MEASURING BUSINESS CYCLES: THE REAL BUSINESS CYCLE APPROACH AND RELATED CONTROVERSIES

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ABSTRACT

This paper presents and discusses the business cycles measurement carried out by the real business cycle (RBC) approach. It shows how the controversy lodges within and outside the RBC frontiers. It also shows how the methodological debate that took place in the 1940s about the relationship between theory and measurement, is revived in modern discussions of business cycles measurement. The paper concludes that this relationship still raises fierce contention.

Keywords: *Business cycles; Real business cycles; Measurement.*

RESUMO

Este artigo apresenta e discute a medição dos ciclos económicos realizada pela abordagem dos ciclos económicos reais (RBC). Mostra como a controvérsia se instala dentro e fora das fronteiras dos RBC e como o debate metodológico dos anos 40 sobre a relação entre teoria e medida revive nas actuais discussões sobre a medição dos ciclos económicos. O artigo conclui que esta relação é ainda motivo de acesa contenda.

Palavras-chave: *Ciclos económicos; Ciclos económicos reais; Medição.*

1. INTRODUCTION

There has been a substantial amount of research on real business cycles (RBC) since the seminal paper of Finn Kydland and Edward Prescott published in 1982[1]. This research program is based on the belief that stochastic technological shocks are a major source of business fluctuations, which represent the optimal response to such shocks. Given the technology available at the time (in terms of economic theory), it is not surprising that Kydland and Prescott have used the stochastic version of the neoclassical growth model as the basis to RBC theory. According to them, long-term growth and short-term fluctuations in output should be explained by the same theory. Actually, Kydland and Prescott (1996, p. 72) come to the conclusion that 'modern business cycle models are stochastic versions of neoclassical growth theory'.

However, this apparently unifying agenda seems to have been overwhelmed by the subsequent emphasis on short-term fluctuations, both in the empirical and theoretical research. At the empirical level, business cycle measurement has even become a prominent part of the RBC research agenda.

Measuring business cycles is certainly essential to the establishment of business cycle stylised facts. These, in turn, are seen as essential to the development of the modern business cycle theory, just as the stylised facts of growth were to the neoclassical growth theory[2]. But the measurement of the business cycle is a controversial issue even within the RBC literature, because of the need to separate growth from cycle, despite the initially unifying purposes. There is no consensus on how to detrend the data, and the so-called business cycle stylised facts are sensitive to the adopted procedure. Hence, this has become not only a controversial issue in RBC theory itself, but a subject of criticism by competing approaches, as well.

After all, as the RBC theorists[3] point out, the relationship between theory and measurement of economic activity brings us back to the methodological debate initiated by Koopmans' (1947) discussion of Burns and Mitchell (1946). This early assessment of the relative role of empirical and theoretical research revives differently in modern

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[1] McCallum (1999, fn. 6, p. 44) claims that this article of Kydland and Prescott was clearly previewed by another one in 1980.

[2] See Kydland and Prescott (1990) and Cooley and Prescott (1995).

[3] Such as Kydland and Prescott (1990).

discussions of business cycles measurement. For instance, whereas the RBC approach takes the apology of the 'theory ahead measurement' argument already present in Koopmans, others such as Harding and Pagan (1998, 1999), contemplating an alternative way of measuring business cycles, prefer to emphasise the argument of 'measurement with statistical theory'.

In this essay we will begin by describing, in section 2, the RBC representation of the business cycle and, related to it, we will try, in section 3, to throw some light upon the way the arguments and debates of the 1940s are resuscitated in modern discussions of business cycles. Section 4 concludes.

2. REPRESENTING THE BUSINESS CYCLE IN THE RBC LITERATURE

The prevailing conception of the business cycle in the RBC literature follows Lucas own conception, which, as he states in his 1977 paper, identifies the business cycle with 'movements about trend in gross national income'. These movements are typically irregular in period and in amplitude; regularities are only observed 'in the co-movements among different aggregative time series' (Lucas, 1977, p. 87). Summarising these regularities, Lucas lists seven features of economic time series where coherence and amplitude are informally used as criterion.

