The unit root problem: Affinities between ergodicity and stationarity, its practical contradictions for central bank policy, and some consideration of alternative

Muhammad Ali Nasir[1], Jamie Morgan

Leeds Beckett University

Abstract: Our initial focus in this paper is the problem of mismatch between policy goals and statistical analysis, based on how data is transformed and processed. This intrinsically raises ontological issues regarding the nature of an economy within which policy is made and to which statistical analysis is applied. These are of general significance to post Keynesians irrespective of the position they take on the specifics of the ergodicity debate. However, they involve some issues that overlap with some aspects of that debate. The problem as posed in this paper is specific and involves a practical contradiction regarding central bank policy and the problem of unit roots. We then consider some additional ways in which one can go beyond 'common practice' based on the example of Forward Guidance in the UK and a more institutional approach to post Keynesian analysis.

Keywords: unit root, stationarity, ergodicity, central bank policy, Forward Guidance

Introduction

The degree to which and the manner in which the future follows the past is a central issue for post Keynesians. One major area of dispute regarding this has centred on the concept of ergodocity (Davidson, 1991, 1996, 2012). This dispute has focused mainly on the long run versus the short run, objective versus subjective probability and interpretations of Keynes' work as a means to situate his legacy and significance (e.g. O'Donnell 2014/15; Davidson, 2015; Álvarez and Ehnts, 2016; and mediating summary Rosser Jr, 2015). Our focus in this paper is parallel to these issues. We are concerned with the general problem of statistical analysis in terms of the broader ramifications of what Rosser Jr refers to as 'common practice' (Rosser Jr, 2015, p. 345). Our focus is the problem of mismatch between policy goals and statistical analysis, based on how data is transformed and processed. This intrinsically raises ontological issues regarding the nature of an economy within which policy is made and to which statistical analysis is applied. These are of general significance to post Keynesians irrespective of the position they take on the specifics of the ergodicity debate. Moreover, though ontological issues overlap with subjective-objective issues for probability, they do not necessarily provide decisive evidence on either side.[2] Our concern is with problems with some degree of family resemblance (affinities) within statistical analysis and

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[1] Corresponding author, Dr. Ali Nasir: Economics, Analytics & International Business Group, Faculty of Business & Law, Leeds Beckett University, The Rose Bowl, Portland Crescent, Leeds, LS1 3HB, UK, Email: m.a.nasir@leedsbeckett.ac.uk, Telephone: 0044(0)7508457981 .

[2] To be clear, it is not our intention to extensively engage with or take a definite position on the ergodicity debate, its centrality to post Keynesian thought and its roots in Keynes' work. For this see the O'Donnell and Davidson exchanges. One might also gain context from King's history of post Keynesian thought (King, 2002). Davidson (2005) also has a position on this and a set of reservations regarding 'Babylonian' method (Dow, 2005) and, inter alia, critical realism's critique of axioms via formalism. Davidson argues for 'small tent' post Keynesianism initially focused around: an economic system as a process moving irreversibly through calendar time, subject to a key role for expectations under ontological or real world uncertainty and with reference to core theoretical commitments to an essential difference between financial and real capital, and where income effects dominate substitution effects. It is with reference to irreversibility and uncertainty that he rejects ergodicity.

what these indicate about economies as places of change through process. This concern is common to post Keynesians.

One way to approach post Keynesian commonality is based on the general claim that economies are complex processes involving institutions, conventions and practices that create some degree of stability, but which are subject to cumulative causal changes and transformations. These changes and transformations necessarily involve degrees of uncertainty (and possible fundamental uncertainty) because the future need not be like the past, despite that economic actors often have good reasons to attempt to reproduce what has previously occurred. In general, post Keynesians conceptualise an economy as a path-dependent yet open system. The commonalities are basic to the claimed adequacy (some would say realism) of post Keynesian economics, which is then instantiated in its different varieties and particular foci on theory and in policy (which may or may not then be restricted by Davidson's 'small tent' definition). Whatever else it involves, the ergodicity dispute highlights that statistical analysis often assumes a characteristic relation where the past follows a determinate (in so far as definably probabilistic) path to the future, and this can involve a variety of the Empiricist mirror thesis.

Before proceeding one should acknowledge that Rosser Jr's assessment of Davidson's position on ergodicity is balanced and reasonable. In mediating between O'Donnell and Davidson, he constructs a credible argument concerning Davidson's historical case for the prevalence of ergodicity in economic theory: it misses some of the nuance of Samuelson (if perhaps not Lucas and Sargent), does not readily account for what Keynes knew and in some sense seemed to prefer, and moreover, the concerns have been in some ways superseded by changes within the approaches available to economics (econophysics, chaos theory etc, which provide both a different intellectual history for the problem of ergodicity and alternative framings for subsequent theory).[3] However, if we focus on 'common practice' and its context there are still issues in terms of at least two related things. First, on what basis economics self-identifies as a science, which continues to involve ergodicity issues (subject to definitions), though the problem of self-identification is broader than ergodicity only. Second, what statistical practices have become habitual and so are rarely questioned in terms of fundamentals, and what these suggest about the current credentials of economics. Though economics has changed in many ways in recent decades the basic claim regarding what a science is (and so what economics must be) remains relatively unchanged. The shift in focus from 'alchemy', the 'con' in econometrics, formalism, the 'empirical turn' and now the 'credibility revolution' have all involved ways of preserving the basic claim that economics

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[3] Álvarez and Dirk (2016) provide a more substantive argument for the meaning of ergodocity in mathematics - it is a concept in a mathematical system and is, therefore, used in a different sense in economics regarding real systems, and this can be confusing. Regarding Samuelson, as many have noted, he was usually careful to acknowledge models are limited, assumptions may be unrealistic, and one should establish, rather than assume, that findings and claims are transferable to real world conditions. As a polymath he was interested in all aspects of economics, including history of economic thought. Still, it remains the case that he creates the template for less careful and able economists to absorb a mindset. His more circumscribed work is translated via textbooks (based on a template he himself created, see Zuidhof, 2014) and later literature and imitators. For example, his comments on ergodocity in the often-cited paper on classical and new classical monetary theory, note there are problems with the assumptions applied, but he still orients on ergodocity in order to avoid 'hystericity' and to enable the possibility of generalisable claims. As such, the probabilistic underpinnings become the defining feature of law-like states expressed through ergodocity, and this in turn becomes a key constituent in what makes economics a science - its formal expression and stochastic range (see Samuelson, 1968: pp. 12-13).

as a science explores the regularity of events (see Hendry, 1980; Leamer, 1983; Angrist and Pischke, 2010).[4] However, as many working on post Keynesian methodology and the philosophy of economics more generally have argued, the claim rests on a misconception of what a science of an economy can do based on how societies are constituted through process (see Dow, 1997, 2005, 2012a; Lawson, 1988, 1994, 2015). The ramifications of this, of course, are arguable and we make comment on this later.

So, in this paper we illustrate one of the problems that persist within economics based on the way statistical analysis continues to be used. The problem is indicative of continued confusions regarding appropriate points of departure for the exploration of economic issues. Those confusions are, in turn, indicative of the influence of a mindset where continued dependence on, and elaboration from, received (and so commonly accepted) methods tend to dominate over considerations of the methodological appropriateness of those methods in any given context. The problem as posed in this paper is specific and involves a practical contradiction regarding central bank policy.

In the ergodicity debate, Rosser Jr makes the point that time series data can be assessed for a range of characteristics, and these characteristics overlap rather than are identical or are all, and under all possible conditions, directly related or mutual. Data can be homogeneous (has the property of constant coefficients through time) and this leads to greater stability, which is not quite the same as stationarity but tends to be associated with it, and such stationarity 'overlap for many cases' with ergodicity (Rosser Jr, 2015, p. 333, 344). Rosser Jr's main interest here is to establish there are cases where non-homogeneous data can be ergodic and that it is 'common practice' to transform data series to render the series stationary for model estimation (Rosser Jr, 2015, pp. 344-345). He does so in order to make the claim that stationarity is neither necessary nor sufficient for ergodicity. But this notwithstanding a problem still arises in so far as data is non-homogeneous and data is not stationary, and the data series *may be* non-ergodic (though the meaning of this term is disputed). Method matters for what is assumed and for what is done.

