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THE RISE AND FALL AND RISE...
OF THE BUSINESS CYCLE

The Shann Memorial Lecture 1996
Adrian Pagan
DISCUSSION PAPER NO. 349
September 1996

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Centre For Economic Policy Research

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The Shann Memorial Lecture 1996

Adrian Pagan

Economics Program
Research School of Social Sciences
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THE HANN MEMORIAL LECTURE, 1996

THE RISE AND FALL AND RISE...OF THE BUSINESS CYCLE

Adrian Pagan
Australian National University

I. Preamble

In 1929 the report of the U.S. President's Conference on Unemployment contained the memorable words, "Our situation is fortunate...we have a boundless field before us". As Shumpeter (1946, p.5) wryly commented "They only forgot that the road into this boundless field leads through a succession of valleys". Myopia over the valleys is not just confined to the twenties, as evidenced in the Wall Street Journal's comment in April of this year,2

"In the 1980s, the notion that the economy invariably goes through ups and downs, culminating in a recession that leads to recovery, fell out of favor. The length of the 1980s recovery led many businesses and economists to believe that good times could go on uninterrupted."

Not only does economic activity go through mountains and valleys, but so does intellectual investigation into the business cycle, to such an extent that the two are almost isomorphic. A century ago very little was written about the business cycle, except for noting its existence and some tentative suggestions about its cause; mostly the analysis centred upon the workings of the gold standard, and the prevailing philosophy seems to have been that associated with Jevons, quoted in The Australian Economist, March 1893, (reprinted on p.301 of Butlin et al. (1986)):

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1 This paper was partly written while visiting the Economics Department at Johns Hopkins University and I would like to thank them for their support. I have also benefited from conversations about the material with Lou Maccini, Mark Watson and David Grubb. Much of section 4 is based on work with Mardi Dungey and she has been a great help in putting it together.

2 Article entitled "Funding conflicts force an economist out of university".
“Money must find its own level like water and flow in and out of a country according to the fluctuations of commerce which no government can forsee or prevent”.

Perhaps there exists no better illustration of this sense of futility than the fact that, in the 10 years of proceedings of the Australian Economics Association from 1888-1898, only one paper was ever delivered on the subject, despite the fact that the members of the group were living through what was arguably the worst period of depression in Australian history - the 1890's. That paper was delivered by a wool broker, W.H. Chard, who, for his efforts, was roundly chastized in the discussion of his paper by constant references to Say’s Law that supply creates its own demand, followed by the coup de grace that wants were unlimited. One of the audience, a Captain Hynes, produced a rebuttal that captures the flavour of the occasion, as well as the attitudes that seemed prevalent at this time, when he disposed of the then depression in the following way, (Butlin et. al (1986).p. 540)

“Mr. Chard must come nearer home for the causes of the depression since 1890, the real source of which lies in the rotten system of professional politicians, in our democratic governments maintaining power by the wasteful expenditure of millions of borrowed money, in buying the political votes of the roads and bridges members, the shamefully overloaded Civil Service vote, and the revolutionary socialistic labour vote”.

It is scarce wonder that Chard made the rather plaintive reply,

“The discussion of my paper has not been logical or satisfactory... The real subject of my paper not being dealt with in any clear or systematic manner, it is useless for me to make any lengthy reply...”.(op. cit. p.540)

Of course all of this changed in the first three decades of this century. Names such as Pigou, Cassell, Aftalion, Spiethoff, Schumpeter, Hawtry and Hayek all came to be associated with particular views of the causes of the business cycle and surveys of views on the subject produced enormous volumes e.g. Robertson (1915) and Schumpeter(1939). The hills of the cycle literature rapidly became mountains during the thirties and after the war, with Keynes, Harrod and Hicks as

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2 As Robertson (1915, p.1) observed “The causes of crises and depressions alleged before the various committees of Congress in the eighties amounted to some 180 in number, and included the issue of free railway passes and the withholding of the franchise from women”.

2
the outstanding contributors. Much of this literature reflected a fear that activity was doomed to remain in a deep valley forever and that “something had to be done” to propel us up the mountainside. Either it was done, or the view was myopic, but, after the war, the valleys of activity seemed rather shallow, and, accordingly, interest in the business cycle waned. Mathews’ (1959) book *The Trade Cycle* represents a high point of the concern; after then it is noticeable that textbooks on macroeconomics had little to say about the issue, preferring instead to concentrate upon the building blocks of the macro system and to find the “microeconomic foundations”. By default, investigation into the workings of the complete economic system was left up to the macroeconometric model builders, but even they did not spend much time on the topic or address it in any precise way.

It was only with the return of deeper economic valleys in the 70’s and 80’s that the business cycle literature rose again. Beginning with Lucas’ (1975) piece, an agenda was promulgated that claimed an understanding of the business cycle as the consequence of optimal responses by economic agents, a development that later became identified as the “real business cycle” literature, which, in turn, was transmuted into that of “stochastic general equilibrium” models. A glance at any journal will show how successful this new agenda has been in academia. Not only academics were affected though. The heightened interest in these matters again started to pervade the financial press, to such an extent that the *Wall Street Journal*’s summary of the article mentioned at the beginning of this section was that, “.....business cycle research is back in vogue”.

Thus my title.

