RECONSIDERING ERGODICITY AND FUNDAMENTAL UNCERTANTY

Introduction

At the ascendance of the rational expectations revolution, Paul Davidson (1982-83) argued that the rational expectations hypothesis (REH) was not to be taken seriously due to a deep ontological reality: that economic reality is nonergodic, that the system does not converge to any long run equilibrium or distribution, more fundamentally that as a result it cannot be meaningfully forecast. Davidson went further to argue that although J.M. Keynes never used the term "ergodicity" (much less "nonergodicity"), this was a strong intellectual foundation for his concept of fundamental uncertainty that has been viewed as deeply important for much of Post Keynesian macroeconomics since the *General Theory* (Keynes, 1936, henceforth "GT")). Davidson has since defended this argument against many critics who have argued that other interpretations or possible foundations for Keynes's fundamental uncertainty might be superior, with leading candidates involving more emphasis on shorter range epistemological issues rather than the long run conditions associated with nonergodicity.

This case has been strongly asserted by Rod O'Donnell (2014-15), engaging in a detailed critique of Davidson's position, from its foundations through his more recent debates with various parties, focusing on a variety of issues. Especially important among these are the time issue, with Keynes's emphasis on the short run (as symbolized by his famous wisecrack that "in the long run we are all dead") suggesting that he would not be sympathetic to the long run elements ultimately involved in the nonergodicity concept. Given that shorter run focus, then a more behavioral approach focusing on the epistemological issues would be more amenable to Keynes's broader philosophical approach. Following on that, O'Donnell (1989; 2013) and allies (Carabelli, 1988; Runde, 1990; Kregel, 1998; Velupillai, 2013) argued that Keynes's philosophical perspective emphasizing probability as a logical issue in his *Treatise* *on Probability* (Keynes, 1921, henceforth "TP") was not based on or consistent with ergodic theory, which had developed later, with his views drawn on much earlier sources with a different approach. Hence, Davidson's argument that his nonergodicity foundation for Keynes's views is not sustainable.

Davidson (2015) offers a variety of responses to these critiques, which shall be considered in more detail below, some of them more successful, some of them less so. However, we shall consider aspects of these issues focused on less clearly by either Davidson or O'Donnell to get at the underpinnings of this dispute. In particular, while both provide some information about the history of ergodic theory, neither presents a clear account that leads to the situation in the 1930s when Keynes (1939) had his contretemps with Jan Tinbergen (1937, 1940) over how to do macroeconometrics, particularly for time-series data to estimate macroeconometric models capable of forecasting business cycles (Boumans, 2007), the last point where Keynes concerned himself seriously at all with such issues.

In particular we shall consider how this history interacted with the development of understanding of the relationship between ergodicity and two other characteristics of time-series data; homogeneity and stationarity. The former is important in that it is what Keynes focused on especially, both in his earliest works on probability theory (Keynes, 1907, 1911, 1921) through to his debate with Tinbergen, without really mentioning any of these other concepts. This serves as the sledgehammer for O'Donnell's argument that Keynes's views had nothing to do with ergodicity, with the main influence on Keynes for this coming from the nineteenth century (Lexis, 1875, 1879). We shall see that this is not such a simple story, with stationarity the intermediate concept between the two. While indeed in the end, stationarity is neither a necessary nor sufficient condition for ergodicity, the two overlap for many cases, with focus in the 1930s especially on stationary ergodic systems. As it is stationarity happens to be a necessary condition for that homogeneity that Keynes was so concerned with, although not always a sufficient condition. Furthermore, through his major professor, Paul Ehrenfest, Tinbergen had close links with one of the main developers of ergodic theory and very likely saw these three as closely linked, whatever Keynes knew or thought about all this fully at the time (Boumans, 2007).

We shall also examine how certain aspects of these relationships in light of the history of ergodic theory relate to other aspects of arguments Davidson (1996, 2010) has made regarding the matters of ontology and epistemology in relation to nonergodicity. These relate to arguments made by Rosser (1998, 1999, 2001, 2006) regarding the relationship between complex dynamics and fundamental uncertainty, especially regarding chaotic dynamics, which are generally ergodic. Davidson long used this fact to dismiss the idea that complex dynamics could serve as a foundation for fundamental uncertainty. However, the possibility that certain forms of chaotic dynamics may be of the non-stationary ergodic kind, something that Davidson long denied existed, forces us to reconsider the nature of what is really ontological and what is really epistemological in this situation.

We shall also consider an argument over whether Davidson was right to be motivated by arguments regarding ergodicity and "economics as a science" by Samuelson (1969). While that may have served a useful purpose at the time, it may be that this was based on a misreading of Samuelson's argument (Samuelson, 1968), as argued recently by Álvarez and Ehnts (2014).

In the end, we shall argue that as a practical matter the behavioral approach urged by O'Donnell may offer much for studying implications of Keynesian uncertainty in the short run, whatever the ultimate ontological status of uncertainty is, with in fact Koutsobinas (2015) arguing that Keynes was the "first behavioral economist," and that even Davidson (2011) has advocated viewing him as a behavioral economist as well. As for uncertainty itself, it may be that non-stationarity is more important given its closer link to non-homogeneity than the more disconnected and distant nonergodicity in terms of providing a solid foundation for an effectively ontological view of Keynesian fundamental uncertainty.