A stationary data series does not possess a unit root and so does not exhibit a trend value dominated by its own past values. If a data series does possess a unit root then it is proportionally influenced by its own internal innovations. It will not revert to a mean. Non-stationary data series in general create problems for standard approaches to causal inference within statistical analysis. Non-stationarity makes it difficult to distinguish the effect of any explanatory factor from the internal changes in the series. As such, unit root tests are

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[4] Two issues after the special section in *Journal of Economic Perspectives* focused on Angrist and Pischke's claim for a credibility revolution (via random control trials etc) Caballero (2010) notes that the dominant attempts to respond to problems in modelling and statistical technique continue to marginalize a concern with the real world - this remains a peripheral matter. This is also implicit in Leamer's response to Angrist and Pischke, updating his 'con' argument from the 1980s. Much of the change is technical sophistication as camouflage rather than genuine attempt to master the problem of context for the dataset. It is notable that American Economic Association journals provide some recognition of core problems. Mainstream economics is not without change. However, critique continues to be partial and limited in effectiveness and scope. For example, there is much to admire in Paul Romer's recent critique of macroeconomic theory, but if one also notes the previous paper on 'mathiness' (Romer, 2015) it is also clear there is something inconsistent and partial in his position: 'Mathiness' is ideological use of mathematics. He ascribes this to Robinson's critique in the Cambridge Capital Controversy. An odd comment, given the argument was about the consistency between formal constructability and logical entailment; and Cambridge Mass conceded the actual argument.

typically applied to data series.[5] These tests often indicate that the data series is non-stationary and may possess a unit root. However, rather than accommodate to this in ontological terms, standard practice, as Rosser Jr notes, is to transform the data series (usually through differencing) in order to enable the application of widely accepted econometric approaches and tests. The point of departure is thus to conform the data to the requirements of methods rather than to recognize the potential problematic mismatch this may create. Put another way, in exploring an aspect of the economy one is always trying to manage a problem rather than avoid it. Perhaps the problem is manageable (this is an open question regarding something that must be demonstrated, see later), but the point of departure typically prevents fundamental consideration of what is achieved and foregone. The practice simply becomes standard or *common practice*. This is particularly significant when the focus is economic policy. In this sense Davidson's work remains highly relevant.

Central bank policy focuses on price stability and thus on inflation targets. This is not all that central banks do and the limits and problems of price stability, in terms of effects on and assumptions regarding, financial stability, are a common subject of debate in the wake of the global financial crisis (with reference to Minsky models, macro-prudentialism, financialisation, post-Washington consensus etc). However, having an inflation target means seeking to achieve a positive value in the data series. The very point of policy means that the data series will typically be non-stationary. It will be non-mean reverting. Moreover, if the inflation target is actually achieved (which it rarely is, hence most targets have an allowable range) then it will necessarily create a unit root. There is, therefore, an immediate and perpetual mismatch between the purpose of policy and the requirement for stationarity. This in turn creates problems of analysis once the data series is transformed. And, more broadly it indicates basic problems when one considers actual bank practice. In what follows we set out the basic argument, beginning with some simple illustrative models. We then consider some additional ways in which one can go beyond 'common practice' based on the example of Forward Guidance in the UK and a more institutional approach to post Keynesian analysis.

The observed situation based on Forward Guidance is irregularity within the breakdown of stability-seeking convention. The activity of people in the present is sufficiently altered and diverse to undermine simple attempts to delineate the future from the past. Not only does changing policy change behaviour, but the complexity of behaviour helps to account for the shifting and contingent nature of path dependency as something quite different than mere equilibrium reverting activity. This, in turn, helps to explain why data is not stationary, and so adds a further dimension to how we make sense of the significance of policy that introduces a positive value.

The practice of transforming data series to achieve stationarity

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[5] Note, the point of this paper is to expose a methodological problem rather than to provide a detailed and sophisticated analysis of the technical aspects of statistical procedures. The focus is referenced to unit roots. If space allowed one might also distinguish issues regarding other reasons for non-stationarity, such as deterministic trends, which produce a more defined period of non-mean reversion. Clearly, the problem also involves distinctions (by degree and source) between stochastic process where a random variable affects evolution and there is some degree of indeterminacy of outcome, from more deterministic forms where evolution can only follow one pathway. The systemic implications are basic to the Davidson-O'Donnell debate, but again we are concerned with 'affinities', whilst taking a slightly different perspective.

In order to establish the importance of the stationarity condition we need to set out some model approaches indicative of common understandings and practice. These do not need to be sophisticated to make the point, rather the reverse, the simpler the initial starting point the clearer the fundamental problem, which subsequent adjustments wrestle with (albeit typically without acknowledging the fundamental nature of the problem). Consider a simple AR (1) model:

$$Y_t = \emptyset Y_{t-1} + e_t \qquad (1)$$

Where e_t is a random noise process. The stationarity condition for the above process is that $|\emptyset|<1$. So, in general, there are three possible cases this one and:

$|\emptyset|>1$ where the series explodes

$|\emptyset|=1$ where the series contains a unit root

It is widely acknowledged in statistics that non-stationarity is problematic. Its absence is often referred to as a 'violation' of stationarity. This is an ordinary language use within a conceptual construction that subconsciously (or habitually) emphasises its problematic status for what is considered the norm within statistical practice. This is longstanding. For example, Granger and Newbold (1974) note that unit roots in particular lead to 'spurious' results.[6] As previously noted, it is common practise to avoid the undesirable consequences of dealing with a non-stationary series by differencing. If we subtract the Y_{t-1} from both sides of equation (1) we derive:

$$Y_t - Y_{t-1} = Y_{t-1} - Y_{t-1} + e_t \qquad (2)$$

$$\Delta Y_t = e_t$$

Now, as the e_t is a white-noise process, and the series Y_t is integrated of order one i.e. $Y_t \sim I\,(1)$ then the original series can contain a unit root, but the transformed data ΔY_t can be stationary.[7] However, transforming the data does not necessarily solve the underlying problem of analysing the data in the context of its relation to the original aspect of the economy. The original aspect manifested an initial dataset that was non-stationary, and so could not (systemically speaking) be homogeneous, and was unlikely to be, under any reasonable first description, stable. Furthermore, transforming the data does not necessarily indicate that the data will now be adequate in terms of policy applications, even if we accept statistical analyses, which derive constant coefficients. The very act of transforming the data can adversely affect these.

Our particular concern here is central bank policy. But as a transitional case consider a data series of output and prices for the UK economy from 1990 to 2016. This series uses the Consumer Price Index (CPI) for inflation and the GDP growth rate for output. One of the most frequently used unit root tests is the Augmented Dickey Fuller Test (see 1979):

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[6] The problem is broader in terms of the way statistical approaches have responded to internal critique. For example, in his well-known critique of Leamer's extreme bound analysis solution to the con in econometrics, Sala-i-Martin (1997) notes that the initial model specifications may involve 'spurious' fits due to endogenous variable effects. Endogeneity is, of course, a core issue for post Keynesians.

[7] Similarly, as any general econometrics text will note, a series will be integrated of order *d* (denoted by if it is non-stationary but is Δ^d *is stationary* (e.g. Asteriou & Hall, 2016).

Table 1 : Augmented Dickey Fuller Test Statistics

Variable	ADF Test Statistic	Critical Value at 1% level	Critical Value at 5% level	Probability
At level (0)				
Inflation	-2.470	-4.374	-3.603	0.338
GDP	-2.935	-4.374	-3.603	0.168
First difference I(1)				
Inflation	-1.912	-4.440	-3.632	0.614
GDP	-5.679	-4.394	-3.612	0.000*

*1% level of significance

Clearly, the series exhibits non-stationary behaviour. So, the point of departure is non-stationary and, by implication, at the very least raises questions regarding homogeneity.[8] In standard practice, the point of transformation is to produce a dataset that can be used to analyse as if the original aspect of the economy meaningfully expressed homogeneity. Concomitantly, one may also be seeking some previously unrevealed relation. For example, one might pursue co-integration, which through differencing seeks to identify constants in data, based on residuals that sum, under a procedure, to zero, but where the initial data was non-stationary. The very point of this is to seek some longer-term underlying precise relation apparently disguised in the original dataset. However, one might equally argue that the process is a sophisticated form of averaging that constructs a precise essence whose point of departure puts aside change as a process in order to highlight what seems to be constant. Clearly, this can be done. But the point is what does it achieve? Arguably, it conflates constancy with consistency, and contributes to a mindset where the economist is looking for something akin to a nomological status, something 'law-like' that does not change. This can then lead to a failure to adequately conceive, and so expect, causal process rather than regularised causally consistent change (as, for example, the equilibrium inducing essence of what previously seemed intrinsically irregular). Regularised causally consistent change is a conceptually different point of departure than cumulative causal change. It creates ambiguity regarding the nature of path dependency. It can be a small step (though one need not take it) from such a regularised approach to an ergodic informed mirror thesis where the past fully imprints the future.