The attempt to make this conception explicit and operational has been one of the preoccupations of a research program that favours 'growth cycles' instead of 'classical cycles' and measures of co-movements between detrended series in detriment of the study of turning points. In fact, despite the RBC's claim 'that *growth* and *fluctuations* are not distinct phenomena to be studied with separate data and different analytical tools'[4], the RBC empirical verification program has been to assess the conformity of a set of second moments of the detrended data with those of the artificial data generated by RBC models. However, as argues Canova (1998, p. 477), dynamic economic theory does not indicate the type of economic trend that series may display nor the exact relationship between secular and cyclical components. Accordingly, the empirical examination of the business cycle became itself subject of controversy within the RBC literature, due to the polemic issue of detrending. Moreover, recent investigation in business cycle measurement provided namely by Harding and Pagan (1998, 1999), judges useless to

perform a detrending operation preferring the classical cycle's tradition of studying fluctuations in the level of activity.

Let us appreciate the idiosyncrasies of the RBC business cycle measurement and the related controversies more carefully.

Growth cycles

Typically, the procedure of detrending developed by Hodrick and Prescott (henceforth, HP) in their 1981 paper is, until now, a standard method employed by RBC researchers. It presupposes an additive decomposition of the economic time series into secular, cyclical and seasonal components where 'the growth component varies "smoothly" over time' (Hodrick and Prescott, 1981, p. 4). The degree of smoothness (the so-called parameter λ) of the growth component is selected resorting to some ad hoc economic criteria. These, rooted on neoclassical growth theory with a variable rate of technological change, preclude the extreme smoothness of a deterministic trend (a linear trend) and lead to select $\lambda = 1600$, for quarterly data, which leaves in the data cycles of average duration of 4-6 years[5].

Since the representation of the data inherent to this decomposition reflects the preferences of the researcher and the economic question being investigated, Fabio Canova (1998) classifies it as an economic-based decomposition by contrast to a statistical-based one. However, among the defenders of a greater intervention of the economic theory on the business cycle representation there are some, such as Singleton (1988) and Harvey and Jaeger (1993), that address sharp criticisms to the HP pre-filtering procedure stressing the risk of reporting spurious cyclical behaviour. They defend instead the study of the secular and cyclical frequencies of economic series simultaneously, since it is questionable that they are generated by distinct and separable economic phenomena.

There is another filtering[6] method widely used by RBC researchers - the band-pass filter developed by Baxter and King (1995). Classified as a statistical procedure by Canova

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[4] Cooley and Prescott (1995, p. 4)

[5] See Canova (1998, p. 485). Prescott, (1986), Cooley and Prescott (1995) and also King and Rebelo (1999) emphasise that the HP filter, with $\lambda = 1600$, leaves fluctuations in economic time series that have periodicity of eight years or less.

[6] Burnside (1998) emphasises differences between detrending and filtering. Filtering involves extracting the business cycle frequency where detrending - removing the trend - may magnify other frequencies.

(1998), it draws on the theory of the spectral analysis of time series data and, ideally, it should eliminate both the high frequency (periods less than six quarters) and the low frequency fluctuations (periods exceeding eight years) associated to the trend growth. However, this ideal filter can not be implemented in finite data series and only a feasible band-pass filter can be designed. Researchers such as Burnside (1998), Christiano and Fitzgerald (1998) or Stock and Watson (1999) reveal a clear preference for the use of this filter. According to Stock and Watson (1999, p. 12), this filter mitigates one of the problems of the HP filter - the permanence of the high frequency noise.

Other detrending methods, some rooted on statistical criteria, others on economic criteria, are used, at times, to perform a comparison between the growth cycles obtained by different procedures. Very often the objective is to examine the robustness of the so-called stylised facts of the business cycle to the detrending methodology. Edward Prescott wrote in his 1986 paper that 'if the business cycle facts were sensitive to the detrending procedure employed, there would be a problem' (Prescott, 1986, p. 10). But, this is exactly what Canova has found and written out in his 1998 paper, wich had a former version in 1991[7]. He sustains that 'the idea that there is a single set of facts which is more or less robust to the exact definition of business cycle is misleading since different concepts of business cycle generate different economic objects which need not to have similar characteristics' and he proceeds by recommending the employment of a variety of selected detrending methods pursuing a more interactive relationship between theory and practice (Canova, 1998, p. 508).

