only a probability needed to be predicted. I know the word "only" here is not entirely justified, but still it is not a dynamic, stochastic model across several sectors and in several time periods that we are looking at here, or a complex risk management model. There are two possible answers to the question raised by this failure: a short one and a longer one. The short answer is really simple: They could not and did not know the result, but since they did not dare or wish to admit this to their clients or to public opinion itself, they bluffed instead. In the act of bluffing, they confused their desires with the aloofness expected of scientific argument. They saw what they wanted to see, and not what they actually saw – or, more precisely, did not see.

The longer answer requires some mathematical exposition. The most important political event of 2016, and arguably of the entire decade, was the Brexit referendum. The world has long talked about casino capitalism, but it seems that a new genre of casino governance has now made its appearance. There is something frightening about the way a proud, centuries-old political culture, which has served as a model for a significant portion of the world, spins a coin and bases its future not on reason or wise political deliberations, but rather on which side the spun coin falls. To begin with, let us return to the basics, to the law of large numbers. As we know, there are two laws of large numbers, a strong and a weak law. The strong law states that the relative frequency converges towards the probability. The significance of the weak law is that it provides an opportunity to estimate the speed of convergence. Let us take a given event $A$, the probability of which shall be $p$. Let us carry out an experiment $n$, and let us assume that in this the event $A$ has occurred $r_n$ times. In other words, let $r_n/n$ be the relative frequency belonging to the probability of event $A$. For the arbitrary $\varepsilon > 0$ and $\delta > 0$ according to the law, there is a threshold $N$, so that if $n \geq N$ then $P(|r_n/n-p|\geq\varepsilon)<\delta$ Proving this statement is essentially trivial, but what is far from trivial is the speed of convergence. The most important question with respect to the speed is how great is $N$ in the case of the given $\varepsilon$ and $\delta$. With respect to Brexit, the expected $p$ was very close to 50%, so that in order to provide a useful and accurate estimate the value $\varepsilon$ must be set very low, since the question is precisely if $p = 0{,}49$ or $p = 0{,}51$Below we will thus reckon on $\varepsilon = 0{,}01$. At the same time, because of the extraordinary significance of the matter, the value $\delta$ must also be set low. We would like to obtain not only a very precise, but a very certain end result. One well-known consideration is based on the central limit

$$\mathbf{P}\left(\left|\frac{r_n}{n}-p\right|\geq\varepsilon\right)=\mathbf{P}\left(\left|\frac{r_n-np}{n}\right|\geq\varepsilon\right)=\mathbf{P}\left(\left|\frac{r_n-np}{\sqrt{npq}}\right|\geq\frac{\sqrt{n}}{\sqrt{pq}}\varepsilon\right)\approx$$

$$\approx\mathbf{P}\left(|N(0,1)|\geq\frac{\sqrt{n}}{\sqrt{pq}}\varepsilon\right)\leq\mathbf{P}\left(|N(0,1)|\geq\frac{\sqrt{n}}{\sqrt{1/4}}\varepsilon\right)=$$

$$=2\left(1-\Phi\left(2\sqrt{n}\varepsilon\right)\right)=\delta\,.$$

theorem.

If $\varepsilon = \delta = 0{,}01$ then

$$\Phi\left(2\sqrt{n\varepsilon}\right) = 1 - \frac{\delta}{2} = 0{,}995,$$

from which

$$2\sqrt{n\varepsilon} = 2{,}5758.$$

Therefore

$$n \geq \left(\frac{2{,}5758}{2 \cdot 0{,}01}\right)^2 = 16587.$$

Apropos of this chain of thought, it is worth emphasizing that in the central limit theorem the speed of convergence is not immeasurably rapid. There are a number of known mathematical considerations that quantify the speed. With regard to the nature of these, a square root speed is implied. This is to say that if we multiply the size of the sample by a hundred, then the error will only be reduced by one decimal. For example, in the case of a sample of 1,000, if the estimated probability is 50% then the distance between the binomial and normal distributions will be around 1.26%. In the case of a sample of 4,000, it will be about half of that, or 0.063%. These values are very small, but we must not forget that the error we would like to estimate is 1%. In other words, for a sample of 1,000 the error made in the calculation is greater than what we would like to estimate.

The *Financial Times* summarized the public opinion polls with respect to the Brexit referendum.[2] Of these, only one asked a sample of 20,000 people, and this opinion poll was carried out on January 20, 2014. At that time, 41% of respondents thought that the UK should leave, and the same percentage thought they should stay. In another sample taken on December 3, 2015, some 10,015 people were questioned, with 42% voting to leave and 40% to remain. In the last opinion poll conducted on June 22, 2016, some 4,700 people were asked, with 55% voting to remain and 45% to leave. If $\delta = 1\%$, then taking

$$2{,}5758 = 2\sqrt{n\varepsilon}$$

as the rule, then:

$$\varepsilon = \frac{2{,}5758}{2\sqrt{n}} = \frac{2{,}5758}{2\sqrt{4700}} = 1{,}8786 \times 10^{-2} \approx 2\%$$

2 https://ig.ft.com/sites/brexit-polling/ - There were four public opinion polls on June 22, of which two guessed the end result correctly, and two did not. In this paper I analyse the two opinion polls that produced the wrong result.