Edward Shann would probably not be surprised by all of this. Despite the fact that his research interests were largely in what determined the size of the economic

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*The arrogance displayed by some of the proponents of this view is no better illustrated than in Kydland and Prescott’s (1996, p.76) comment that “Prior to the development of these methods, business cycle fluctuations were viewed as deviations from theory, and very little progress was made in understanding them”.*
mountains, he contributed to the business cycle literature in a number of ways, most remarkably in his 1927 work (Shann(1930a)). Nevertheless, perhaps because of Shann's fundamental interests, the words "business cycle" have rarely appeared in the Shann Lectures. Graham Snooks' lecture was an exception, but, owing to the sweep of his vision, the mountains and valleys that I look at would be scarcely visible there. Generally, the lecturers have shown more interest in the mountains than in the valleys, particularly what they were made out of and how high they might be. Sometimes, such as in Bob Gregory's lecture last year, an attempt is made to verify how many people are in the valleys, or to try to determine whether they were at the bottom of the valley or half way up the mountain, or maybe whether they had shifted valleys over time. But the undulations in the landscape were taken as given. In some ways this is odd. The business cycle has been alive and well in Australia since the very first one in 1826-8.5

Tonight I wish to do a few things. First, I want to briefly describe the mountains and valleys. Second, I want to look at the literature on what makes a business cycle. Without such a perspective we will always fall into the same trap as identified by the The Economist,

"Few economic issues grab more misguided attention, from experts and laymen alike than the business cycle. Their obsession is almost always founded on misconceptions about the powers of economists and politicians."

Finally, I want to briefly study some aspects of a particular sequence of cycles, those of the 1980's and early 1990's. Of course, such an agenda is overly ambitious, and inevitably will result in an over-simplification of many issues. Moreover, I will have to be selective and to confine myself to certain issues that seem to have become particularly important in academic and public debate.

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5 Although Butlin(1994,p.224) notes "the commercial crisis of 1810-13".
6 Article entitled "No escaping volatility in a less-than-perfect world"
2. The History

What is a business cycle? The classic definition of this was provided by Burns and Mitchell (1946, p3)

"Business cycles are a type of fluctuation found in the aggregate economic activity of nations that organize their work mainly in business enterprises: a cycle consists of expansions occurring at about the same time in many economic activities, followed by similarly general recessions, contractions, and revivals which merge into the expansion phase of the next cycle; this sequence of changes is recurrent but not periodic...."

It is useful to formally think about this statement in the following way. Suppose that sectoral output is the sum of a common factor, $F_t$, and a component that has a zero mean. Then when we form a weighted average of the sectoral outputs, as is done with GDP, the aggregated sectoral components will tend to average to zero and GDP will be the common factor that remains. A study of GDP behaviour is therefore a study of this common factor, and it must be the properties of such a factor that are crucial to the existence and nature of a business cycle. Fig 1 therefore shows the log of Australian GDP from 1830 until 1995. Although one can see some examples of what has been termed the “classical cycle” i.e the cycle in the original level of activity, principally in the 1840s, 1890s and the 1930s, many of the hills and the valleys tend to be obscured by the strong trending behaviour of the series. Moreover, the initial rapid growth of the colonies suggests that it will be hard to find an adequate model of trend that is constant, leading to the decision to focus upon the period after 1880. Accordingly, fig 2 presents the detrended log of GDP formed by regressing out a time trend from the observations and plotting the resulting residuals. Then the hills and valleys are much clearer. One can go further and apply more sophisticated trend removal filters such as phase averaging, used by Boehm and Moore (1984) for example, or the Hodrick- Prescott filter. Using the latter gives the other series in fig2. Such trend removal operations aim to isolate what has been named the “growth cycle”.

Unlike the U.S. there seems to be no widely recognised dating of cycles within Australian economic history. An early attempt is in Thorpe and Mitchell (1926) and more recent versions are Boehm (1993) and Boehm and Liew (1994). Table 1 gives a rough chronology based on a variety of sources: Hartwell (1956) for the 1820's, Butlin (1953) for the 1840's, Thorpe and Mitchell (1926), Boehm (1993), Boehm and Liew (1994), Oyster and Meredith (1990), and my own assessment for the final peak. In constructing this table I have followed the rule of eliminating any cycle that is less than 9 months in duration from peak to trough.
<table>
<thead>
<tr>
<th>Peaks</th>
<th>Troughs</th>
<th>Peaks</th>
<th>Troughs</th>
</tr>
</thead>
<tbody>
<tr>
<td>1826</td>
<td>1830</td>
<td>1951m4</td>
<td>1952m11</td>
</tr>
<tr>
<td>1840</td>
<td>1843</td>
<td>1955m12</td>
<td>1958m1</td>
</tr>
<tr>
<td>Late 1889</td>
<td>3rd quarter 1895</td>
<td>1960m9</td>
<td>1961m9</td>
</tr>
<tr>
<td>Late 1900</td>
<td>mid 1903</td>
<td>1965m4</td>
<td>1968m1</td>
</tr>
<tr>
<td>1907Q4</td>
<td>1908Q4</td>
<td>1970m5</td>
<td>1972m3</td>
</tr>
<tr>
<td>1914Q1</td>
<td>end 1915</td>
<td>1974m2</td>
<td>1975m1</td>
</tr>
<tr>
<td>1920Q4</td>
<td>1921Q4</td>
<td>1976m8</td>
<td>1977m10</td>
</tr>
<tr>
<td>1924Q4</td>
<td>1925Q3</td>
<td>1981m6</td>
<td>1983m5</td>
</tr>
<tr>
<td>1929Q2</td>
<td>late 1931, early 1932</td>
<td>1985m11</td>
<td>1987m3</td>
</tr>
<tr>
<td>1938q1</td>
<td>1939q2</td>
<td>1991m7</td>
<td>1992m12</td>
</tr>
<tr>
<td>1951m4</td>
<td>1952m9</td>
<td>1990m4</td>
<td>1995m9</td>
</tr>
<tr>
<td>1955m12</td>
<td>1957m12</td>
<td>1995m9</td>
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</tr>
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<td>1960m9</td>
<td>1961m9</td>
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<td>1974m7</td>
<td>1975m10</td>
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<td>1976m8</td>
<td>1977m10</td>
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<tr>
<td>1981m6</td>
<td>1983m5</td>
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<tr>
<td>1985m11</td>
<td>1987m3</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1990m4</td>
<td>1991m7</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1995m9</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>


One should treat this table simply as indicative for the purpose of this lecture as I am sure that a more precise investigation would reveal further cycles, particularly in the 1860-1880 period. It is a pity that we don’t have a good chronology like the U.S. Even after 1950 the dating has largely been done by one individual - E.A. Boehner unlike the US where a committee of experts determines the authorized dates. For the purpose of Australian macroeconomic research there would seem advantages in trying to set up some similar institution here.