 Sometimes a comparison between the cycles obtained by trend-removal procedures - the growth cycles - and the cyclical turning points dated according to the chronology of the National Bureau of Economic Research (NBER) is provided. The NBER reference dates constitute a broadly accepted business cycle chronology and the origins of their underlying methodology remount to Burns and Mitchell's (1946) seminal study *Measuring business cycles*. In practice these dates are determined in a two-step process where: first, cyclical peaks and troughs are determined for individual cycles (defining specific cycles); second, common turning points are determined by comparing these series-specific turning points. Then, as Stock and Watson stress (1999, p. 8), 'if, in the

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[7] This paper was subjected, for a long time, to sanguine comments, as Canova wrote in his 1998 reply to Burnsides (1998) criticisms, published in the same JME's number.

judgement of the analysts, the cyclical movements associated with these common turning points are sufficiently persistent and widespread across sectors, then an aggregate business cycle is identified and its peaks and troughs are dated'. Although these cycle measurements are obtained by a mixture of mechanical applied rules and ad hoc judgements, as Mary Morgan (1990, p. 52) had pointed in reference to Burns and Mitchell's 1946 work, it is possible to develop algorithms to mimic NBER dating procedures[8].

Albeit the use of the NBER taxonomy with the aim of strengthening the employment of a particular detrending method or on the ground of deciding which method is more consistent with it, the divergences between their implicit representations of the business cycle surpass largely the question of the lacking or the presence of a statistical foundation in those methodologies. While RBC researchers concentrate upon a set of second moments of random variables previously detrended, which are taken to represent economic activity, the NBER approach examines the turning points occurring in the level of variables that measure economic activity, the so-called classical cycles. Even when the research is motivated by the comparison of the turning points obtained with the application of both methodologies, which accuses a (little) schizophrenic attitude in Harding and Pagan's (1998, p. 2) opinion, it is impossible to avoid existing divergences between them. Classical cycles are necessary in a minor number and have more lengthy expansions than growth cycles. Harding and Pagan (1998, p. 3-4) relate that in the USA post-World War II 'there were 8 post-war cycles of average duration of 62 months, with expansions absorbing 52 of these months' according to the NBER definition, whilst, with the HP filter, cycles were of 30 months duration and exhibited expansions and contractions with equal length[9, 10].

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[8] This task was provided namely by Bry and Boschan (1971) and by Christina Romer (1994). Harding and Pagan (1998, 1999) try to develop a statistical foundation to Burns and Mitchell methodology.

[9] Also Christina Romer (1994) shows that the current belief that expansions have become longer and contractions shorter follows from the existence of inconsistency in the NBER chronology of business cycle troughs and peaks. She points that business cycle reference dates before 1927 appear to be derived from detrended data, whereas the dates after 1927 are based on data that include the secular trend. After having removed such inconsistencies, she concludes that 'although the time separating contractions has become longer between the pre-World War I and postwar eras, recessions themselves have not on average become shorter, less severe, or less persistent over time' (Romer, 1994, p. 606).

[10] We must remark that there is an apparent contradiction with the way these authors interpret the NBER methodology and the interpretation provided by Fabio Canova. In his 1999 paper, this researcher explicitly refers that NBER uses, in standard practice, growth cycles extracted by elaborate and ad hoc procedures that are hard to reproduce. In fact, Harding and Pagan (1998) recognise that the NBER researchers have also studied growth cycles obtained by a phase-averaging method. But, according to them, the growth cycle has rarely been of much interest in