O'Donnell's Critique of Davidson's Argument and the Latter's Response

Given that Davidson developed his argument over several decades in numerous works, not all of them in full agreement with each other on every point, we shall proceed initially by using O'Donnell's (2014-15) summary of Davidson's argument as our starting point, despite the danger that this may be unfair to Davidson and tilt the argument in O'Donnell's direction. However, hopefully this will be counterbalanced by trying to give the strongest possible argument in reply, whether that is one made by Davidson (2015) in his rejoinder, or whether it comes from some other source. We wish to emphasize that while it may look like this is what we are doing, the main purpose is not to play an adjudication game of "three points for person A and two points for person B with four points a draw," although admittedly there will be some of that going on here. Rather we wish to provide the deeper context to clarify this dispute and move it to a higher ground above the immediate back and forth.

We start by noting O'Donnell's terminology, which Davidson also follows in his rejoinder. So, O'Donnell refers to Davidson's view as the "Ergodicity/Nonergodicity" (ENE) approach, whereas he labels his own epistemologically oriented behavioralist view as being the "Human Abilities and Characteristics" (HAC) approach. We shall use these acronyms where appropriate.

O'Donnell identifies six fundamental arguments that Davidson supposedly makes, although some of these are closely interlinked with each other. They are a view of Frank Knight's (1921) risk versus uncertainty distinction based on the ability to assign probabilities or not; an objective frequentist approach to probability; the idea that probabilities represent knowledge not uncertainty (closely related to the Knight view); the linking of the risk and uncertainty distinction to the ergodic/nonergodic distinction, the incorporation of the epistemological issues of learning, rationality, and survival into the ontological framework as mere epistemological problems that should disappear over time in an ergodic system, and finally a reformulation of Knight's distinction to fit within the ENE framework, with Davidson critical of Knight for having a merely epistemological view of uncertainty rather than the proper ontological one based on nonergodicity.

Let us begin by accepting that O'Donnell appears to be correct about the second argument, that Davidson adopts a classical frequentist approach to probability theory and his whole presentation of the ENE approach. This is clearly very much at odds with Keynes's strongly subjectivist and quasi-Bayesian approach derived from a logical perspective. Indeed, while Keynes accepted some frequentist arguments at times and was not totally a subjectivist, Aldrich (2006) argues that on these matters Keynes in the TP (1921) was both a voice for past viewpoints such as those of Lexis (1875, 1879),[1] but also of a broader continental tradition that elevated subjective and Bayesian elements over the objective frequentist view. Keynes (1911) had long criticized the leaders of this latter, largely Anglo-American, view, especially as represented by Karl Pearson (1907). But followers of Pearson, especially the enormously influential R.A. Fisher (1922),[2] would go after Keynes after the TP came out in 1921, excoriating him on numerous grounds ranging from alleged mathematical imprecision to a tendency to ramble on about unimportant philosophical issues. This debate spilled over onto the ongoing development of ergodic theory happening at that time (which O'Donnell seems to be unaware of), with Paul Ehrenfest supporting Fisher's view, and through him this view being taken up by Tinbergen (1937). It is thus somewhat ironic that Davidson so strongly supports this approach that Keynes so disliked.

Let us now turn to the first and closely related last matter regarding Davidson's views regarding how the views of Knight and Keynes relate regarding risk and uncertainty, with the third question of

[1] Keynes seemed particularly to pay attention to statisticians and probability theorists who also wrote separately on economics, which was the case with Wilhelm Lexis and also with Ladislaus von Bortkiewicz (1918) who was another continental who influenced Keynes, although more for Keynes to disagree with him. Lexis has been sometimes been called the "father of the insurance industry" (Aldrich, 2006), and both Keynes and Knight spent much time worrying about such issues.

[2] Fisher's main focus was on biology and biometrics, with him and J.B.S. Haldane along with Sewall Wright combining probability theory with population genetics to codify a quantifiably sound theory of Darwinian evolution long known as the "neo-Darwinian synthesis" during the early 1930s. This view has more recently come under criticism from complexity-based evolutionary theorists (Kauffman, 1993; Rosser, 2014).

whether probabilities represent knowledge or not coming in as well. The ironic coincidence of Keynes and Knight publishing their most famous works on probability theory in the same year (1921) has long been noted, with many probably exaggerating how similar their ideas were. In fact, it was Knight who actually coined the "quantifiable risk versus unquantifiable uncertainty" distinction in terms of those words. He indeed defined this quantifiability in terms of the ability or lack thereof to assign probabilities to events. Later interpreters of this, including Keynes, would expand this to the broader concept of probability distributions, with standard modern finance assuming such distributions are some transformation of Gaussian that can be fully characterized by a mean and a variance, the latter thus becoming the measure of risk., with these two supposedly being traded off by rational, risk-averse financial economic agents. For Knight, the unquantifiable uncertainty became the zone for entrepreneurs who took leaps in the dark based on instincts beyond some point, not really all that different from Keynes's animal spirits, which would increasingly be tied also to crucial decision making by agents in the face of uncertainty as emphasized by Davidson in many places, and by Keynes in the GT.