Given that the most reliable dating and analysis of the cycle are likely to be done with post-WW2 data, the business cycle characteristics we will work from
are best found from that era. Fig 3 plots the data for the period 1950-1995 from Fig 2 i.e. deviations of GDP from the trend line established over 1880-1995.

One can discern the peaks and troughs recorded in Table 1. However, such information is hard to use and this leads us to seek some quantitative measures that summarize the nature of the cycle. The simplest of these involves the average time spent between the various combinations of peaks and troughs. Table 2 contains such summary measures of the cycle over the period 1950-1995, replication of which will be the core of many of the arguments advanced in what follows. The table is sourced from Boehm and Liew (1994). We will maintain that a series which exhibits these features can be regarded as generating a business cycle. Perhaps the items of greatest interest in the table are that the classical cycle is longer than the growth cycle (around 6.5 rather than 4.5 years) and it is asymmetric, with expansions being some three times longer than contractions. To “make” a business cycle we will need to be able to produce such outcomes.
Table 2 Number and Average Durations of Cycles (in months)

<table>
<thead>
<tr>
<th></th>
<th>Growth Cycle</th>
<th>Classical Cycle</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number</td>
<td>10</td>
<td>7</td>
</tr>
<tr>
<td>Peak to trough</td>
<td>23</td>
<td>17</td>
</tr>
<tr>
<td>Trough to peak</td>
<td>30</td>
<td>61</td>
</tr>
<tr>
<td>Peak to peak</td>
<td>51</td>
<td>78</td>
</tr>
<tr>
<td>Trough to trough</td>
<td>53</td>
<td>78</td>
</tr>
</tbody>
</table>

3. The Theory

To talk about theories we must have a framework. As Frisch (1933) pointed out, an important distinction in any business cycle analysis is that between the impulse driving the cycle and the mechanism which propagates the impulses; a taxonomy that he attributed to Wicksell. Such a distinction is an invaluable one; without it much of the discussion of business cycles is amorphous, and the resulting arguments are irritatingly vague and unfocussed. To be sure there are theories for which such a distinction seems to be inappropriate; a fine example being the “manic depressive” theory of business cycles revealed in the following remark about the 1820’s recession by John Dunmore Lang (Lang (1852)) (quoted in Goodwin (1966, p.220))

“in short the body politic of the colony has passed through a crisis of violent and unnatural excitement, which, according to the well-known maxim of Hippocrates, the father of medicine must necessarily be followed by a corresponding unnatural depression”.

Goodwin (1966), in his excellent account of this early thought, wrote that

“by 1867 the manic-depressive theory had obtained wide currency, and that as a result the inevitability of depressions had become universally accepted”.

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While acknowledging these exceptions, it nevertheless proves to be invaluable to bear the above dichotomy in mind, especially when reading the older literature, which regularly confuses the two.

So let us first turn to impulses. From the demand side the structure of the national accounts directs us to impulses categorized as being related to consumption, investment and the balance of trade. Those stemming from investment have been most popular, labelled under various guises such as “animal spirits” or “crises”. Literature old and new abounds in references to the role of animal spirits and confidence. Apart from Keynes himself, one can find many examples from Australian history, perhaps none better than the ever-colorful J.D. Lang’s description of the 1820’s cycle, ((quoted in Goodwin1966, p.229))

“...no sooner had the existence of the Agricultural Company been duly announced, and its operation been commenced in right earnest, than the sheep and cattle mania- a species of madness undescribed by Cullen, and formerly unknown even in the colony- instantly seized on all ranks and classes of its inhabitants”.

In more modern times we have the voice of The Economist

“Recessions are often triggered by unpredictable external factors, such as an oil-price shock or sudden changes in consumer or business confidence”.

As the quote from The Economist above foretells, international factors have also had a good run as a cause of the cycle, particularly in Australia. As the 1880’s wore on, the colonists became impressed with the role of international factors, and R.M. Johnston, the Tasmanian statistician

“...believed that apart from misallocation other causes of Australian depressions lay beyond the country’s control, and in particular inexplicable worldwide disturbances of product and capital markets” (Goodwin(1966, p. 225)).

Shann took this line in relation to the 1890’s recession, focussing on the fluctuation in British foreign lending, while many others pointed to the strong terms of trade decline that preceded and accompanied it. Today the most visible

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7 This looks like mad cow disease 170 years before the British one - another Australian first - although if that were true one might want to think of it as a consumption shock.
direct descendent of this line of argument would have to be Synect, whose predictions on the back page of the Australian each day regularly proclaim this fact with headlines such as ‘National Accounts a ticket on foreign-driven roller-coaster’, (The Australian, Nov 30, 1995).