Summing up, avowedly the process of detrending for the purpose of representing the cyclical components of economic time series is a controversial issue, even within the RBC literature, since there is no consensus on the properties of the trend and on its relationship with the cyclical component of a series. Even when economic models, such as those described by Canova (1998, 1999), are used to guide the decomposition, the argument remains. In fact, RBC researchers manifest a revealed preference to the use of two filters - the HP and the band pass-filter - that generate cycles with a range of periodicities similar to those found in NBER dating. However, as Canova (1998, p. 500) has pointed out, 'even within the class of methods which extract cycles with duration close to the conventional 4-6 year periodicity, several qualitative differences emerge'. His findings make him defend the use of various detrending filters to avoid misdirect theoretical research. Meanwhile, others, such as Harding and Pagan (1998, 1999), defend the return to the older tradition of studying the classical cycle where they saw a natural interpretation of the origin of the cycle in terms of trend and the second moments of growth rates.

Although Harding and Pagan talk of the older tradition of studying the classical cycles, referring to Burns and Mitchell tradition, it is possible to remount the growth cycles study to another older tradition rooted in the 1919 work of Warren Persons, another empirical economist. One of his main contributions was the development of methods to decompose economic time series into components and expose the business cycle[11]. However, according to Morgan (1990, p. 58), 'the basic conceptual framework, from which Persons proceed, was that fluctuations in economic series occur as a result of various forces: secular (or long-term) forces, seasonal forces, cyclical (or wavelike) forces and accidental (or irregular) forces'. These forces occurring simultaneously leave, to the researcher, the task of carrying out the elimination of the unwanted fluctuations and reveal the business cycle. With this aim, he attacked first the problem of measuring the secular trend resorting to a direct trend-fitting method. The cyclical fluctuations are the remaining component after the subtraction of the seasonal and trend elements[12], as it is customary to do through RBC empirical researching. Nevertheless, Persons' work is a

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most western economies. With this methodology there were 12 growth cycles of 46-month length with equal size of contractions and expansions (Harding and Pagan, 1998, p. 3-4).

[11] Mary Morgan (1990, p. 57) argues that the methods Persons develops were not entirely new, but had been used piecemeal in economic statistics from the late nineteen century.

[12] See Morgan (1990) for more details on Persons' methodology.

widely forgotten reference[13] while Burns and Mitchell's[14] work has been an obligatory one.

Co-movements and stylised facts

The belief that regularities on cyclical fluctuations are only to be found in the way variables move together - their co-movements - is certainly one of the reasons for the RBC revealed preference for growth cycles. The necessity of calculating measures of these co-movements by a set of second moments of random variables motivates the detrending procedure as a way of obtaining stationarity. The more frequently reported statistics are the percentage standard deviation of the series and the cross correlation of the series with real GDP/GNP, including the lead and lag correlations with output. Thus, the cyclical component of the output is used as the reference variable, being 'a useful proxy for the overall business cycle', in the words of Stock and Watson (1999, p. 15). Evidently, within the RBC literature, this choice has a theoretical fundament since, according to Kydland and Prescott (1990, p. 10), they 'let the neoclassical growth model dictate which facts to examine and how to organize them'. So, the choice of the output as the reference variable is not surprising. The other (real) facts that the RBC's core theory suggests to examine are the production inputs, the output components and the factor incomes. Sometimes nominal variables are also considered since their cyclical behaviours have been at the heart of many controversies over the nature of business cycles.

In short, the standard way the RBC research program summarises data is dictated by the assumption that theory precedes measurement - the *theory ahead of business cycle measurement*[15] methodology - and it is provided as follows:

- Selected economic time series - such as, the real output and its components, the production inputs and their income - are made stationary by a standard trend removal procedure.

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[13] Singleton (1988) constitutes an exception.

[14] Cooley and Prescott (1995, p. 27) consider that the Burns and Mitchell's methodology implies the study of the business cycle on the deviations from the growth path. This could be interpreted as a growth cycle approach. However, as Harding and Pagan (1999) sustain, the dated turning points is done on the original series and not on the detrending series, albeit the use of a stationary series as an input into the dating process. So, they argue that Burns and Mitchell's cycles are classical cycles and not growth cycles.