While it is not widely noted now, Keynes in the TP did not do any of this precisely, but rather analyzed a much subtler and more variegated set of possible outcomes (Rosser, 2001). In effect he had Knight's two cases at the extreme ends of his spectrum of possibilities, with both epistemological and ontological issues determining the variations along the way. At one end indeed was the situation where "no probability distribution" exists, a point that Davidson accurately notes is in the TP, even as O'Donnell somewhat mysteriously accuses Davidson of downplaying or ignoring the TP. Davidson with some justice argues that later quotes such as in the GT about how knowing about the price of copper in 20 years of whether or not there will be change of economic system are fundamentally uncertain because no probability distribution exists for them clearly link to Keynes's discussion of this extreme case in the TP. Indeed, if there is anything we can probably be sure of, it is that in Keynes's own mind it was this lack of a probability distribution that is the foundation for fundamental uncertainty above all else (and of course, a system without a probability distribution is not homogeneous, stationary, or ergodic, even if Keynes did not specifically think about all of those simultaneously).[3]

At the other extreme of this spectrum were cases that essentially boiled down to objective probabilistic risk identifiable by a probability, with the flipping of a "fair coin" the canonical example, even as Keynes warned that while an insurance company might bet on such tosses, they would not use the actual probabilities per se to determine their pricing, worrying indeed about unpredictable variability. The intermediate cases involved such matters ranging from data availability issues to inability to distinguish between one possible probability distribution and another. These cases do not neatly fall into the Knightian dichotomy, although O'Donnell prefers to focus on Davidson ignoring Keynes's logical approach rather than dealing with this array of possible cases that are not easily labeled. They are not "true uncertainty" if that involves the nonexistence of a probability distribution (presumably due to ontological reasons), but they are not straightforwardly easily measured risk either.

O'Donnell takes Davidson to task for sort of having it both ways with Knight. On the one hand he does seem to fold Knight into the ENE framework by talking about "Keynes-Knight uncertainty" (as do many others, some preferring "Knight-Keynes")[4] defined by a lack of a probability distribution. But then, Davidson goes on to "chastise" Knight for allegedly believing in an ergodic long run equilibrium that the system will go to, with the lack of a probability distribution just an epistemological problem due to

[3] Álvarez and Ehnts (2014) pose that the distinction should be between "stochastic processes" based on a probability distribution and "non-stochastic processes," not based on a probability distribution.

[4] Among those identifying "Keynes-Knight" uncertainty with the lack of any probability distribution whatsoever has been Nassim Nicholas Taleb (2010). He has labeled events reflecting such uncertainty as "black swans." A bit like Keynes he allows for an intermediate case between easily measured risk (representing the variance of a knowable Gaussian distribution) he calls "white swans," with this intermediate case being "grey swans" involving non-Gaussian distributions with skewness or kurtosis, "fat tails," which he argues most financial returns exhibit. He argues that the stock market crash of 1987 was a black swan event, whereas that of 2008 was a grey swan one, easily foreseen as an obvious bout of kurtosis. Davidson (2010) treats Taleb as just being another Knight, not fully on board with the ENE approach and wallowing in a lot of supposedly inferior epistemological thinking.

agents being insufficiently knowledgeable or brilliant to figure it out. It is hard to defend Davidson's position on this point.

This brings us to the central matter of the ergodic/nonergodic distinction. We shall investigate this more deeply in the following section, but for now let us look at how these two pose it. They agree on a definition of what "ergodicity" is, a system whose "space averages" equal its "time averages." It happens to be the case that this is not a definition used by any mathematician who has studied this, although it is a widely cited and quoted implication of what happens in a garden variety ergodic system, first identified as such by Paul Ehrenfest and his wife Tatiana Ehrenfest-Afanessjewa (1912) who also first coined the phrase "ergodic hypothesis," with Paul Ehrenfest becoming Tinbergen's major professor in the early 1920s. This leads to perhaps the sharpest disagreement between our two interlocuters, namely over the matter of time horizons, with, it must be said, O'Donnell probably gaining the edge on this one.

On the one hand, the idea that "space averages," not always an appropriate term for what is involved, with much of the literature preferring "microcanonical phases," or "ensembles" (van Leth, 2001), will equal long run time averages opens the possibility of forecasting those long run time averages from the shorter run space averages. This indeed might be possible, if in fact one knows that one is dealing with an ergodic system. But here we run into this serious epistemological problem that Davidson simply refuses to own up to, even in his rejoinder. He notes theorems that establish that after some time, errors of observation or estimation will converge to zero in ergodic systems. But that convergence occurs ultimately at the infinite time horizon, and O'Donnell is completely accurate to note that for an ergodic system the shorter run averages may well deviate far from the longer run ones, even moving further away from them for possibly arbitrarily long times, certainly longer than the lifetime of any individual or even of a long-lasting organization such as a firm or an organized religion or a nation state. There simply is no guarantee at all of being able really to determine whether or not one is dealing with an ergodic system. Surely, this is the most devastating blow to the ENE view, especially given Keynes's own dislike of longer run analysis as noted above already, although we shall return later to what may have motivated Davidson to follow such an approach despite this clear contradiction with Keynes's views.