Monetary impulses have been cited as an important determinant of the cycle almost from the beginning of research into the area, and a proper account would take up the whole lecture. Views of the importance of such impulses have waxed and waned. For example, Mathews (1959, p.59) gives his summary as

“It was at one time thought that the causes of fluctuations lay wholly or largely in the sphere of money and finance. The trend of opinion has now swung in the opposite direction. Most modern theoretical treatments of the cycle are based on an analysis of real forces, and it is implicitly assumed that secondary importance, at most, attaches to any effects that may be brought about by changes in the cost and availability of finance”;

although he does concede that it

“...must have at least a permissive significance in the cycle: even if fluctuations originate from real forces...”.

Within four years of publication of this view Friedman and Schwartz (1963) swung the pendulum of opinion back the other way, where it remained until the assault of real business cycle theorists in the 1980’s. Where it rests now depends a lot on the observer. There almost certainly are still adherents to the school of thought that money doesn’t matter, just as there are those who believe that (Ball and Mankiw (1994, p. 129))

“The Romers’ results suggest not just that money is non-neutral, but that monetary contractions are a major source of U.S. business cycles”.

Turning to the supply side there have also been a large range of suggestions which might be subsumed under the heading of shocks to total factor productivity (TFP). Schumpeter’s identification of waves of technical change was an early example. The Swedish school, particularly Cassell, also had this orientation, but

* "No escaping volatility in a less-than-perfect world".
supplemented it with an emphasis on productivity shifts coming from the
importance of new markets in an expanding international economy. Cassell may
well have been the first to emphasise the role of what is now referred to as Tobin’s q ratio, when he pointed to the importance of a discrepancy between the value of an investment project and its replacement cost as a driving force in the cycle. In the past decade the real business cycle literature has made TFP shocks the centrepiece of their theories. Within Australian economic thought one also finds echoes of this theme e.g. S.J. Butlin’s work on the 1840s (Butlin (1953, p.317)) and N.G. Butlin’s attribution of part of the 1890’s downturn to productivity declines (Dyster and Meredith (1990, p.46-47)).

All of the above makes it clear that the range of shocks proposed as the basis of a business cycle is quite wide. What about propagation mechanisms? To put some structure on this discussion we need to establish what type of propagation mechanism will generate a business cycle, in the sense of being able to replicate the features summarized in Table 2. Consider the following simple form for the factor, \( F(t) \), driving the cycle,

\[
F(t) = a + b F(t-1) + \epsilon(t),
\]

(1)

where \( \epsilon(t) \) is an independently distributed random variable with zero mean and standard deviation given by \( \sigma \), while \( F(t) \) is measured monthly. Thus \( \epsilon(t) \) is the basic shock and it gets propagated by an autoregressive process. Our strategy will be to first select values of \( (a, b, \sigma) \), then to generate data on the computer with that process, and finally to see if a “business cycle” emerges. As just mentioned, to judge the latter we will refer to the characteristics detailed in Table 2. To measure those we apply a standard computer program used in business cycle dating, Bry and Boschan (1971), to the simulated data. The program automates NBER approaches to the dating of cycles through a sequence of steps. These steps involve repeated smoothing of the data, with each smoothed series being used to

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* I am grateful to Mark Watson for providing me with a GAUSS version of this program that I have modified for simulation work.
provide an adjustment to the cycle dates established at a prior stage. In the dating part of the algorithm the application of a series of rules distinguishes between "real" and "spurious" peaks and troughs e.g. a movement from peak to trough cannot be shorter than six months. Fig 4 shows the original series and one of the smoothed series, with the latter being formed by a Spencer 13 point moving average of the original data (scaled so as to make the two series distinct). The simulated data used in this figure is an autoregressive process, specifically that identified as (iv) later, and it is clear that the smoothing process is very important in the identification of cycles. Boehm and Moore use the same program for business cycle dating with actual data but they also intervene if the automatically chosen dates seem deficient in some way. Clearly, as we will be generating 200 samples of artificial data in each experiment, it would be infeasible for us to make such adjustments, even if we knew exactly how Boehm and Moore used their expert knowledge. It would appear that this knowledge results in the elimination of cycles whose amplitude is small so that the cycle lengths automatically chosen by the program will understate those in Table 2.

\[10\] The oft-quoted rule that a recession exists if there are two quarters of negative GDP growth would therefore pass this test, thereby defining a trough in the classical cycle. 

\[11\] It should be emphasised that we use a single series to represent the common factor whereas Boehm and Moore seem to combine at least six indicators (see Chart B on p. 41 of their paper). I have some reservations about this practice. In particular, the use of the unemployment rate and total employment to date recessions can be misleading if there are large real wage changes. In that event, activity may be only slightly affected but the demand for this factor may change a great deal. This seems to be what happened in 1974/5, where GDP growth was affected in a small way but the unemployment rate changed dramatically.
For ease of reference we will give names to four particular configurations of the above process.

(i) $I(1)$ with drift; $a=.028$, $b=1$, $\sigma=.0096$.

(ii) $I(1)$; $a=0$, $b=1$, $\sigma=.0096$.

(iii) Stationary $AR(1)$; $a=0$, $b=.8$, $\sigma=.0096$.

(iv) White Noise; $a=0$, $b=0$, $\sigma=.0096$.

The identifiers are taken from the modern time series literature. When $b=1$ the process is said to be integrated of order one, or $I(1)$, as $F(t)$ involves the sum (integral) of the $e(t)$ and possesses a stochastic trend in that its variance increases with time. If $a=0$, $F(t)$ can be written as the sum of a deterministic and a stochastic trend, with the growth rate of the deterministic part being “a”. Process (iii) is a stationary first order autoregression. The difference between (i),(ii) and (iii) is that any impulse into the former processes, once it occurs, remains in $F(t)$ forever i.e. the impulse persists, while in the latter it dies away. Nevertheless, it does last for some time, with the effective length being determined by the magnitude of $b$. When $b=0$, as occurs in the fourth process, the shock’s impact is restricted to the period in which it occurs.