[15] This is the title of Prescott's 1986 paper.

- Volatility, persistence and co-movement are assessed resorting to a set of second moments of the detrended data, such as: the absolute and the relative[16] standard deviation; the first-order autocorrelation; the cross correlation with the output at different lags. Relevance is specially conferred to the relative volatility and to the type of co-movement exhibited with output, which can be classified as pro, counter, or a-cyclical and leading, lagging or coincident.

- After examining these summary statistics the so-called stylised facts of the business cycle are then set up.

It is now recognised that these business cycle facts are sensitive to the detrending method. Moreover, the report of an 'incorrect' business fact may misdirect theoretical research, which can be the case of the Lucas' 1977 documented pro-cyclical price level behaviour. In fact, Kydland and Prescott (1990, p. 17) emphasise the importance of this myth in the development of business cycle theories in which the price level plays a central role and behaves pro-cyclically. However, the same authors, depreciate the criticism addressed to the use of the HP filter remarking that 'the resulting deviations from trend are nothing more than well-defined statistics', which 'measure nothing' and they proceed by saying that 'talking about the resulting statistics as imposing spurious cycles makes no sense' (Kydland and Prescott, 1996, p. 76-77, fn 6)[17].

The business cycle RBC's representation is at least puzzling. It presupposes detrending with methods, such as the HP filter, that impose zero-correlation between the trend and the cyclical components of the series without any support of the dynamic theory of real economic fluctuations[18]. It stresses co-movements, where the business cycle regularities are supposed to be, but statistic testing of their stability across cycles[19] and across detrending methods[20] is hardly provided. It resuscitates Burns and Mitchell's work but it subverts its methodology. It restores the 'measurement without theory' debate taking the 'theory ahead measurement' argument so far that one could claim it almost empties the measurement concept.

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[16] This is the standard deviation of the cyclical component of a variable, in percentage of the standard deviation of the output gap. It measures its relative volatility.

[17] James Hartley (1999, p. 4) ironically classifies these statements as belonging to the 'new world of Pseudo-measurement Without Theory'.

[18] See Canova (1998).

[19] Harding and Pagan (1998) refer some exceptions.

[20] Canova (1998) is an exception.

3. THE 1040S DEBATES IN MODERN DISCUSSIONS OF BUSINESS CYCLES

The 'Measurement without Theory' debate initiated by Koopmans in 1947, in reference to Burns and Mitchell's 1946 book on *Measuring business cycles,* was seen to be responsible for the taboo of the business cycle facts' reporting by Kyndland and Prescott (1990). Since the 1980 and 1982 pioneer papers of these two authors, the transcription of the so-called stylised facts of the business cycle became trivial in a flourishing literature of which empirical verification program has been to assess the conformity of data business cycle's measures with those generated by RBC models. To surpass the stigma of being at the 'Kepler stage' - the empiricist mark that Koopmans had appended to Burns and Mitchell's work of systematic description, classification and measurement of business cycles - it was necessary to restore the debate and to redeem, in some way, Burns and Mitchell's empirical work. With this goal, Kydland and Prescott invoke the crucial importance of the empirical work of Kuznets and the empirical regularities set up by Kaldor as the 'stylised facts' of growth, to the development of the neoclassical growth theory and, simultaneously, they seem to take Koopmans' point of the indispensability of theoretical preconceptions.

Recently, some of those that present an alternative way of summarising business cycles manifest a renewed interest in Burns and Mitchell's work and Koopmans' criticism. This is the case, for instance, of the Harding and Pagan 1998 and 1999 papers. At the same time that they defend the advantage of a return to the Burns and Mitchell older tradition of studying the cycle, by opposition to the modern one carried out by the RBC researchers, they try to avoid one of the Koopmans' criticism by developing a statistical foundation for their work.

As we can realise, some of the arguments and controversies of the 1940s are recovered in modern discussions of business cycles. Vestiges of these debates can also be found in the literature[21] on the topic of calibration versus estimation, but we will focus our attention on those developed under the mantle of the Burns and Mitchell's work and Koopmans' review.