After this, the remaining issues regarding incorporating epistemological concerns such as learning, rationality, and survival seem less important. Indeed, O'Donnell's discussion of these issues seems more directed at showing that his HAC approach can handle these in ways that are consistent with Keynes's logical view, which emphasized the "weight of evidence" as determined by "confidence," a concept that brings in the subjective element of Keynes very strongly (Shackle, 1972; O'Donnell, 1989; Dequech, 1999; Rosser, 2001). Once one allows oneself to assign weights following some quasi-Bayesian prior, then one can proceed in a reasonably rational, if bounded, way, and even Davidson accepts much of this when he emphasizes that consumption decisions may be ergodic (or routine), but that weights must be put on to deal with the decisions under uncertainty such as capital investment, where animal spirits run rampant (Davidson, 1972).

The Development of Ergodic Theory

The development of ergodic theory came along several channels, mostly inspired by problems in physics that came together at various points, drawing together various branches of mathematics and particularly playing a deeply foundational role in the development of probability theory. It must be noted that this has resulted in different definitions and conceptualizations at different times regarding what "ergodicity" really means, although for our purposes these were largely resolved in the 1930s, despite some later modifications and extensions we shall concern ourselves with below. While the main strand has been driven by trying to model the kinetic theory of gases and the related laws of thermodynamics in what has come to be known as *statistical mechanics*,[5] another important strand has involved celestial mechanics, and later with the Moscow School issues related to electrical fluctuations.[6] The split between the former two, involved to some degree the fundamental division in mathematics between algebra and analysis (with geometry), with the crucial 1930s theorems of von Neumann (1932) and Birkhoff (1931) both reflecting that split in terms of their surface interpretations, even as they brought these strands together in a more deeply unified foundational theory of probability.

It is generally agreed that the starting point for the probabilistic approach to the kinetic theory of gases was Daniel Bernoulli's (1738) *Hydrodynamica*, which was mostly about fluid mechanics, but contains a chapter on relations between temperature and pressure and so on.[7] Only in the following century would there be further studies of this as people such as Lord Kelvin began worrying about how temperature is determined, with Maxwell (1860) taking up the challenge more formally, attempting to specify how a bunch of stochastically interacting particles could aggregate into an absolute temperature. It can be argued that this was the initial problem of ergodic theory, with Ludwig Boltzmann following up on Maxwell's formulation as he extended the study of thermodynamics on many fronts and lines. Thus the primordial ergodic problem involved among other things relations between stochastically dynamic parts and a stochastic whole, with the latter representing some sort of longer term time average of the behavior of the microcanonical parts.

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[5] The term was coined by J.W. Gibbs (1902), who studied what we now think of as ergodic theory without ever using the term as he died in 1903. Paul Samuelson was heavily influenced by Gibbs in his views of ergodic theory. Statistical mechanics is also the most widely used branch of physics in *econophysics* (Mantegna and Stanley, 2000; Rosser, 2008).

[6] The Moscow School came to be very important after the crucial theorems by von Neumann and Birkhoff, led by such figures as Khinchin, Kolmogorov, Sinai, Gelfand, and Arnol'd (Fine, 1973; Gallavotti, 1999; Uffink, 2006). The focus on electrical oscillations was initially inspired by Lenin's claim that "communism equals electrification."

[7] At the time Bernoulli was also working on the St. Petersburg Paradox, also an important problem in early probability theory as well as marginal utility theory, with many considering it be a foundational analysis of the theory of quantifiable risk.

What preceded the term "ergodic" was the term "ergode," coined by Boltzmann (1884), a deep student of Greek. His student Paul Ehrenfest (Ehrenfest and Ehrenfest-Afanessjewa, 1912) would first coin "ergodic hypothesis," with Ehrenfest as a student of Boltzmann carrying much authority on the matter. The Ehrenfests posed that "ergode" combined two Greek words meaning "work," *ergos*, and "path," *hodos* (Uffink, 2006). However, this widely accepted and conventional interpretation has been challenged by Gallavotti (1999), who finds Boltzmann coining the term "monode" earlier to mean a *stationary distribution*, following his efforts to more deeply formalize Maxwell's theory, with "ode" supposedly coming from *eidos*, of "similar." He then began using "ergomonode," with "erg" in this case indicating "energy," from the Greek, *ergos*, "work," and thus an *ergomonode* representing a stationary distribution of kinetic energy. From this Gallavotti argues came his "ergode," from which the Ehrenfests coined "ergodic," which makes a lot of intellectual sense, but which cannot be proven, and in this case the personal authority of Paul Ehrenfest as Boltzmann's student must be taken seriously.