Table 3 reproduces the results in Table 2 (in the columns marked “data”), along with the corresponding quantities for each of the four processes. The latter are
constructed by simulating artificial data from each of these processes over a 45 year period and then passing them through the Bry-Boschan program. 200 such series are generated for each process and the statistics in Table 3 are averages across these 200 series.

**Table 3 Business Cycle Characteristics for Four Processes**

<table>
<thead>
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<th>Process</th>
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<th>data(c) (ii)</th>
<th>data(g) (iii)</th>
<th>(iv)</th>
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<td>7</td>
<td>12</td>
<td>10</td>
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<td>Peak to trough</td>
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<td>17</td>
<td>23</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>17</td>
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<tr>
<td>Trough to peak</td>
<td>63</td>
<td>61</td>
<td>24</td>
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<tr>
<td>Peak to peak</td>
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First, look at the results for I(1) plus drift i.e. process (i). It is clear that these match the duration and asymmetric characteristic of the classical cycle very well, pointing to the fact that a simple propagation mechanism like (i) does extraordinarily well in replicating the business cycle facts. We can find out what it is that accounts for this success by slowly stripping away the structure. Removing drift i.e. a deterministic trend, as in process (ii), causes cycles to become symmetric with upturns and downturns of roughly the same length. Hence any theory explaining the classical cycle needs some deterministic growth in order to account for the asymmetric length of contractions and expansions. Moreover, the degree of asymmetry will increase with the value of "a", so that periods of very fast growth, as in 1860-1880, are likely to witness cycles that are much longer than in those periods with slower growth (provided of course that the persistence of shocks in (i) is the best description of the data). Similarly, the "Tigers" would be
unlikely to experience much of a classical cycle as their “a” is currently rather large. 12

We can formalize some of this discussion a little by using the rule that a classical cycle trough occurs after two consecutive quarters of negative growth, rather than the more complex rules of the NBER type dating algorithms. Suppose we think of (1) as applying to quarterly data with $F(t)$ being the log of output. Then we would have

$$
\phi = \text{prob(through)} \\
= \text{prob}(\Delta F(t) < 0, \Delta F(t-1) < 0) \\
= \text{prob}(\Delta F(t) < 0) \cdot \text{prob}(\Delta F(t-1) < 0),
$$
due to the fact that $\Delta F(t)$ is independent of $\Delta F(t-1)$. Moreover, since $\Delta F(t)$ is a stationary process, $\text{prob}(\Delta F(t) < 0) = \text{prob}(\Delta F(t-1) < 0)$ and

$$
\text{prob}(\Delta F(t) < 0) = \text{prob}(\Delta F(t) / \sigma < 0) \\
= \text{prob}(\{(a/\sigma) + \varepsilon(t) : \varepsilon(t) < 0\}) \\
= \text{prob}(\varepsilon(t) < (a/\sigma)),
$$
demonstrating that the probability of a trough, and hence the expected length of a cycle, depends upon the ratio of the trend growth rate to the magnitude of the impulses. Formally, the expected duration of a classical cycle (in months) will be $3/\phi$. Using estimates of “a” and $\sigma$ from quarterly Australian data over the period 1959Q1-1996Q1, and assuming that $\varepsilon(t)$ is $N(0,1)$, we find that the expected duration of a classical cycle in Australia would be 80 months, very close to the 78 months reported in Table 2.

Having found what type of process will produce the classical cycle we turn to the question of what is needed to reproduce the growth cycle. This is a much harder question to answer, as the growth cycle works with “detrended data”, and the method of detrending will be extremely influential in determining the resulting

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12 In the discussion above it should be remembered that these results are from a simulation so that there is some variation in the average durations due to chance.
cycle characteristics. For example, if one just regresses the \( F(t) \) from process (i) against time and uses the residuals from the regression as detrended data, the resulting process would be process (ii) and so the growth cycle is symmetric and of about four years duration. If, instead, one applied the popular Hodrick-Prescott (HP) filter to the same series, the detrended data would be an AR(1) with coefficient of around .8 i.e. the series would resemble that in (iii) and the growth cycle would be found to recur every three years.\(^{13}\) Judging from the fact that Boehm and Liow have a growth cycle of around 4 years it appears that their phase averaging method of trend removal leaves an AR process that is close to being I(1). What is striking is that there is little change to the cycle characteristics as \( b \) moves from .8 to zero; it is the transition from \( b=.8 \) to \( b=1.0 \) that produces a remarkable change.

So the implication of this analysis is that a remarkably simple propogation mechanism can yield business cycles of the type we see. Is this type of propogation mechanism the one in the data? Following our earlier argument that GDP should be close to the factor in (1), Table 4 presents estimates of \( b \) when \( F(t) \) is set to \( \Delta \log(GDP)(t) \), as well as the first four serial correlation coefficients of \( \Delta \log(GDP)(t) \) over the period 1959Q1-1996Q1 for Australia and 1947Q1-1995Q1 for the US. Clearly any serial correlation in growth rates is small and there is very strong evidence in favour of an I(1) process (with drift, because the average growth rate in GDP is non-zero).\(^{14}\) To emphasise that the serial correlation seen in US GDP growth rates is unimportant for cycle analysis I generated data in the same way as done in constructing Table 3, but with a factor whose growth rate is assumed to be the same as an AR(2) estimated with the U.S. data. If there had been no serial correlation in the growth rate the number of cycles would have been

\[^{13}\text{This result was found by generating 100 simulated data sets of 147 observations on process (i), filtering with the HP filter, and then finding the estimate of } b \text{ by regression. The average estimate of } b \text{ was } .78. \text{ Note that this result is invariant to the value of } a \text{ as the HP filter eliminates a deterministic trend.}\]