Moreover, procedures such as differencing introduce a further break point that must be justified for how data is processed.[9] It is basic to statistical analysis that one must have an adequate understanding of the data generating process. However, if the process itself resulted

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[8] Clearly, whatever one eventually accepts in terms of ergodicity, one cannot evade Keynes' initial concerns here. Keynes' work may or may not be explicable or translatable in ergodic terms, subject to definition, but beneath this is the problem of deriving and justifying meaningful constant coefficients and related issues of model specification. The ongoing issue has become how problems are managed and accommodated to within economics as statistical practice, and what is achieved by doing so. This was basic to the Cowles Commission project (the antecedents of which initiated Keynes' concerns) and also to the more interesting attempts to reconcile issues. In addition to the Nell and Errouaki example referred to later, one might also note Aris Spanos' (e.g. 2015) various papers setting out the development of the LSE tradition building from Haavelmo (and sharing some commonality with Hendry's work; see more generally Keane, 2010).

[9] One should not also that differencing can lead to further issues including loss of information central to the theoretical model (see Freedman, 2009). The error process can also be differenced, which then produces a non-invertible moving average error process, leading to issues with estimation (Plosser and Schwert, 1977). Furthermore, if we difference the variables the model can no longer give a unique long-run solution (see Asteriou and Hall, 2016).

in non-homogeneity (and then *can* lead to and often so even if it need not*,* under Rosser Jrs claim), instability and non-ergodicity, then transforming data to construct stationarity requires careful consideration and justification. One must be able to reasonably claim that the procedure is taking one to something hidden in the data generating process (that is also illuminating) rather than taking one further away from it. In the end this requires a combination of theoretical commitments and empirical claims in a position of fallibility. And this, of course, is a problem or challenge more basic than the specific problem of stationarity, or of co-integration. In a family resemblance sense it invokes wider issues raised by Ziliak and McCloskey (2008); by which we do not mean statistical significance in particular, but rather the significance of statistics. That is, the need to go beyond the statistical approach that is applied, to fully justify a statistically based argument in a way that does not simply reduce to: *this is the common practice and we have applied it*.

So, our initial point begins from, transforming data requires understandings and justifications of what is done and why. This may seem trivial and perhaps obvious. Yet it is not itself typical of common practice to routinely demonstrate a good understanding of the context of the data generating process and the genuine limits of what is done to the data.[10] This can have real consequences in particular circumstances. If we step back from co-integration to the more basic procedure of transformation of data for stationarity in general then it should be clear that homogeneity-stationarity underpins any coefficients that are derived, and so the derivation of those coefficients must be adequately understood and defended.[11] This is in addition to standard problems of model specification (measurability, completeness, seperability/additivity and independence, ordering, transitivity, implicit truth value claims based on tacit assumptions regarding non-change within the error term etc; see Freedman, 2009). This returns us to the problem of unit roots and policy for central banking as a particular circumstance.

Central bank inflation targeting and the creation of unit roots: a practical contradiction

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[10] Ultimately, this must extend to ontological issues, though one may not like the term and one need not be an adherent of any current specific form of ontological position in economics. It is worth noting that the most self-aware attempts to justify the adequacy of statistical analysis first ask: what is the nature of the economy that we then seek to analyse? One need not agree with the mainly negative position on mathematics often attributed to Lawson to recognize the primary nature of this question. Arguably one must have a response to Lawson's critique in order to have a basis to proceed. The range here has been wide in economics (see later examples, but also Downward, 2003).

[11] At root the dataset which is supposed to be representative of the data generating process that we assume we can know has manifested as non-stationary, and thus non-mean reverting and non-homogeneous. Intuitively this leads to the inference that the *process* generating the data is irregular to a degree that results in this first order non-homogeneity. For investigation to be meaningful there must be something that then arises within the data generating process to enable the application of statistical analysis (which can then be deemed appropriate). This is where a great deal of dispute has arisen. To what degree and in what sense is there an opaque order? It is a primary point recognized at the beginning of introductory econometric textbooks that stationarity is a step in processing data to enable the disentangling of causal factors according to econometric techniques (econometricians typically speak of 'well behaved data'). Unfortunately this usually and quickly gives way to a focus on various procedures to process data and test the processing: linearity, normality etc. This becomes an introverted focus on what is appropriate *to* statistical analysis. This point of departure is subtly different than: is the statistical analysis appropriate *for* its point of reference. This in turn has governed a great deal of the recognition of problems (alchemy and the con) and has, thereafter, structured proposed solutions (e.g. distribution-seeking iterative work along the lines of Hendry).

Modern central banking is focused on price stability and typically involves targets. Targeted inflation takes a positive value. If we return to the AR model Equation (1) in the previous section:

$$Y_t = \emptyset Y_{t-1} + e_t$$

And, if we specify the model as inflation and the inflation target is set at 2% and this is *consistently achieved* then Y_t will become equal to Y_{t-1} and $\emptyset$ will become 1 and e_t zero. Since $(\emptyset)$ becomes 1 there is a unit root. Since this will manifest in the original data series, transforming the data series to achieve stationarity thus creates a statistical point of departure that is consistently at odds with the consequences of a core aspect of real central bank policy. Of course, it is rare that target inflation levels are actually achieved and central banks are not intending to create a unit root consequence. They are engaged in a dynamic adaptive strategy where they are seeking to shape the future pathway of inflation with reference to economic growth and (increasingly consciously) in the context of financial stability.[12] Central banks constantly overshoot and undershoot the inflation target. Nevertheless, once it is recognized, the mismatch or practical contradiction is necessarily a cause for concern. The very point of policy involves non-stationarity and, in so far as convergence occurs, it is on a unit root outcome. As such, given that non-stationary data series cannot be homogeneous, an immediate question mark is placed regarding the nature of any insights to be gained from transforming the data. There is an immediate challenge posed in terms of links to the data generating process, and there is a subsequent more prosaic problem of the adequacy of any precision in the statistical information drawn on the basis of the differenced data. The former of these two points leads back to Davidson's concerns posed in terms of ergodicity and policy. However, if we take the latter first…

Consider the following table, which sets out UK Consumer Price Index data and its first difference 2004 to 2015:

Consumer Price Index (2004 – 2015)

Year	CPI(Y_t)	ΔY_t	Year	CPI(Y_t)	ΔY_t
2004	1.3	-0.1	2010	3.3	1.1
2005	2.1	0.8	2011	4.5	1.2
2006	2.3	0.2	2012	2.8	-1.7
2007	2.3	0.0	2013	2.6	-0.2
2008	3.6	1.3	2014	1.5	-1.1
2009	2.2	-1.4	2015	0.0	-1.5

Source: Office for the National Statistics (ONS; 2016)

If we again apply an Augmented Dickey Fuller test:

Augmented Dickey Fuller Test: CPI 2004-2015

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[12] To be clear, the only point being made here is that central banks do have inflation targeting and a price stability remit. This is not a comment on the adequacy of price stability measures or of the real role of central banking in contemporary financial architecture (are they, for example, forced to expand reserves to respond to private banks' capacity to create money through credit creation from which deposits arise?). Davidson also has a position on this (2006)

ADF test Stats	With intercept	p-value	Trend & Intercept	p-value	None	p-value
At level I (0)	-1.239	0.619	1.152	0.999	-0.777	0.358
At first diff I(1)	-3.227	0.046*	-3.152	0149	-3.385**	0.003

*5% level of Significance, **1% level of Significance.