While this debate over etymology may seem obscure, it reflects the fact that what the "ergodic hypothesis" really was that the Ehrenfests formally proposed was less than clear, drawing on an immense amount of work by Boltzmann that varied over time in what it said and how it was to be interpreted, with him coining a variety of terms that were closely related to these concepts. Some of the confusion arose over precisely how one was defining these lower level phenomena and how they were to relate to the higher level phenomena: were they a multiple ensemble of loosely interacting trajectories or was there from the very essence of the implied aggregate stationarity some deeper unity, a single energy rather than many coming together? In formulating "the ergodic hypothesis," the Ehrenfests focused on a formulation by Boltzmann that given a set of underlying microcanonical trajectories on an energy hypersurface characterized by a Hamiltonian equation, each with a mass subject to an arbitrary time-independent potential, all of this a *phase point* or ensemble; for any given region the probability that the phase point will be in that region will be a stationary distribution. In that regard, the original ergodic hypothesis was deeply and totally connected to the concept of stationarity, a link that would remain well into the 1930s. This form of the ergodic hypothesis proposed in effect that trajectories would return to any given zone, an idea of recurrence and space-filling or measure preservation, with the recurrence theorem of Poincaré (1893) emphasizing this more, although he was inspired by problems of celestial mechanics, with this theorem important in the early development of chaos theory.

The formulation and discussion by the Ehrenfests received a lot of attention and became enormously influential, along with their arguing that it coincided with the condition that "space averages equal time averages" in the infinite time horizon, which has come down to be so widely used in economics discussions as "the definition" of the hypothesis. It is thus ironic that within statistical mechanics, it was quickly understood that the precise formulation the Ehrenfests gave had problems, with they themselves worrying that the hypothesis as they formulated it (we are avoiding precise details here) was in fact not true in statistical mechanics. Indeed, within a year or so this came to be accepted, even as the hypothesis continued to spread widely among physicists and broader intelligentsia (Uffink, 2006).[8] As this is relevant to whether or not Keynes knew about ergodic theory or "the ergodic hypothesis," the degree to which this argument spread is relevant, and while he certainly operated in relevant circles at that time and was actively interested in problems of probability, we do not know whether he heard of the formulation by the Ehrenfests then or not (or even later).

The failure of the Ehrenfest formulation, even as it became famous, led to a period of floundering around and looking at alternative approaches, including even Keynes's ideas from the TP

[8] Without getting into too many details the kinds of problems that their formulation allowed for were trajectories that followed only rational paths, thus appearing to be "space-filling" without in fact hitting all points, with the set of points hit constituting a set of Lebesgue measure zero, thus bringing in how measure theory was crucial to solving this problem.

somewhat.[9] However, eventually the frequentist approach favored by Maxwell, Boltzmann, and the Ehrenfests would dominate as John von Neumann (1932) succeeded in formalizing a widely accepted and more precise version. He was coming from working on stochastic problems in quantum theory (as well as applying the Brouwer fixed point theorem to primitive game theory) when he came to study the problem of the narrower "quasi-ergodic hypothesis"(or "mean ergodic theorem"), with the issues of how measure theory relates to probability theory being central (Halmos, 1958). While his paper was published after Birkhoff (1931), the latter openly stated that he was inspired to prove his extension after seeing von Neumann's paper first.

Let us briefly consider von Neumann's formalization, as it has provided the solid foundation for all that has followed since in ergodic theory. Let T be a measure-preserving transformation on a measure space with for every square-integrable function f on that space, $(Uf)(x) = f(Tx)$, then U is a *unitary operator* on the space. For any such unitary operator U on a Hilbert space H, the sequence of averages

$$(1/n)(f + Uf + \dots + U^{n-1}f) \qquad (1)$$

is strongly convergent for every f in H. We note that these are finite measure spaces and that this refers to stationary systems, just as with Maxwell and Boltzmann. Birkhoff's (1931) extension, sometimes called the "individual ergodic theorem," that modifies the above sequence of averages to be

$$(i/n)(f(x) + f(Tx) + \dots + f(T^{n-1x})) \qquad (2)$$

that converge for almost every x. These theorems have been generalized to Banach spaces and many other conditions. While they are complementary, they emphasized different elements, with von Neumann focusing more on measure preservation and essentially algebraic concerns, whereas Birkhoff's

[9] While most of ergodic theory in the 1930s followed the frequentist path, later formulations by the Moscow School and followers would reflect influence from Keynes's ideas (Velupillai, 2013, fn. 8, pp. 432-433).

formulation harks more to the dynamical systems concerns of Poincaré and his recurrence theorem, with Birkhoff showing that not only is measure preserved, but that a properly defined stationary ergodic system exhibits *metric indecomposabililty* such that not only is the space properly filled, but that it is impossible to break the system into two that will also fully fill the space and preserve measure. It was from these theorems that the next wave of developments in Moscow and elsewhere would develop. This was the state of ergodic theory when Keynes had his debate over econometrics with that student of Paul Ehrenfest, Jan Tinbergen, at the end of the 1930s.