In other words, compared to the 55% the possible margin for error would be less than 2%, so that – based on the sample – they might have thought that Great Britain would not leave the EU as a consequence of the result of the Brexit vote. It is ironic but worth noting that the final result was 51.89%, which really was within 2% compared to $p = 50\%$, only on the other side of that level.

This public opinion survey was widely publicized and had a very reassuring effect. It is conceivable that, since it showed a significant lead for remain votes, the opinion poll had a major influence on the final outcome of the voting, greatly contributing to the superficial and thoughtless attitude among voters that was demonstrable after the event. Many people believed that nothing was really at stake in the referendum, and thus used the vote to give voice to their general dissatisfaction, disregarding the actual matter at hand.

Naturally, I am well aware that it is incorrect to apply the law of large numbers directly. The world is always far more complex than in textbooks, and for this reason we cannot measure the results against a simple textbook example. In reality, none of the mathematical conditions behind the above-outlined considerations are valid. The distribution of elements in the sample was not equal. It is commonly recognized that the result of the vote depended on age, education, residence and numerous other factors. It is likewise clear that the sample elements were also interdependent, since in the event of direct questioning it was probably easier to find an individual willing to respond in a big city than in the countryside, and it is also conceivable that the proportion of those declining to respond, or deliberately giving the wrong answer, was substantial among certain groups of society. In other words, there was a significant model risk, and consequently the necessary $\varepsilon = 0{,}01$ expectation was insupportable, so that beyond a certain point the increase in $N$ became meaningless precisely because of the high model risk. Given that this is well known, pollsters routinely apply weighting when assessing a sample. If the weights are correct, then the result will be accurate, but if they are wrong then the results will be very inaccurate. Of course, it is clear from the result of the referendum that in this case, besides the small sample, the weights applied were also wrong.

A great many people offer a great many explanations for this spectacular blunder. The majority of these concentrate on the social and sociological background. I do not dispute the legitimacy of these analyses, but evidently these are only arguments made with hindsight, the scientific value of which is slight. Unfortunately the goal of public opinion surveys is to predict, and not to provide explanations after the event. For us, however, this is not the essential point. The bottom line is that no responsible forecast could be given based on either the quality or quantity of data, and it is precisely this which represents the main barrier to statistical analysis of economic processes. Increasingly often we encounter complex and bewildering statistical models used in the arguments of researchers who regard the various data as if drawn from a kind of random number generator. They attempt to reach statistical conclusions based on tens of thousands of pieces of data gathered over many years. They use modern computer programme packages as their main tool, the operation of which is completely obscure and unpredictable beyond a certain desired accuracy. Researchers have no conception of either the nature of errors or their magnitude. A different programme package may deliver a slightly or significantly different result using the same data. This is when manipulation and weighting of data on an expert basis takes over. In other words, the statistical background is but a smokescreen we use to conceal our ignorance.

In conclusion, I would close with an observation concerning how seriously we should take *Samuelson*'s ergodic hypothesis. In order to answer this, it is worth examining the circumstances of its genesis. Samuelson worked in the economics department of MIT, one of the world's most important centres of science. MIT has the highest number of Nobel laureates per square metre of its grounds. People breathe mathematics in a place such as this. For me, one of the most startling aspects of Samuelson's work is how soon he recognized, for example, the potential financial applications of the Itô calculus. When in the 1960s Samuelson placed the Itô calculus on the axis of mathematical theory describing price movements, even most experts in mathematics did not really know what kind of mathematical theory they were dealing with. Samuelson was an exceptionally ingenious thinker with an astonishing breadth of knowledge, whose abilities can only be praised. Not only was his knowledge unusually deep, but he was able to combine and interpret what he knew in a highly sophisticated way. In an intellectual environment steeped in mathematics such as that of MIT, a scientist will only be taken seriously if they themselves speak the language of mathematics. It follows entirely naturally that they should adopt that environment's received conceptual frameworks, behavioural forms and terminology. If we add to this the fact that the United States was at its peak economically and politically in the period in which Samuelson worked, and that the questions that rightly arise today were not really considered at that time, then it stands to reason that someone would throw in the concept laying the foundations of thermodynamics. Why? Simply because it sounds good and you can dazzle people with it in the university cafeteria.