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[21] Represented namely by the articles of Hoover (1995), Kydland and Prescott (1996), Hansen and Heckman (1996) and Sims (1996). Canova (1994) roots the actual confront calibration vs estimation on the two opposite methodologies presented in the 1933 work of Frisch and in the 1944 work of Haavelmo. Hoover (1995) makes an interesting and careful analysis on this topic concluding that the calibration method is a kind of indirect estimation.

The mantle of Burns and Mitchell

As we have already said, the RBC literature roots its representation of the business cycles in Lucas' own conception. This one, in turn, is seen to follow Burns and Mitchell's work when it emphasises that the business cycle regularities are to be found in the co-movements of variables[22]. On the other hand, Lucas' definition is seen to differ from that of Mitchell's since the latter 'represents business cycles as a sequence of expansions and contractions, particularly emphasizing turning points and phases of the cycle' (Kydland and Prescott, 1990, p. 4). There seems to be a contradiction in these statements. However, following Harding and Pagan's (1999, p. 5) interpretation of this subject, the use Burns and Mitchell made of the inter-relationships (or co-movements) between the specific series - the construction of the reference information through the way turning points in many series clustered together -, was simply an instrument when in much of the modern literature, it has become an end unto itself. Furthermore, the output growth cycle became the reference cycle relatively to which the growth cycles of selected variables' co-movements are measured. Implicit in the Harding and Pagan's interpretation is the point that for Burns and Mitchell co-movements are important to assess the reference cycle, an attempt to get the common factor out, while for some modern researchers they are the substance of the business cycle.

Even when one sustains, in disagreement with those two authors, that for Mitchell business cycles were the co-movements - 'these congeries of cyclical movements that move together'[23] -, it is debatable that these were seen by Burns and Mitchell as the source of regularities - the 'defining features of the business cycle' (Cooley and Prescott, 1995, p. 26) as the RBC literature intends it to be. Epstein (1999) contends that this stylised view is not supported by Burns and Mitchell's statistical evidence since this reveals a substantial divergence in specific cycle dynamics. Moreover, he claims that the tendency to emphasise the uniformity of episodes follows Koopmans and Marschak's interpretation, which ignores behavioural irregularities.

Referring to Burns' 1951 position on Mitchell, Epstein (1999) states that even if the reference cycle had been derived from a single series, such as output or national income, the essence of the business cycle was seen to be in the systematic divergence of the many

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[22] See Cooley and Prescott (1995, p.26).

[23] Mary Morgan, flour discussant in the conference on "Theory and evidence in macroeconomics".

activities, rather than in their relation to an index of business conditions from which the reference cycle would be proxy. No single reference measure can capture this complexity. The value of the specific cycle information is as highly important as that contained in the reference cycle. In Burns later terminology, two sets of cycles coexist - a 'seen' cycle, in aggregate activity and an 'unseen' cycle, in its components. These unseen cycles in the many activities did not prove to be invariant sequences of specific-cycle timing relations.

In short, most of the modern discussions on business cycles recover the work of Burns and Mitchell as reference for their arguments. Whereas Lucas and the RBC literature stress the importance of co-movements' regularities between the output and other specific variables, other modern researchers emphasise the importance of their analysis in terms of classical cycles and turning points. In the meanwhile, most of the re-readings made on Burns and Mitchell's work spring partly from Koopmans' 1947 review, as Epstein (1999) had argued. Despite the recurrent reference to Burns and Mitchell's work, it seems that the RBC literature adopts many of the points of view Koopmans had expressed in his 1947 review.

Resuscitating the measurement without theory debate

The publication of the 1946 Burns and Mitchell's book gave rise to a methodological debate between Tjalling Koopmans - a senior researcher of the Cowles Commission - and Rutledge Vining - a visiting research associate at the National Bureau of Economic Research (NBER). This debate that became known as the 'Measurement without Theory'[24] debate was apparently won by Koopmans since, in Hendry and Morgan's words, 'the dominant theme of econometrics for the next few decades reverted to studying empirical models through the eyes of pre-specified theoretical models' (Hendry and Morgan, 1995, p. 71). However, this victory is a controversial issue since many of the arguments used in the contention between the NBER and Cowles Commission's methodologies are actually resuscitated in modern business cycle's discussions.