Stationarity, Homogeneity, and Ergodicity

One of the matters that separates Davidson and O'Donnell is the matter that we might label, "What did Keynes know, and when did he know it?" Both of them agree that Keynes never used the word, "ergodic" in any of his writings. However, Davidson long argued that Keynes's views on uncertainty were consistent with the concept of ergodicity, while O'Donnell argues that Keynes probably knew nothing of this and was stuck in a past driven by dreary continentals writing in German such as Lexis and von Bortkiewicz. There is no doubt that Keynes was strongly influenced by these figures and others, although in the case of the latter more to disagree with him.

Without doubt a major focus for Keynes both earlier and in his debate with Tinbergen was his concern with homogeneity of data sets. In critiquing Tinbergen's econometric approach, Keynes specifically questioned how Tinbergen would know that the coefficients estimated for his macroeconometric models would remain properly constant over time, almost sounding like an early version of Robert Lucas, albeit without any assertion of rational expectations providing some possible solution to this problem. Keynes's favored method for testing for homogeneity, drawn from Lexis (1879), was to break down the time-series into sub-parts and estimate the coefficients for each of them to see if there was such a stability of them over time. Such stability is not quite the same thing as stationarity, but Keynes saw them as positively linked and argued that greater homogeneity was associated with more stability and more stationarity. On this he disagreed with von Bortkiewicz (1918) who argued that in German data sets on suicide over time in different regions, breaking down the regions more narrowly might increase the homogeneity of the data, while decreasing the stability of local time estimates of suicide rates. One can see that while this sort of discussion is not exactly the same thing as ergodicity, it has a similarity in relating literal space averages to time averages. Whether or not Keynes knew of any of the definitions or theorems of ergodicity, his concern with homogeneity was clearly closely linked to similar concerns, particularly in a world where ergodic theory was all about stationary systems. And, it happens to be the case that while a non-homogeneous system may be made stationary by some appropriate transformation, such as first differencing, a non-stationary system cannot be homogeneous (Suppes, 1969).

Keynes's concerns were very close to the issues involved in the debates over ergodic theory at the time. Indeed, at the core of the debate between Keynes and Tinbergen was very much the matter of transferring concepts from physics to economics. Keynes drew on philosophy, logic, and even morality and aesthetics in his approach to probability theory, doubting homogeneity of economic time-series because he doubted that they represented any probability distribution, his clear concept of uncertainty. He doubted the relevance of bringing in concepts directly from physics. On the other hand, influenced by Ehrenfest who first suggested that this physics PhD should apply his ideas to economics, Tinbergen argued for the existence of a stable "natural law" underpinning economic time-series, even as he was doubtful that such series actually reflected equilibrium outcomes. This natural law would be ergodic and stationary and homogenous.

Since that time as broader situations and contexts have been considered, the once apparently tight links between these concepts have weakened, even as many important systems exhibit all three of these conditions. As noted already, stationary systems may be non-homogeneous, and it is common practice to transform time-series data into homogeneity by various methods before estimating time-series models; although, again, if there is no stationarity, homogeneity cannot be achieved, there is no getting around Keynes's deep concern, which certainly suggests that what was ultimately most important for Keynes was stationarity, the matter he debated with von Bortkiewicz.

Furthermore, it came to be realized by Malinvaud (1966) that it was possible for a stationary system to be nonergodic, with the canonical example being a limit cycle, with this pointed out by Davidson (1982-83) from the beginning of his identification of Keynesian uncertainty with nonergodicity. Keynes was never a fan of explaining business cycles by using limit cycles, essentially viewing all business cycles as ultimately idiosyncratic. He accepted in principle that if a business cycle were driven by a limit cycle its parameters might be estimated with the resulting model able to predict, but he was simply skeptical that there was sufficient homogeneity or stationarity to be able to do so. Nevertheless, many of his followers, and even some related predecessors, who used his ideas did model business cycles as arising from endogenous limit cycles generated by combinations of multiplier and accelerator effects or special formulations of one or the other.[10] The still widely used idea of separating trends in macro data from cycles predates Keynes, with the widely watched NBER "Harvard Barometer" model doing so in an ad hoc way (Persons, 1928), although the failure of that model to predict the Great Depression badly damaged its reputation.

[10] An incomplete list includes Kałecki (1935), who actually preceded Keynes's GT, along with Samuelson, Kaldor, Metzler, Hicks, and particularly notably Goodwin (1951), whose more complex formulation would later be shown capable of generating more complex dynamical patterns, including erratic fluctuations, possibly chaotic, with almost all of these models under suitable modifications later being shown to be capable of generating chaotic dynamics (Rosser, 1999).

While this observation by Malinvaud is widely known, the vast majority of Post Keynesian economists believe that it does not go the other way that an ergodic system might not be stationary, with that set apparently including both Davidson and O'Donnell.[11] And indeed for a long time this was a more widely held view, particularly given the conditions for the theorems of von Neumann and Birkhoff and the immediate extensions. However, relaxing some of the conditions for those theorems does allow for the possibility of non-stationary ergodic systems. One of those conditions is to allow infinite measure-preserving spaces, along with some other special relaxations (Madsen and Isaacson, 1973; Anily and Federgruen, 1987; Aaronson, 1997). Among the systems that can exhibit such characteristics include some exhibiting certain forms of *ergodic chaos* (Aizawa, 2000; Shinkai and Aizawa, 2006). This development in some sense brings ergodic theory back to some of its earliest concerns and origins, given the link with Poincaré's (1893) recurrence theorem. Chaos theory has always involved dynamics where paths return near to where they have been in the past, a characteristic deeply associated with ergodicity, even as they may be highly locally unstable and thus prone to non-stationarity.