\[^{14}\text{The values for } a, b \text{ and } \sigma \text{ used in Table 3 were in fact set so that a monthly process would produce quarterly estimates of growth and its standard deviation which might be regarded as representative of the data. Note that the } a \text{ and } b \text{ values in Table 4 are for quarterly data. For monthly data a would be smaller and } b \text{ would be higher.}\]
12 with an average duration of 46.84 months, while, with the observed degree of serial correlation in the U.S. data, we would have still found twelve cycles with an average duration of 47.28 months. Thus the presence of the type of serial correlation revealed in Table 4 is of no consequence for the nature of the cycle.\footnote{\textsuperscript{15} Indeed, provided \( z(t) \) does not have a unit root one can allow it to be correlated without changing the characteristics relating to processes (i) and (ii) in Table 3 i.e. the fact that we choose \( z(t) \) to be}

\begin{table}[h]
\centering
\begin{tabular}{lcccc}
 & \( a \) & \( b \) & \( r(1) \) & \( r(2) \) & \( r(3) \) & \( r(4) \) \\
\hline
Australia & .0094 & .98 & .03 & .03 & .15 & -.15 \\
U.S. & .0077 & .98 & .37 & .22 & .04 & -.04 \\
\end{tabular}
\caption{Time Series Properties of GDP for Australia and the U.S.}
\end{table}

The standard error of the \( t \) is around .08. For the U.S. \( b \) is estimated as \( .96 \) if the post 1974 period alone was used.

One argument that might be advanced against our conclusion is that GDP may not be the right measure of output. In the US much work with RBC models has involved the construction of "stylized facts" based on "private output" rather than GDP, and such series typically remove government expenditure and, sometimes, other components from GDP e.g. King and Watson (1996) take out ownership of dwellings, the government and farm sectors. After doing so they seem to produce a cycle in output growth, in the sense that there is a peak in the spectral density of output growth rates "at the business cycle frequency". That would not be true of the processes in (i)-(iv). To investigate the effect of such a change in definition I have constructed "private output" as they detail in their footnote 1. I do find an estimated spectrum that resembles theirs, in that it has a peak away from the origin. However, this peak does not arise from complex roots in (say) an AR(2) but rather from the presence of some very high order serial correlation in growth rates around lag 12 and onwards. Closer inspection of where the peak in the spectrum is located shows that it corresponds to a cycle between 76 and 112 months, which is far too long to qualify as a growth cycle (remember that growth rates are being analysed). As a final exercise I passed their "private output" growth rate series through the Bry-Boschan program, finding that the average period
between troughs (and peaks) was 36 months, which is what would be expected if there was weak serial correlation in growth rates. I have also done a similar exercise for Australia, with the same outcome.

The foregoing presentation about the nature of random processes needed to produce a cycle is an old theme or, more accurately, is "old wine in new bottles". Its antecedents are Fisher (1925), and Slutsky (1937) (originally published in 1927 in Russia). Fisher (1925, p.200) for example says

"If by the business cycle is meant merely the statistical fact that business does fluctuate above and below its average trend, there is no denying the existence of a cycle and not only in business but in any statistical series whatsoever...In the same way weather conditions necessarily fluctuate about their own means, so does the luck at Monte Carlo. Must we then speak of 'the weather cycle' and 'the Monte Carlo cycle'? I see no more reason to believe in 'the' business cycle."

I think it is the case that economists have been rather sceptical of this argument with Mathews (1959, p. 200) speaking for many with the following reservations:

"If it were true that ups and downs in business activity reflected nothing more than the operation of random disturbances, as Fisher here in effect contends, there would clearly be no need for any special theory of economic fluctuations. But in its extreme form the contention is not tenable...In a truly random series each value shown will be independent of all others and is equally likely to be the same side or the opposite side of the trend line as the one immediately before it, in a series like national income a high value will normally be followed by another high value..."

Mathews' intuition about something being wrong with Fisher's argument is borne out if we concentrate upon model (iv), which represents the model implicit in Mathews' comments - there would be too many cycles. However, once we

uncorrelated is of no significance.

16 This outcome shows that the connection between peaks in the spectrum and what we normally conceive of as a cycle is very tenuous. It also shows the danger in evaluating the ability of a model to produce a business cycle simply by comparing the model and data spectra as King and Watson do. My own belief, articulated in Canova et al (1992) and Pagan (1994), is that a complete comparison of the VAR's implied by model and data is needed.

17 Morgan (1990) gives an excellent account of the impact that Slutsky's experiments had upon business cycle researchers. It is interesting to note that the process which Slutsky simulated to reach his conclusion was one that summed 10 independently and identically distributed random variables and it differs from an I(1) process in that the latter sums as many as there have been to that point in time. I have repeated Slutsky's experiment and analysed the output with the Broy-Boschan program. The resulting cycle characteristics are those in columns five and six of Table 3 i.e. on average they produce three yearly rather than four yearly cycles.
switch to processes (i) and (ii), which allow for persistence in shocks, the picture completely changes. In any case, the two perspectives are not incompatible.
Equation (1), thought of as an equation for output, would be the “final equation” of some system of relationships, and that system could be regarded as a proposed economic model. The challenge is for economists to devise systems that will produce a propagation mechanism whose final equation is as simple as that which we observe in reality.  