And again, the results clearly establish that the initial series was non-stationary and then becomes stationary once transformed. Now, if we represent this graphically:

UK Consumer Price Index (2004 – 2015)
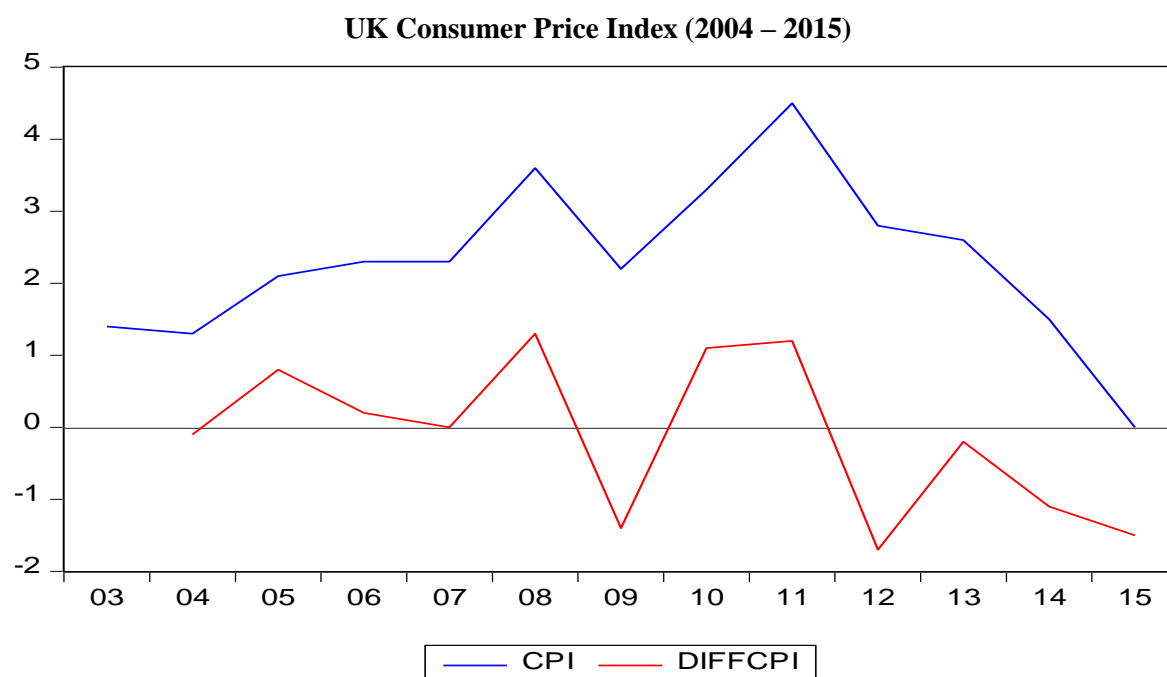
Source: Office for National Statistics (2016)

Though the pattern for CPI and differenced CPI are similar they are not the same. The differenced data takes negative values where the original data does not. Transforming the data for stationarity is done in order to expedite causal inference based on common analytical statistical methods. There will now be a difference in signs and magnitudes for any variables of interests between the original and differenced data. This creates a practical problem in shifting back and forth between datasets for the purposes of policy making. There is an adequacy and precision problem for the economist (though many will simply see a statistical problem where the original data leads to spurious models).[13] The response here should not simply be: *common practice is*. Good use of statistical method requires a further understanding of the context in which the statistical methods are applied. Significantly, understanding the context reduces the confidence one can have in the precision of findings. Methods may result in precision but the very act of applying methods makes it clear that one is seeking a 'best practice' among several options. Best practice is clearly interpretive. This of course is truistic, who does not know this about central banking? But nevertheless it is worth stating clearly, since it is an important reminder regarding economics as 'science'. The practice is not reducible to a technical matter only. Moreover, recognizing this and challenging common practice based on examples that draw attention to mismatches in policy

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[13] Consider this in terms of forecasting: The Bank of England has been using ARIMA modelling. A primary condition of ARIMA modelling is stationarity. If inflation targeting necessarily leads to a non-stationary data series, but the model forecasting future inflation levels requires the inflation series to be stationary, a potential weakness is built into the modelling procedure.

application can be a useful exercise for post Keynesians in making the case for different ways to address specific issues of theory and policy. From a methodological point of view there is a statistical analysis challenge *and* an argument for endogeneity.

'Common practice' as briefly referred to by Rosser Jr is not in and of itself a solution to other problems (which, to be clear, is not to suggest Rosser Jr claims that it is).[14] Common practice is not necessarily 'good' practice. This is particularly so where critical reflection on fundamentals ceases to occur. This is a point made in a slightly different way by Gallegati et al (2006) with reference to the kind of 'econophysics' that has developed drawing on the more complexity-oriented varieties of statistics Rosser Jr draws attention to. We do not note this in order to refute Rosser Jr's point that there are more sophisticated and different ways of exploring economic systems and activity (a point also made by O'Donnell). Gallegati et al are of course proponents. Rather we do so to highlight that reference to practice and method is always incomplete as a position. Common practice can be an additional potential source of problems. In the end one must consider what is achieved and foregone through the choices made. The ultimate frame of reference here is insight regarding economic reality (and then purposes such as policy to influence outcomes). We are, of course, used to thinking of this in more theory-specific ways for macroeconomics and central banking; for example, the intrinsic problems of the use of DSGEs, issues based on new classical or new Keynesian synthesis (e.g. Arestis and Sawyer, 2002, 2008, 2012; Docherty, 2011) and so forth (see Morgan, 2009). But the above should demonstrate there are other dimensions to this.

We now come to Davidson's concerns posed in terms of ergodicity and policy. It may be reasonable of Rosser Jr to remind us that there are different frames of reference for ergodicity -- for example, regarding its meaning and development within mathematics that then fed into economics. However, if we return to Davidson's primary concern with policy it should be clear that it contains an important insight that is relevant regarding central bank policy. If the very point of central bank inflation targeting has non-stationary consequences and, in so far as it is achieved, results in a unit root, then a primary characteristic of Davidson's account of ergodicity becomes relevant. The data series will be responding to policy within a reality that is demonstrably not reducible to a convergent statistical identity. There may be points of

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[14] To be clear, Rosser Jr's assessment of Davidson's argument is reasonable. However, since it is necessarily focused and attempts to draw together a great deal of material in a concise form based on a journal article word limit, it also has absences. Rosser Jr puts aside critical inquiry into the standard practices and attitudes he refers to. He provides no comment on the actual prevalence of non-homogeneity, non-stationarity and non-ergodicity. He recognizes that the reference point for these is also conditions and possibilities of the world over and above further considerations of subjective-objective probability in terms of the nature of that world. One could make more of this. Rosser Jr may be correct to note, as O'Donnell suggests, there is 'no guarantee at all of being able to really determine whether or not one is dealing with an ergodic system' (2015: p. 339). However, this is not a warrant for methods that assume homogeneity, stationarity and typically ergodicity. It generates a burden of proof issue for the adequacy of methods, rather than a reasonable presumption that one can adopt assumptions and so use standard practices, such as transformation via differencing. This is especially so where the methods impose more restrictive assumptions about reality that observation seems to render questionable. One might also note that Davidson seems reasonably justified in claiming O'Donnell's critique is tendentious. Whilst the technical issues that arise are not wrong per se, he approaches Davidson's position rather unsympathetically in search of contradiction rather than considering the general point at issue. O'Donnell dichotomises where polar positions do not necessarily apply. However, Davidson also contributes to the problem of meaningful communication by periodically shifting the terms of debate without recognizing this is what he is doing.

contact between policy goal and real world manifestation, but this is different.[15] As such, one cannot reasonably adhere to an ergodic concept where:

> [T]ime averages calculated from past data will converge with the statistical averages computed from any future time series. Knowledge of the future merely involves projecting averages based on past or current realizations to forthcoming events. The future is merely the statistical reflection of the past and economic actions are in some sense timeless. (Davidson, 1991: p. 132)

One might go further and argue that the non-differenced data series is likely to exhibit variation indicative of process. There may be some degree of stability, since economic activity is structured and purposive. Inflationary targeting is a prime example. But the future cannot be a simple expression of the past. Though it is not our main purpose to provide detailed comment on the Davidson-O'Donnell debate, Rosser Jr seems well-justified in claiming that for Keynes it was 'the lack of a probability distribution that is the foundation for fundamental uncertainty' (Rosser Jr, 2015: p. 337). Uncertainty can be 'both' epistemological (what one can know) and ontological (what one can know it about) (Rosser Jr, 2015: p. 347). Davis (2017) expresses this neatly in a recent paper, sometimes we do not know *because we cannot* know. The very nature of social reality can make this the case. In this sense, at least, Davidson's concerns remain highly relevant (though any nuanced and sympathetic reading of O'Donnell would acknowledge, *inter alia*, he does not dispute the importance of the concerns, rather the consistency of the claims used to address those concerns).