Ergodic Chaos and the Battle of the Axioms

Among the parts of the argument between Davidson and O'Donnell that this observer finds the least interesting to the point of annoying is the wrangling over who has the better and purer and more truly "Keynesian" axiom, or more precisely, the axiom that Keynes most fundamentally did not accept thereby making it the one true foundation for truly fundamental Keynesian uncertainty. For Davidson with his ENE approach, it is the Axiom of Ergodicity that is what Keynes most profoundly would have rejected, with Davidson arguing that this makes uncertainty an ontological concept beyond mere problems of learning. For O'Donnell, it is the Axiom of Perfect Information, fitting into O'Donnell's view that in fact uncertainty for Keynes was ultimately an epistemological problem. This observer finds that

[11] One eloquent exception is Velupillai (2013, fn. 8, pp. 432-433).

in the face of the problems now noted regarding the relations between stationarity and ergodicity, particularly the possibility of ergodic chaos this argument becomes basically empty. Keynesian uncertainty is based on both.

This observer has been through this before with Davidson in particular, although without this most recent addendum. Davidson (1996) specifically rejected chaotic and more complex dynamics as possible foundations for Keynesian uncertainty on his old ontological axiomatic approach. Since most chaotic dynamics are ultimately ergodic, they do not violate the ergodic axiom. This means that they present only epistemological problems. Without precisely bringing in this issue of non-stationarity, Rosser (1998, 1999, 2001, 2006) argued that such that the unpredictability of such systems was "effectively ontological," due to the infinite knowledge requirements involved in knowing a system subject to sensitive dependence on initial conditions, aka, "the butterfly effect." The argument was that complexity provides an explanation or foundation for fundamental Keynesian uncertainty.

In a sense O'Donnell makes a similar complaint, without bringing up ergodic chaos, in his pointing out that given the infinite horizon convergence aspect of ergodicity, it may take infinite time to know whether a system is ergodic or not. Those believing in rational expectations may simply argue that they are axiomatically assuming away the problems, and Davidson may be right to point out that they are effectively assuming an ergodicity they have no real right to assume, especially given the massive experimental and other evidence casting serious doubt on the reality of rational expectations. But such phenomena as ergodic chaos push this further, with systems being ergodic, but fundamentally unknowable due to their non-stationarity, with Keynes and his focus on homogeneity in effect being in the right ballpark on this issue. Ergodicity does not save one from the problem of Keynesian uncertainty, and whether one wishes to call the problem ontological, epistemological, or effectively ontological really does not matter all that much.

As it is, it may well be that despite all the wrangling over this, it is recognized by both Davidson and O'Donnell to lead to a somewhat similar outcome: that Keynes was substantially a behavioral economist and that Post Keynesian approaches to dealing with uncertainty that implies bounded rationality at best need to rely on behavioral concepts (Davidson, 2011; Rosser and Rosser, 2015).[12] Maybe the battle of the axioms really is over.

The Curious Role of Samuelson

While many of the things we now know about ergodic theory were not as clear when Paul Davidson first formulated his argument, the question nevertheless arises as to why he made it given all the difficulties it has encountered over the years since. Can we understand the context even if the argument now seems seriously flawed on various grounds? Widely repeated verbal arguments have said that it was Roy Weintraub who suggested the argument to Davidson, and that this was at least partly due to some arguments that Paul Samuelson had made following his own formulation of the efficient market hypothesis (Samuelson, 1965). In followups, Samuelson (1968, 1969) in effect repeated the old argument that Tinbergen made drawn from Ehrenfest: that natural science depends on assuming the ergodic axiom. Samuelson's strongest statement along such lines came in his 1969 piece on p. 184, quoted frequently by Davidson.

As it turns out, however, as pointed out by Álvarez and Ehnts (2014), a closer reading of Samuelson (1968) indicates that this may not really have been the view of Samuelson after all. It is ambiguous in that the 1969 statement appears unmodified. But in the 1968 piece he identifies this view

[12] One surely incorrect argument made by many of these Post Keynesian behavioralists is the claim that "Keynes was the first behavioral economist" (Koutsobinas, 2015). Now it is true that he was advocating forms of behavioral economics and bounded rationality prior to Herbert Simon, who coined both of these terms after Keynes was dead (Simon, 1955). But in fact the "axiom of perfect knowledge" that O'Donnell claims was so widespread was not so at all prior to Keynes. Belief in it, rarely actually clearly stated, was probably more the exception than the rule, with one classical political economist who wrote and revised several times a book now viewed as a deep foundation of behavioral economics being Adam Smith (1759), with his *The Theory of Moral Sentiments*.