In fact, Kydland and Prescott (1990) retake two of the Koopmans' arguments against Burns and Mitchell's methodology - the theory's precedence argument (Koopmans' first argument) and the one related with the role assigned to the statistical theory. They

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[24] The title of Koopmans' 1947 paper that initiated the debate.

vehemently disagree with the hypothesis, embodied in Koopmans' third argument, of the aggregate time series being generated by a probabilistic model (represented by a structural system of equations) which must be estimated and tested. Whereas Vining (1949), in his reply to Koopmans, vindicated the 'hypothesis-seeking' stage of Burns and Mitchell's work against Koopmans' 'hypothesis-testing' stage in the context of discovery in economics - he argued that Koopmans has vaulted over the problem of the construction of tentative theoretical models which presumably still occupied Burns and Mitchell[25] -, modern discussions made the debate evolve to one centred in the topic of calibration and estimation's methodologies[26]. The problem to Vining seems to be Koopmans' jump over 'the searching for regularities and interrelations of regularities and the feeling around for interesting theoretical models'[27] rather than the statistical estimation. The formal theoretical model Koopmans had in mind - 'based upon postulated and fixed individual motives and transformation functions' - wasn't yet proved to be adequate to justify the jump to the hypothesis-testing stage[28].

The present point of contention in this subject is slightly different. According to Canova (1994, p. s123-s124), 'the estimation approach asks the question "Given that the model is true, how false is it?" while the calibration approach asks, "Given that the model is false, how true is it?" What calibrators defend is to adjust the (unrealistic) models they built to the data, without question the theory while estimators accept that alternative theories compete among them for the support of the data. In Hoover's words[29], 'For the calibrators ... data help discriminate only between different adaptations of the common core' theory. Thus, despite Kydland and Prescott's rejection of the estimation approach - which they call the system-of-equations approach - their reliance in their 'well-tested theory'[30] brings them closer to Koopmans' points of view[31] rather than Vining's.

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[25] See Vining (1949, p. 82-83).

[26] The calibration versus estimation debate is only an incidental remark in our exposition. A careful examination of this point would lead us far beyond our aims and it would have obliged us to take seriously the works of Haavelmo and Frisch likewise.

[27] Vining, 1949, p. 83.

[28] According to Qin (1993, p. 178), 'the Koopmans-Vining debate brought up the issue of how to construct models with tentative and indefinite hypothetical theories' although, neither of them 'was able to suggest constructive, systematic methods for the uncertainty in model construction by the probability approach'.

[29] Hoover (1995, p. 30).

[30] The neoclassical growth theory.

[31] This seems consistent with the opinion of Cooley and Dwyer (1998, p. 60-61) when they argued (referring to the study of business cycle fluctuations) that the dynamic general equilibrium (DGE) approach adopted by the RBC literature 'is closer to that envisioned by the Cowles Comission economists' than the structural VAR approach that assigns a very small role to the economic theory.

In fact, RBC researchers, such as Kydland and Prescott, subscribe Koopmans' first argument that 'even for the purpose of systematic and large scale observation of such a many-sided phenomenon, theoretical preconceptions about its nature cannot be dispensed' (Koopmans, 1947, p. 163). Pursuing this argument Koopmans states that the lack of guidance from theoretical considerations, in Burns and Mitchell's work, is perceivable in the selection of the variables to analyse - which, being the possible might not be the relevant variables - and in the choice of the measures - which, are intended to be measures of the aggregate, of the business cycle's behaviour, instead of the behaviour characteristics of its ultimate determinants, the behaviour of groups of economic agents. He also refers the necessity of the theoretical analysis to integrate the study of fluctuations and trends, around which variables fluctuate (Koopmans, 1947, p. 164). In Morgan's words (1990, p. 56), 'Koopmans' group had advanced a Walrasian model based on the aggregation of individual economic agents as units and in which the cycle was seen as a deviation from an underlying equilibrium level'.