Ultimately, for post Keynesians it is the degree and basis of relative stability and the breakdown in that stability that is important. Issues of process, breakdown in stability and matters of path dependent open systems create challenges for the use of statistical analysis. Various scholars, influenced by Keynes, have addressed this in different ways.[16] For example, Nell and Errouaki's recent *Rational Econometric Man* (2013) seeks to build on the legacy of structural econometrics derived from the early Cowles Commission. The basic premise is that one can reconcile Keynes to econometrics, since institutions create sufficient stability for econometric techniques to be meaningfully applied to well understood economic contexts. There is a reciprocal theory-empirical nexus if one moves beyond timeless and unrealistic assumptions and mere functional relations (see also the special forum in *Review of Political Economy* focused on this, 28(3), 2016). There is some crossover here with Mark Setterfield's work (see Setterfield in Morgan, 2016), based on the existence of conditional closures created by periodised relatively stable institutions, practices etc, which enable an open systems ceteris paribus approach to modelling (OSCP). Both of these are sophisticated attempts to move beyond 'common practice', though neither is about banking. They

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[15] Such an identity would be difficult to interpret. It might imply that policy interventions today can only delay, but not change the long run optimal solution already predetermined by free markets (Davidson, 2012), so the identity was an epiphenomena of a deterministic system. This would further indicate that no additional causal intervention has occurred or is relevant. This is also odd in terms of symbolic logic since it both seems to entail and not entail a unit root ordered system.

[16] These are conceptual, not only technical, and so different than issues addressed as essentially technical: for example, time homogenous properties in Markov Chains ('memorylessness' etc). Markov introduces new ways in which stochastics are posed. In quantitative finance this has led to new models for asset prices. A lot of this is problematic based on a general equilibrium framework.

implicitly recognize that good use of statistical method requires a further understanding of the context in which the statistical methods are applied and both are intrinsically sensitive to the limits of modelling and statistical technique. Both recognize that what is done is interpretive and must be justified. And both take seriously the problem of realism as a final frame of reference. As such, both are attempts to address the basic problem that economics continues to default to an understanding of science as simply the exploration of regularity in events.[17]

A further step along this line of inquiry is to consider the irregularity aspects of economic practice. The recent central bank policy frame of Forward Guidance in the UK provides a convenient way to illustrate the significance of irregularity as something post Keynesians should be concerned with. This too can be a way to go beyond 'common practice'. Exploring irregularity helps to explain why the future is not a simple mirror of the past and so also helps explain why data is non-stationary and how this is of real significance.

UK central bank Forward Guidance and irregularity

Following the appointment of Mark Carney as Governor, the Bank of England introduced Forward Guidance in August 2013 (Morgan and Sheehan, 2015). Central bank communicative strategies to shape expectations and subsequent behaviour are not new, and Forward Guidance has fitted within this approach.[18] Forward Guidance provided a set of commitments that the Bank stated would be met prior to any interest rate change (the base rate, which was then at 0.5%). The initial statement was that the base rate would not increase until unemployment fell below 7%. However, the Bank also stated the base rate *may* rise if inflation (CPI) continued to exceed 2.5% (based on the target of 2% -- at the time Forward Guidance was introduced the CPI was 2.8%). Alternatively, it may *not* rise if a significant event (using the standard language of external shock) was to occur (renewed financial crises within Europe or domestic problems such as a switch to sudden deflationary pressures). In February 2014, this Forward Guidance was restructured as Mark II. The core commitment now focused on 'spare capacity'. Spare capacity was defined as the non-inflationary ability of the economy to absorb the previously unemployed as growth occurs. The reference point for this capacity was changes in investment and productivity, in so far as these are assumed to affect an equilibrium rate of unemployment. According to the Bank, a lower equilibrium rate increases spare capacity and reduces the need to raise the base rate (as a means to forestall inflationary pressures).

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[17] How successful this can be, of course, remains a matter of dispute. Further innovative approaches to the problem are to be found in philosophy of statistics. For example, Gillies (2000) provides an excellent overview of the history and grounding assumptions of varieties of probability in order to advocate pluralism. One of the positions he sets out is propensity probability, which has been taken up by a variety of scientific realists working mainly in sociology and social policy, but also economics. Case based propensity seeks or constructs datasets for states in relations (such as medical records). Since the approach is based on actual cases that can be recorded through time in different categories the approach is different than frequency or sample approaches, and so does not require a tacit representative subject or default to requirements of a qualitatively invariant aspect of the social world (at least in the sense implicit to samples based on frequency constructs). Techniques like cluster analysis can explore the dataset under different classifications, and so changes of state for relations can be expressed through time. Advocates in general tend to view social measurement as clues to mechanisms for abduction etc. For innovative work and argument in the UK see the four volume edited collection Olsen (2010), and also Byrne and Ragin (2009).

[18] The Fed introduced something different in January 2012: a dot plot of each member of the Federal Open Market Committee's projected fed funds rate for the next three years, deemed necessary to achieve stated economic goals. This graphs FOMC sentiment for analysts to then interpret.

The Bank of England has not repudiated Forward Guidance. However, since 2014 media interest in the specific policy conditions, as a statement of commitment, has gradually waned. Moreover, the way in which members of the interest rate setting body, the Monetary Policy Committee (MPC), use the term has also shifted. Members construct theory and evidence based argument for purposes, but within those purposes the term Forward Guidance is now typically used more loosely (lower case forward guidance) to simply mean the communication of what may subsequently happen to the base rate. For example, in a recent speech MPC member and Bank of England Chief Economist Andrew Haldane stated, 'That is why I supported forward guidance on a further lowering of interest rates in the run-up to the end of the year, provided the economy evolved in line with the MPC's projections for inflation and output at the time,' (Haldane, 2016: p. 13). This shift to looser usage is indicative of a tacit acknowledgement that Forward Guidance has simply failed to do what it was intended to. Forward Guidance was introduced as an attempt to firm up the communicative aspect of the MPC's role by providing some tighter evidential basis for a subsequent *increase* in the base rate from its historic low. The main consistency communicated until the Brexit referendum, June 2016, was that the base rate would *rise* and that this would be to a maximum of 3% in 2017.

From its introduction Forward Guidance had a number of unintended consequences. Guidance was interpreted as a test to be met or threshold to be passed, which would then trigger a base rate change decision. However, the calculation and credibility of the components of Guidance immediately became a focus of attention. Forward Guidance Mark I centered on unemployment falling below 7%. Unemployment in mid-2013 was 7.8% and the Bank's projections forecast net job creation of 250,000 per year for 36 months, implying the threshold would be achieved mid-2016. But by January 2014 unemployment had already fallen to 7.1%. The Bank thus found itself having to justify both the inaccuracy of its projection and why it would *not* be imminently increasing the base rate. The latter was not unreasonable, since evidence suggested there were widespread problems of debt servicing vulnerability in the economy, which made interest rate sensitivity an issue. Additionally, corporate investment remained historically low, and problems in the Eurozone and bank balance sheet issues remained recognized concerns. Forward Guidance Mark II, focused on spare capacity, was then introduced to replace Mark I. Putting aside for the moment the adequacy of an equilibrium unemployment rate concept, spare capacity hinged on weightings that could be influenced by underlying factors affecting wage growth: increased self-employment, part-time employment, zero hour contracts, retirees returning to work etc. These responses to the employment environment after the Global Financial Crisis involved new sets of relations and contexts, and so different interpretations of the future development and significance of factors that would affect 'spare capacity' were possible. As such, spare capacity did not embed as a tighter more evidence based framework for a base rate decision. Instead, Mark II resulted in further skepticism from those it was intended to influence regarding what exactly might cause the MPC to raise the base rate.