with the "classical" rather than the "neoclassical" view of money. He says that in his youth, when he was "a jackass," he was taught and accepted the classical view, that there is an ergodic reality to economic systems resembling natural law as Tinbergen argued. But he came not to believe this anymore, with his reading the GT of Keynes in 1937 being the turning point. If so, this means that he was really in agreement with Davidson to some extent at least. Of course he dragged in the ergodic axiom in his 1965 paper on the efficient market hypothesis, but while that ergodicity condition was part of it, he never said that he believed the hypothesis was actually true. He was simply showing when and how it might be true. Of course, a motive for Davidson to resist Samuelson was the latter's effort to adopt the term "post-Keynesian eclecticism" for his own particular version of Keynesian economics, which most Post Keynesians reject, giving some added motivation to negate an apparent argument of Samuelson's.

Of course the more immediate target of Davidson in his formulation of the argument in the early 1980s was not Samuelson, but Lucas and Sargent and their rational expectations hypothesis, not unrelated to the narrow conditions of the Samuelson (1965) theorem, which certainly also relied on such an assumption, even if Samuelson did not believe it to be really true. This was indeed the period when new classical economics was riding at its highest point of prestige, with Lucas and Sargent with their rational expectations assumption apparently sweeping the boards of any sort of Keynesian theories. Curiously, they did not seem to care if the assumption was actually true, because it was "an axiom," something that is assumed and cannot be tested.[13] So, Davidson may well have been motivated to assert his own axiom, the axiom of nonergodicity, which Keynes supposedly believed in and which could be used to battle against the rational expectations axiom of Lucas and Sargent. On this matter,

[13] This matter of "testing axioms" is controversial. Davidson is right that Keynes was partly inspired by Einstein's Theory of General Relativity that was based on a relaxation of the parallel axiom of Euclid. So, Davidson argued not unreasonably that he would also be inclined to wish to relax any ergodic axiom. However, of course, the rejection of the parallel postulate (or axiom) did come from empirical tests showing that it does not hold in space-time in general due to gravity curving it. So, the empirical testing of axioms is relevant, and the failure of the rational expectations axiom to hold empirically is certainly reasonable grounds for rejecting it.

Samuelson indeed may have been in deeper sympathy with Davidson's efforts than he knew, and the various encomia and praise Davidson received for his arguments from people such as Solow, Hicks, and North may well have reflected their pleasure at seeing an argument being made on similar grounds to contest this then domineering movement. Davidson's argument served a useful purpose at that time, even if that time has now passed, and we can take a broader view of exactly when and where it holds.

Conclusions

The concept of fundamental uncertainty is a centerpiece of much of Post Keynesian economics. The foundation of this concept in Keynes's own work and in broader intellectual foundations and considerations has generated much debate and discussion in recent decades, most recently between Rod O'Donnell (2014-15) and Paul Davidson (2015). We have reviewed their arguments, finding some grounds for O'Donnell's criticism of Davidson's claim that rejection of the ergodic axiom is the strongest ontological foundation for a solid theory of Keynesian uncertainty. Probably the strongest argument made by O'Donnell involves the problem of time horizons. That ergodicity is only known or determined as one approaches an infinite time horizon not only makes it a deep epistemological problem even to determine if a system is ergodic or not, but also this approach appears to contradict some of Keynes's own strongly held views in favor of looking at shorter time horizons in studying economies.

On the other hand, Davidson can make some claims that whether or not Keynes knew of the discussions regarding "the ergodic hypothesis" (which he never mentioned in any of his writings, even while being in milieus where it is quite likely it was discussed), the understanding of ergodicity in the time period he lived and wrote in was such that it was closely linked with concerns about homogeneous data and stationarity of time-series, which we know he was concerned with, even if we now know these links to be both weaker and more complicated than was thought in the 1930s. Of course, it is true that

Keynes's own definition of uncertainty as involving processes not based on probability distributions would certainly not be ergodic. Davidson can also be defended for using the nonergodicity argument at a time period when advocates of the rational expectations hypothesis were arguing against pretty much any form of Keynesian economics by asserting the axiom of rational expectations, and asserting the nonergodic axiom was a useful way to directly combat this argument, despite its ultimate flaws.

In working through some of the history of ergodic theory, we have also highlighted hopefully relations between the closely related concepts of homogeneity, stationarity, and ergodicity, which were thought to be nearly identical at the time of Keynes. Thus, non-homogeneity can coincide with stationarity, but non-stationarity implies non-homogeneity. Furthermore, while it was long known that stationary systems such as limit cycles might not be sufficient for ergodicity, it has been widely believed, particularly among most of the participants in this debate, that it was a necessary condition for ergodicity, with indeed the main theorems of the 1930s involving stationary ergodic systems. However, it is now known that this is not the case, and examples include forms of ergodic chaos that are not stationary. Such cases undermine the old argument regarding whether Keynesian uncertainty is fundamentally ontological or epistemological. The matter is more complex, and complexity can be a foundation for Keynesian fundamental uncertainty.

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