All these arguments, as we have seen before, are adopted by the standard RBC literature. Its ambassadors defend 'theory ahead measurement' where this theory is paradigmatically represented by a 'neoclassical optimal-growth model with stochastic shocks to technology which cause the equilibrium growth path to fluctuate about its steady state' (Hoover, 1995, p. 26). Kydland and Prescott (1996, p. 76, fn 6) take the theory's precedence argument so far that, as they state, their representations of the business cycle fluctuations - 'the resulting deviations from trend' - 'are nothing more than well-defined statistics' which 'given the way the theory has developed [...] measure nothing'.

Thus, the debate about the precedence of theory over evidence seems to represent more the debate about the type of economic theory to use rather than whether any should be used[32]. In fact, according to Morgan (1990, p. 45)', it would be a mistake to think that Mitchell rejected any role for business cycle theories, for he recognised [...] that theories determined which "facts" should be examined'. Having doubts about which theory is valid, Mitchell defends, in his 1913 book, the study of the facts to elucidate this argument. Indeed, neither Koopmans nor RBC researchers accept the idea of the choice of the relevant theory being driven by the data evidence. The maximum the RBC

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[32] This is also the opinion of Mary Morgan and David Hendry expressed in the introduction of their 1995 book.

methodology accepts is to adjust their models[33] in order that they can fit the data - at least in selected dimensions - without rejecting the core theory. But, the data they try to fit - the so-called business cycle facts - proceed already from a summarising data procedure where theory takes part. Moreover, this one, as we have already seen in section 2, lacks relevant hints to guide this summarisation stage in a less controversial way.

Another point of the Koopmans' first argument, which received attention in its epoch and is yet subject of controversy, is the one that opposes the aggregate to the individuum as unit of analysis. Vining, in his reply to Koopmans' criticism, stresses the limits of Koopmans' 'individual economizing agent' as an alternative unit 'in the study of many aspects of aggregate trade fluctuations' (Vining, 1949, p. 79). He claims that 'the aggregate has an existence apart from its constituent particles and behavior characteristics of its own not deducible from the behavior characteristics of the particles' (Vining, 1949, p. 79). In the same sense, fifty years later Hartley defends that 'theory is showing us that the representative agent, equilibrium real business cycle model is unable to explain aggregate dynamics, that we need some sort of aggregate model' (Hartley, 1999, p. 11).

4. CONCLUDING REMARKS

RBC researchers claim the merit of the empirical work of collecting and reporting business cycle regularities to the development of the business cycle theory. They realise that, for a long time, the realisation of this task was avoided due to Koopmans' criticism on Burns and Mitchell's work. In the methodological debate between the Cowles Commission group and the NBER's there was an argument to which the RBC literature was particularly sensible - the theory's precedence argument. In fact, the issue was the precedence of a particular type of theory. However, dynamic economic theory lacks relevant hints to guide RBC's summarisation business cycles data. Thus, the controversy settled down among the RBC researchers. The detrending procedure is a polemic issue that may jeopardise the so desired business cycle facts. These regularities should be found in the co-movements between selected detrend variables and the output gap.

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[33] Furthermore, some authors, such as Pagan and Hartley, argue that these models don't generate business cycles and use this as an argument against that research program.

Theory dictates this latter to be the reference cycle, a 'one-dimensional basic pattern of cyclical fluctuation' more in accordance with Koopmans' interpretation than with Burns and Mitchell's conception.

Recent investigation in business cycles measurement, provided namely for Harding and Pagan, emphasises the benefits of Burns and Mitchell's methodology criticising vehemently the one embodied in the RBC's program. The debates keep going on.

Some, as Hartley (1999, p. 12), proclaim real business cycle dead and the long live to the business cycle's study while others, such as King and Rebelo (1999, p. 995), claim that 'the RBC literature has been a positive technologic shock to macroeconomics'.

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