It should not, of course, be neglected that Forward Guidance has sat awkwardly with the mandate of the MPC. The MPC sets the base rate based on a vote. The Committee is set up as a way to balance rule-based and discretionary approaches within the environment of Bank of England independence from direct government intervention in monetary policy. Ostensibly, Forward Guidance introduced an evidence-based commitment that enabled other economic

actors to have greater confidence in interpretations of what the MPC might do. At the same time, Forward Guidance has no statutory force (requiring MPC members to act) and involves what are essentially numerous caveats and ambiguities. This creates a further tension, since economic actors must assume that the core commitment will follow as claimed -- falling unemployment in Mark I and then narrowed spare capacity in Mark II will trigger a base rate rise. But they simultaneously recognize that this continues to not happen. This in turn creates grounds for arbitrage as well as loss of credibility, both of which add to contextual problems that may be further reasons why a base rate rise does not occur (since responses include speculative volatility in capital markets, reduced confidence for current and future investment and other effects). So, a curious situation arises where the Bank is treated as though it had complete control over the economic environment (e.g. priced in base rate changes that then do not occur), whilst it is simultaneously criticized because it manifestly does not have that control (and the result is that the combination has complex effects on behaviour feeding back on the general environment). In Davidson's terms: 'economics actions' are not 'timeless'.

This situation has subsequently evolved in the context of Brexit (see Morgan, 2017). In the run up to Brexit, the Bank highlighted that the UK seemed to be constructing its own 'shock' (and forecasts based on this have now become something for which the Bank is criticized).[19] Since Brexit, the likely date of any base rate rise has been put further back into 2019 or later. The only actual change to the base rate has been, rather than a rise, a reduction to 0.25% (combined with a further £70 billion in quantitative easing to add to the prior £375 billion in asset purchases). Events have clearly overtaken any attempt to impose a tighter test for an interest rate decision. Instead, it is the softer acknowledgements, caveats and recognitions that additional context matters that have been decisive so far. This indicates something important about policy, irregularity and open systems in process. Something cumulative and shifting is occurring in terms of causal process. Path dependency may be able to account for this. However, an equilibrium approach can only distort our understanding of what is occurring.

Clearly, none of the above will come as a surprise to anyone who has read Keynes. But it is an important reminder of the continuing relevance of core insights. In the last section we set out the unit root problem inherent in central bank inflationary targeting. We did so in order to draw attention to problems of common practice, to which post Keynesians might respond in a variety of ways. The example of Forward Guidance is instructive for several reasons. It provides an illustration of the role of conventions in a situation prone to uncertainty. The qualitative aspects of this are not separate from the technical specification of models or matters of statistical analysis. They are inquiries into a common social reality, a commonly constructed economy (see Davis, 2017; Dow, 2012b, 2015; Latsis et al. 2010; Lawson, 1988, 1994). In acknowledging that uncertainty can be *both* epistemic and ontological one must have some reasonable account of what it is that means the future is not a simple expression of the past transmitted via the present (subject at the extreme to a mirror thesis in ergodic form).

From a Keynesian and institutional point of view, Forward Guidance is an attempt to create rules, and so influence the construction of conventions. Conventions are a key way in which

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[19] However, it was the UK Treasury that received most of the initial criticism. Its initial medium term forecast was that Brexit would reduce GDP by 3.6% over the first two years, and its longer-term steady state comparison claimed that the UK would be 3.4% to 9.5% worse off outside the EU in 2030. The former assumed an immediate trigger of Article 50, and a sharp reduction in consumption and business revenue.

some *degree* of stability is introduced into an economic system. However, in the case of Forward Guidance nothing turned out as intended (increased certainty) or as anticipated (actual outcomes). Forward Guidance involved an inherent tension - an intent to shape the future by an organization aware that economic reality exceeds both the domain of its organization and its control. Forward Guidance was conceived as a communicative act. An act introduced *into* an existing system already subject to a wide variety of problematic features. Problematic features might be considered specific to a post global financial crisis world (the continued influence of financialisation, austerity as an induced aggregate demand deficiency etc). However, the problematic features also have further general characterisations mediated by distrust and scepticism. An economic system has multiple actors, and they are positioned differently. They *may* follow conventions, but they are also *all* reflexive entities, and so the difference between them is not just a possible information asymmetry, but rather the capacity to respond differently to a system that also may be changing, giving them reasons to respond differently. An economic system in its parts and whole is thus one of differentiation, feedback and causal reciprocation. It is a non-linear process in which there can be more or less uncertainty, and where there is always the potential for fundamental uncertainty. No determinate basis for central bank interest rate rises has emerged in the UK. One can know that they are possible and that they will occur eventually, but decision making has been characterised by a series of 'it depends' and 'we don't knows'. A kind of hypothetical 'one can know' (a possibility) is, therefore, quite different than *one knows that*. The connotation of 'one can know' requires a level of generality that removes it from time in any meaningful sense of specification. With reference to the ergodicity debate, this would seem to add credence to an ontological basis, in addition to an epistemic one, for fundamental uncertainty - one does not know because one cannot (in any more specific sense) know.

Clearly, we have good reasons to want to impose stability on our economic system, since reliability and control-seeking are core to the very point of society. But everything that we do confounds our best efforts to impose stability, precisely because of what we are as entities. If it did not then there would be no reason for active economic policy (to respond to failure based on some goal), no basis to the marginal efficiency of capital argument (subject to the state of confidence), and no credence in anticipating average behaviour for speculative investment (exploiting the norm and wilfully repudiating 'fundamentals'). The very nature of real 'micro' prevents a 'macro' of smooth convergent equilibria. Conversely the absence of this smooth convergent 'macro', reconceived as an operative system in process feeds back into the reflexivity of agents (and so an effective critique of DSGEs can take this point of view). One might go further and argue that non-homogeneity, instability and non-ergodicity (in the mirror thesis sense) are statistically observable features of this process. It then becomes important to undertake qualitative post Keynesian work that explores particular convention-seeking activity as a way to explain these observable features. This allows one to go beyond 'common practice', and rather than just or only manage the problems of possible mismatches to method, work with the more basic ontological claims. Concomitantly, a post Keynesian institutional analysis along these lines can be an effective way to address points of weakness in actual policy.

A post Keynesian institutional analysis provides one way to explain why economic reality is not captured by equilibrium accounts. It thus adds further plausibility to path dependency and its observed variation as an alternative account. *Inter alia* it explains why data is not

stationary and why processes are not ergodic in the mirror thesis sense. In the case of central bank activity it also helps explain why actual policy results in non-stationary outcomes that are also rarely unit root, since policy is consistently undermined by the complexity of economic reality. One might, therefore, argue that post Keynesian institutional analysis can (and to some degree already does based on the work of Dow and others) add to the diversity, distinctiveness and constructive critical repertoire of post Keynesian work. In so doing, it can provide useful further insights into current central bank activity.

Post Keynesian institutional analysis

Central bankers have technical proficiencies, but it would be a misrepresentation to characterize them as reductive technocrats or modelers. MPC members, as key decision makers, use varieties of evidence sources and are fully aware that there are limits to the insights to be gained from models. For example, Michael Saunders recently stated, "I stress again that the actual path of policy is a choice for the MPC at the time - policy decisions are not set by a computer model but are taken after extensive analysis and debate at the Bank of England. Moreover, [models] are just simulations: the economy's response to monetary policy changes may well turn out to be greater or smaller, faster or slower, than these models imply" (2016, p. 10). However, MPC members like any other persons, are products of their socialization, so the attitudes they hold, and the theory and evidence forms they are exposed to and are open to are still significant. An 'open mind' is not empty and nor is it lacking in influential constraint. The post global financial crisis is sometimes referred to as one of a radicalization of central bank practice - a new more activist position, indicative of significant change (a new 'normal'; e.g. Mersch, 2016). In many ways the Bank of England has become more self-critical and, due in part to Andrew Haldane, more open to a wider range of methods and argument.[20]

However, the main outcome of this so far has been the exposure of tensions because of partial acknowledgements but continued reliance on previous assumptions and approaches. The Bank of England now acknowledges a more sophisticated account of money creation through commercial bank's capacity to create credit (see McLeay et al, 2014). This would seem to be a step away from money as a neoclassical 'neutral veil', analytically distinct from the 'real economy'. However, Haldane recently stated, "At least over the longer run, it is reasonably clear that monetary policy is ill-equipped for the task of either expanding the economic pie or altering the way it is sliced. Over those horizons, nominal things like money and monetary policy cannot affect real things like productivity, skills and investment. In the language of economics, money and monetary policy are neutral in their impact" (2016, p. 8). This, of course, is familiar as a new Keynesian synthesis position. The Bank regularly publishes research innovating along new Keynesian lines. For example, addressing monetary transmission mechanisms by treating the pricing of futures in relation to anticipated Bank interest rate activity as a source of monetary shocks, modeled using VARs (Miranda-Agrippino, 2016). The Bank remains reliant on DSGE modeling for its overall macroeconomic framework, using an equilibrium concept of unemployment as well as an equilibrium interest rate (see Carney, 2016, p 9).[21] One might argue then, that there is a basic

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[20] For example, the Bank of England commissioned Fed researcher David Stockton in 2012 to produce an analysis of the Bank's forecasting performance. This has also been followed up by Bank commissioned research by Andrew Wood at University of Essex in 2013.