So what have theorists had to say about the propagation mechanism? The literature on this is enormous. Mostly, the _modus operandi_ was to imagine an impulse and then describe plausible mechanisms whereby this impulse was magnified. Indeed, one might say that the literature became obsessed with the propagation mechanism, probably because this was the “economic contribution”. Armed with the concepts of classical mechanics they sought to find what forces would restore equilibrium to a system once it was shocked. The development of differential and difference equations, along with the physics of items like pendulums, led to lots of analogies and formal treatments. However, whether the cycle thereby created resembled what was to be seen in the data was rarely rigorously addressed (of course Tinbergen’s classic work (1939), which was one of the seminal contributions to modern econometrics, asked exactly that question and proceeded to answer it in much the same way as I am doing tonight).

In discussing this literature it is useful to again refer back to the GDP identity and the facts we have established about how GDP needs to behave to produce cycles. In particular, it needs to be close to I(1), and that means either consumption, investment or the balance of trade must have that property (even then this is necessary but not sufficient). Making exports I(1) is perhaps the simplest thing to do, as it is likely that world GDP is I(1)- certainly US GDP seems to be - and that might be regarded as an estimate of the common factor

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18 In saying this we are merely reiterating what Tinbergen (1939) said almost 50 years ago.
underlying world output. That would result in the US and Australian cycles resembling one another, which we know to be the case - see Boehm and Liew (1994) Table 3. Notice that what I have argued here is that US GDP represents a measure of the common trend to world output. It doesn’t mean that US GDP causes Australian GDP or that the cycles are caused by the US. It is not even necessary that we trade a great deal with the US for the result to hold, just that it is the best measure of the common trend in world GDP. Indeed we know that the deterministic trend in Australian GDP exceeds that of US GDP, a feature that could be a consequence of our trading with other faster growing economies. But the stochastic trends are common and it is the stochastic trend that is the key to the business cycle (of course there are other characteristics of a cycle that we have not measured such as amplitude, governed by the size of σ, as well as possible asymmetries, all of which may make the cycles in each country different, if not the turning points).

Much literature on the propagation mechanism has however been concerned with closed economies and so it was natural that it was conceived of as stemming from the responses of consumers and investors to the shocks. There were many attempts at explicating such adjustments. The king of them all was the multiplier-accelerator mechanism; its influence probably peaked in Mathews’ (1959) book but many famous models, such as Hicks’, were based on it. Its most common incarnation, well summarized by Mathews, showcases investment, with that variable being determined by the lagged change in GDP while consumption responds directly to income. The theory implies that GDP growth would be positively related to the lagged rate of change of GDP growth i.e. the underlying model for the factor has ΔF(t) depending on ΔF(t-1) and ΔF(t-2), with the strong restriction of equal coefficients but of opposite sign. Once growth slows, this acts as a negative impulse to reduce growth in the next period. Certainly, this produces

\[ \text{\textsuperscript{13} There is slippage in my argument here as the results in Table 4 are for the log of GDP while the GDP identity is in levels. My purpose is to suggest some connections rather than being perfectly rigorous.} \]
a cycle, but one at variance with the factor structure in (1), as that would claim there is little (or no) influence of $\Delta F(t-1)$ and $\Delta F(t-2)$ upon $\Delta F(t)$. Indeed, it is at variance with the data as well. Computing the correlation between GDP growth and its rate of change gives -.23 for quarterly data, which is the wrong sign. Perhaps our interpretation is too strict and the multiplier-accelerator theory should be applied to yearly growth rates; making such a modification, a regression of the growth rate of GDP on its lagged rate of change over the period 1900-1995 gives a coefficient of .008, while for the period 1951-1995 it is -.001, neither of which is significantly different from zero. Moreover, the analytics of the multiplier-accelerator model show that this coefficient should be approximately equal to the product of the capital output ratio, the multiplier, and the growth rate, and this would tend to imply a coefficient of around unity for yearly data. All of this points to the fact that this propagation mechanism is not a satisfactory one, and theories built on it will not be compatible with the data.\(^20\)

The regular accelerator comes from a production relation. Instead, one might view the interrelationship between financial and real decisions as an important propagation mechanism. Interest in this has been heightened by the slow recovery from the last recession in many countries, and this has been treated as a consequence of the accumulation of debt and the collapse of asset prices in the 1980’s e.g. see Schinasi (1994) for an analysis of a range of countries and Blandell-Wignall and Bullock (1992) for Australia. Asset price movements are taken to affect expenditure decisions, not only through well known q type effects, but also because the value of collateral for loans has been sharply reduced. The resulting “financial accelerator” Bernanke et al (1996)- can both magnify and prolong impulses, although it is difficult to see it as the primary propagation mechanism. Until recently, the idea has been more suggestive than rigorous, but work by Kiyotaki and Moore (1995) sets forth a model that generates "credit

\(^{20}\) One needs to qualify this statement with the observation that models such as Hicks’ add floors and ceilings to the basic multiplier-accelerator mechanism and this induces some non-linearity into the relation. Nevertheless, cross plots of the two variables do not suggest that such “topping and tailing” can rescue the relationship.
cycles). It appears that their model effectively makes output a stochastic process with complex roots which, as we have observed, would be at variance with the data.

A more convincing approach is to turn attention away from investment to consumption. Such a move runs counter to almost all the history of business cycle analysis, but remember we are concerned here with propagation and not impulses. Consumption turns out to be a very promising candidate, as Hall (1978) observed that the life cycle approach to consumption means that it should be l(t). Hence, one might argue that, at least from the demand side, the business cycle comes from the smoothing of consumption by households, rather than from investment, the traditional focus of attention. Such a simple observation can immediately account for why early econometric models could replicate business cycle facts—see Adelman and Adelman (1959) on the Klein-Goldberger model. In many of these models, particularly quarterly ones, there was a great deal of persistence in consumption relations i.e. in the regression of consumption on its own lagged value and other determinants the coefficient on the former was always close to unity.