[21] As Haldane states: "To pinpoint the specific contribution made by monetary policy, we need a model of the economy. By using the Bank of England's suite of macro-economic models, we can quantify the specific role

disconnect: critique can still result in perpetuation of problematic theory. This is typified by former IMF director of research Olivier Blanchard's recent critique-yet-defense, in which he states, "I see the current DSGE models as seriously flawed, but they are eminently improvable and central to the future of macroeconomics" (2016, p. 1).

The disconnect has various aspects. In addition to anything else one might argue, reconciling a DSGE model sits awkwardly with what Bank members must know about the reality of 'spare capacity' based on what we have previously noted. This too is a source of tension between what central bankers must be aware of, in terms of the complexity and ambiguity regarding what has and will happen, and what the model (as a reconcilable set of equations) allows for. Furthermore, MPC members are clearly aware at some level of the problem of conventional behaviour. For example, Gertjan Vlieghe recently stated, "There is much to learn in the coming quarters and years about the nature of the UK's new trading arrangements with the EU, and therefore the likely impact on the economy. What really matters here is not what the MPC thinks will be the new trading arrangements, but what everyone else thinks" (2016, p. 3). Concomitantly, the disconnect ultimately extends to the concept of uncertainty. Uncertainty is a concept that central banks, including the Bank of England, are increasingly concerned with. In a recent speech the then MPC member Kristin Forbes discusses this:

> Uncertainty is the modern equivalent of a "whipping boy" for economics […] today, "uncertainty" is often blamed for any expected weakness in company earnings or broader economic growth […] Even the MPC minutes have recently set a record of averaging 15 mentions of uncertainty each meeting, up sharply from averaging less than 6 times per meeting since the Committee was created. Uncertainty is a concept that is easy to talk about, but hard to measure [...] Carney (2016) highlights that uncertainty can come in many forms - such as economic uncertainty, geopolitical uncertainty, and policy uncertainty. Economists try to differentiate between model uncertainty (when an agent is uncertain about the correct model of the economy), Knightian uncertainty (when the distribution of outcomes for a variable is uncertain), and Bayesian uncertainty (when the shape of the parameter distribution is uncertain). (Forbes, 2016, pp. 2 & 5)

It is curious that Forbes refers to Knight but not to Keynes. However, putting this aside, what is more interesting is the actual impact of thinking about uncertainty on what the Bank does. There are three key aspects to this. First, though Forbes acknowledges uncertainty is 'hard to measure', the main focus of Bank concern with uncertainty is probabilistic. The Bank produces different evolutionary pathways for the economy in what have become familiar fan diagram forms. Second, uncertainty is modeled using uncertainty indicators (see also Broadbent, 2016, p. 5). For example, there is more uncertainty if investment ratios fall relative to anticipated returns, if 'waiting' or 'delays' extend in terms of given types of economic activity (buying a house etc). Using a range of such indicators allows the Bank to suggest there is greater uncertainty in the economy. Clearly, this is a highly restrictive concept of uncertainty. It simply involves inference regarding confidence based on assumed reasons why activity does or does not occur. Third, uncertainty is used to justify why things did not turn out as models predicted. This is one of the main ways the term is used in MPC meetings. This can initially appear to be a tacit reference to Keynesian fundamental uncertainty. However, if you think of it in terms of disconnects where given theory forms

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played by monetary policy in explaining movements in incomes and jobs, albeit rather imperfectly given that all models are imperfect" (Haldane, 2016, p. 9).

have persisted, then the underlying function of the use of the term is to insulate new Keynesian approaches from falsification. Uncertainty is simply error, a reason why the models were not vindicated. This is rather different than a frame of analysis that questions a model's realism and relevance and markedly shifts the meaning-through-usage away from a Keynesian position.[22] It is, ironically and inadvertently, a usage that prevents the Bank taking a further step towards an open system path dependent account of an economy in process.

Consider this in terms of the argument typically stated for forecasting: forecasting is a necessary guide to the future. It prevents policy becoming merely impressionistic or a product of intuition or ignorance. It gives a baseline for decision-making, introducing discipline. The substructure of this argument is that forecasting is built on a learning process. Identifying how something went wrong in prior forecasting improves understanding of the economy, and so leads iteratively to improvements in theory that then lead to better models, and thus better interventions and so better (if never perfect) forecasting. But consider what is intrinsic to the substructure of learning -- the process is not impeded or subverted. However, the use of uncertainty as a justification can become a barrier, which short-circuits learning because theory and method do not respond to failure at a fundamental level. This seems a very basic contradiction for central banking, the nature of an open system periodically undermines forecasting, but forecasting is insulated from assimilating this insight because of the use to which a concept of uncertainty as unavoidable error can be put in an open system. The important point here is that it is through an institutional analysis that this becomes apparent. Clearly, therefore, post Keynesian constructive critique of central bank policy can be augmented by an institutional analysis. This is another productive way to go beyond 'common practice'.

Conclusion

Our point of departure in this paper was a specific problem posed in terms of central bank policy and what this indicates about statistical analysis. 'Common practice' involves transforming data for stationarity. However, the very point of inflation targeting is to achieve a positive value that then becomes part of the data series. This necessarily introduces non-stationarity and, if the target is achieved, introduces a unit root. In addition to providing a basis for some further parallel insights regarding the ergodocity debate, consideration of this problem allows a series of further points to be made. Most importantly, many post Keynesians have positioned their work as explicitly seeking to manage rather than merely evade basic problems for statistical analysis. The reference point for this is commonality based on a conception of an economy as a path dependent open system subject to causal process. Recognizing this also opens up institutional aspects of post Keynesian research. Forward Guidance illustrates how this is not a separate issue from technical matters of how we conceive of an economy, but rather intrinsic to it. This too can become part of the constructive critique of central bank activity. It can add to the diversity, distinctiveness and constructive critical repertoire of post Keynesian work.

References

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[22] An associated language of upward and downward revisions gives the impression the problem is always data absence - or some statistical error that is later accommodated. This is quite different than accepting the model cannot be what it is constructed to be.

Álvarez, M. and Dirk, E. "Samuelson and Davidson on ergodicity: A reformulation." *Journal of Post Keynesian Economics*, 2016, 39 (1), 1-16

Angrist, J. and Pischke, J. "The credibility revolution in economics: How better research design is taking the con out of econometrics." *Journal of Economic Perspectives,* 2010, 24 (2), 3-30

Arestis, P. and Sawyer, M. "The Bank of England macroeconomic model: Its nature and implications." *Journal of Post Keynesian Economics,* 2002, 24 (4), 529-545

-- "A critical reconsideration of the foundations of monetary policy in the New Consensus Macroeconomic Framework." *Cambridge Journal of Economics*, 2008, 32 (5), 761-779

-- "The 'new economics' and policies for financial stability." *International Review of Applied Economics,* 2012, 26 (2), 147-160

Asteriou, D. and Hall, S. *Applied Econometrics*, Basingstoke: Palgrave Macmillan, 2016

Blanchard, O. "Do DSGE models have a future?" Peterson Institute for International Economics, 2016, PB 16-11

Broadbent, B. "Uncertain times." Bank of England, speech at the Wall Street Journal, The News Building, London, 2016, 5th October, http://www.bankofengland.co.uk/publications/Pages/speeches/default.aspx

Byrne, D. and Ragin, C. editors. *The Sage Handbook of Case-Based Methods* London: Sage, 2009

Caballero, R. "Macroeconomics after the crisis: Time to deal with the pretence-of-knowledge syndrome." *Journal of Economic Perspectives*, 2010, 24 (4), 85-102

Carney, M. "The spectre of monetarism." Bank of England, speech/Roscoe Lecture, Liverpool John Moore's University, 2016, 5th December, http://www.bankofengland.co.uk/publications/Pages/speeches/default.aspx

Davidson, P. "Is probability theory relevant for uncertainty? A Post Keynesian Perspective." *Journal of Economic Perspectives* 1991, 5 (1), 129-143

-- "Reality and economic theory." *Journal of Post Keynesian Economics* 1996, 18 (4), 479-508

-- "Is economics a science? Should economics be rigorous?" *Real World Economics Review,* 2012, 59, 58-66

-- "Response to Lavoie, King and Dow on what Post Keynesianism is and who is a Post Keynesian." *Journal of Post Keynesian Economics*, 2005, 27 (3), 393-408

-- "Can or should a central bank inflation target?" *Journal of Post Keynesian Economics*, 2006, 28 (4), 689-703

-- "A rejoinder to O'Donnell's critique of the ergodic/nonergodic explanation of Keynes's concept of uncertainty." *Journal of Post Keynesian Economics*, 2015, 38 (1), 1-18

Davis, J. "The continuing relevance of Keynes's philosophical thinking: Reflexivity, complexity and uncertainty." *Annals of the Fondazione Luigi Einaudi: An Interdisciplinary Journal of Economics, History and Political Science,* 2017, forthcoming

Dickey, D. and Fuller, W. "Distribution of the estimators for autoregressive time series with a unit root." *Journal of American Statistical Association,* 1979, 74 (366), 427-431

Docherty, P. "Keynes' analysis of economic crises and monetary policy in the *General Theory*: Its relevance after 75 years." *Review of Political Economy,* 2011, 23 (4), 521-535

Dow, S. "Mainstream economic methodology." *Cambridge Journal of Economics*, 1997, 21 (1), 73-93

--   "Axioms and Babylonian thought: A reply." *Journal of Post Keynesian Economics*, 2005, 27 (3), 385-391

--   *Foundations for New Economic Thinking: A collection of essays*. Basingstoke: Palgrave Macmillan, 2012a

--   "Policy in the wake of the banking crisis: Taking pluralism seriously." *International Review of Applied Economics*, 2012b, 26 (2), 161-175

--   "Addressing uncertainty in economics and the economy." *Cambridge Journal of Economics* 2015, 39 (1), 33-47

Downward, P. editor. *Applied Economics and the Critical Realist Critique*. London: Routledge, 2003

Freedman, D. *Statistical Models: Theory and Practice*. Cambridge: Cambridge University Press, 2009

Forbes, K. "Uncertainty about uncertainty." Bank of England, speech to JP Morgan Cazenove Best of British Conference, London, 2016, 23rd November
http://www.bankofengland.co.uk/publications/Pages/speeches/default.aspx

Gallegati, M. Keen, S. Lux, T. and Ormerod, P. "Worrying trends in econophysics." *Phsyica A*, 2006, 370, 1-6

Gillies, D. *Philosophical Theories of Probability*. London: Routledge, 2000

Granger, C. and Newbold, P. "Spurious regressions in econometrics." *Journal of Econometrics*, 1974, 2 (2), 111-120

Haldane, A. "One car, two car, red car, blue car." Bank of England, speech to Materials Processing Institute, Redcar, 2016, 2nd December
http://www.bankofengland.co.uk/publications/Pages/speeches/default.aspx

Hendry, D. "Econometrics -- Alchemy or science?" *Economica,* 1980, 47 (188), 387-406

Keane, M. "Structural vs. atheoretic approaches to econometrics." *Journal of Econometrics,* 2010, 156, 3-20

King, J. *A History of Post-Keynesian Economics since 1936*. Cheltenham: Edward Elgar, 2002.

Latsis, J. de Larquier, G and Besis, F. "Are conventions solutions to uncertainty? Contrasting visions of social coordination." *Journal of Post Keynesian Economics*, 2010, 32(4): 535-558

Lawson, T. "Probability and uncertainty in economic analysis." *Journal of Post Keynesian Economics,* 1988, 11 (1), 38-65

--   "The nature of Post Keynesianism and its links to other traditions: A realist perspective." *Journal of Post Keynesian Economics,* 1994, 16 (4), 503-538

--   *Essays on the Nature and State of Modern Economics.* London: Routledge, 2015

Leamer, E. "Let's take the con out of econometrics." *American Economic Review,* 1983, 73 (1), 31-43

McLeay, M. Radia, A. and Thomas, R. "Money creation in the modern economy." *Bank of England Quarterly Bulletin*, 2014, 54 (1), 14-27

Mersch, Y. "Grasping the new normal of the banking industry -- A view from a European Central Banker." Speech by ECB Board Member at European Financial Congress, Sopot, Poland, 14th June, 2016 Available at:
https://www.ecb.europa.eu/press/key/date/2016/html/sp160614.en.html

Miranda-Agrippino, S. "Unsurprising shocks: information, premia, and the monetary transmission." *Bank of England Staff Working Paper*, 626, 2016 Available at:
www.bankofengland.co.uk/research/Pages/workingpapers/default.aspx

Morgan, J. "The limits of central bank policy: Economic crisis and the challenge of effective solutions." *Cambridge Journal of Economics,* 2009, 33 (4), 581-608

--   editor, *What is Neoclassical Economics? Debating the origins, meaning and significance.* London: Routledge, 2016

--   "Brexit: Be Careful what you wish for?" *Globalizations,* 2017, 14 (1), 118-126

Morgan, J. and Sheehan, B. "The concept of trust and the political economy of John Maynard Keynes, illustrated using central bank Forward Guidance and democratic dilemma in Europe." *Review of Social Economy,* 2015, 73 (1), 113-137

Nell, E. and Errouaki, K. *Rational Econometric Man: Transforming Structural Econometrics.* Cheltenham: Edward Elgar, 2013

O'Donnell, R. "A critique of the ergodic/nonergodic approach to uncertainty." *Journal of Post Keynesian Economics,* 2014/15, 37 (2), 187-209

Olsen, W. editor, *Realist Methodology: Volume 1-4* London: Sage, 2010

Plosser, C. and Schwert, W. "Estimation of a non-invertible moving average process: The case of over-differencing." *Journal of Econometrics,* 1977, 6 (2), 199-224

Romer, P. "Mathiness in the theory of economic growth." *American Economic Review,* 2015, 105 (5), 89-93

Rosser, J. Jr. "Reconsidering ergodicity and fundamental uncertainty." *Journal of Post Keynesian Economics*, 38 (3), 331-354

Sala-I-Martin, X. "I just ran two million regressions." *American Economic Review*, 1997, 87 (2), 178-183

Samuelson, P. "What classical and neoclassical monetary theory really was." *Canadian Journal of Economics*, 1968, 1 (1), 1-15

Saunders, M. "The economic outlook." Bank of England, speech to Institute of Directors, Manchester, 2016, 5th October, http://www.bankofengland.co.uk/publications/Pages/speeches/default.aspx

Setterfield, M. "Heterodox economics, social ontology and the use of mathematics." pp. 221-237 in Morgan, editor, 2016.

Spanos, A. "Revisiting Haavelmo's structural econometrics: Bridging the gap between theory and data." *Journal of Economic Methodology,* 2015, 22 (2), 171-196

Vlieghe, G. "Umbrellas don't cause rain." Bank of England, speech at Sheffield University, 2016, 28th November, http://www.bankofengland.co.uk/publications/Pages/speeches/default.aspx

Ziliak, S. and McCloskey, D. *The Cult of Statistical Significance*. Michigan, The University of Michigan, 2008

Zuidhof, P. "Thinking like an economist: The neoliberal politics of the economics textbook." *Review of Social Economy*, 2014, 72 (2), 157-185