As with a pair of scissors, any economic problem has two sides to be watched, and to date we have ignored the supply side. In doing this we have largely been following the history of econometric modelling. But, following the supply side shocks that hit the OECD economies, the latter underwent a dramatic change in the past two decades, and it is therefore not surprising that these events have had their impact upon business cycle theorizing. To evaluate the developments in the context of our previous remarks, consider a Cobb-Douglas production function linking output (Y(t)), capital (K(t)), labour (L(t)) and total factor productivity, A(t), \[ Y(t) = A(t)K(t)^α L(t)^β. \] Taking logs leaves the relation

21 Some of the arguments in this literature are tenuous. For example, it is frequently claimed that demand declined as "households and businesses repaired their balance sheets". It appears, however, that the repairs imply a transfer, generally within the same sector, and any reduced expenditure would therefore require the assumption of differing propensities to spend by different units within these sectors.
\[ y(t) = a(t) + \alpha k(t) + \beta l(t), \]

where \( y(t) = \log Y(t), \ k(t) = \log K(t), \ l(t) = \log L(t) \) and \( a(t) = \log A(t) \). The real business cycle (RBC) school proposed that \( a(t) \) follow the I(1) with drift process in (i) above. It is clear that, provided \( z(t) \) is either constant, weakly serially correlated, or a function solely of \( a(t) \), the output process just inherits the I(1) structure assumed for the TFP process. Consequently, given what we know about the type of process for output which produces a business cycle, it is immediately apparent that a RBC model will generate a cycle simply by what is assumed about the evolution of TFP. All that the economics does is to ensure that \( z(t) \) has properties that do not inhibit such an outcome.  

If one fixes the TFP process to be deterministic or just an uncorrelated random variable one needs to ask how output could be made persistent. There have been some clever answers to this. One is to allow for increasing returns to scale in production \( \alpha + \beta > 1 \), wherein the solutions for \( k(t) \) and \( l(t) \) may become functions of unobservable I(1) random variables due to some indeterminacy in the solution to the underlying rational expectations problem. Farmer and Guo (1992) term these "animal spirits" and show that there is persistence in \( z(t) \) even when there is none in \( a(t) \). Other solutions might be to add public capital to the production function as an exogenous I(1) process.  

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22 Early versions of RBC models had b around .9 but this has slowly crept up to unity in recent work. In the simplest version of these models the optimal levels of capital and labour are functions of the TFP shock, meaning that the latter becomes the single factor driving output.

23 It is a curious feature of this literature that the business cycle has been re-defined as something related to the volatility in output and the covariances of various expenditure quantities with output. The former really concerns the amplitude of the cycle and not its existence; white noise is volatile but does not produce a business cycle. Emphasis on expenditure correlations is typically justified by reference to Lucas' rendition of the Burns and Mitchell definition cited earlier, but the point of the co-variation identified by the latter was that only the common elements of sectoral movements should be regarded as the business cycle. There may be many reasons for expansion and contraction of particular sectors, but these should not be counted as part of the business cycle unless the effects are widespread. It may be of interest to measure how the relative position of sectors changes over the cycle but that seems a secondary issue. In this regard, arguments have been made that the amplitude of cycles has decreased due to the decreasing importance of manufacturing, see "The Ups and Downs of Services", The Economist, July 6, 1996.

24 Warwick McKibbin has pointed this out to me. Such an effect would be consistent with N.G. Butlin's interpretation of Australian economic development.
Having now moved to the supply side and isolated the important role for TFP shocks in producing a cycle, one might be led to query whether such well known macro models of the Australian economy as MM2 (Murphy) and TRYM would, if simulated, be capable of replicating a cycle, particularly given that they have TFP following a deterministic rather than a stochastic trend. To answer this one needs to recognise that these models have an aggregate supply relation that depends upon the real wage, and that relation replaces the production function. Thus the question can be re-formulated as whether they model real wages as an I(1) process. The answer is in the affirmative, due to their use of a Phillips curve, which makes the change in the real wage a function of demand and supply side shocks. Consequently, the business cycle dynamics in these popular models fundamentally stem from the Phillips curve, perhaps the least well determined relationship in them.

4. The Econometrics

It emerges from the preceding section that there are a plethora of shocks and propagation mechanisms, all of which are capable of inducing a business cycle. Trying to determine the relative importance of each of these has occupied econometricians’ attention from the earliest days of the discipline -see Tinbergen(1939). What emerged from all the debate was the need to take a multivariate and simultaneous perspective.25 The challenge has been to devise systems that are both manageable and yet represent the data with as little distortion as possible. Ultimately, there is no unique way of doing this, and we are always in the process of experimenting with variations on themes that have been fruitful in the past.

In the last two decades the methodology that has dominated most macroeconomic research, at least in the U.S, has been that of vector autoregressions (VAR’s). These tended to replace, at least in academia, larger

25 One might add that this comment applies equally to other public debates, a good example being the recent controversy over the impact of fiscal policy on demand.
simultaneous equation models. In fact, their novelty is rather exaggerated. In the earlier tradition, recursive models, in which one assumed that it was possible to determine an order of causation e.g. output influences inflation in the current period but not conversely, and in which the shocks to the system were uncorrelated, were always regarded as a possible way of proceeding, and were sometimes used. With yearly data though, recursivity did not seem plausible, and it was only with the advent of data sets based on quarterly and monthly observations that the time was ripe for a resurrection of the idea. Sims (1980) did that, adding on the assumption that every structural equation contained the lags of every variable in the system. The latter addition was a radical departure from traditional macroconometric models, which generally only had a few lagged variables in each equation. Other methodologies, such as the LSE approach, maintain that one should test whether the lag coefficients really do contribute anything to the explanation of the phenomena, and it seems more a matter of taste which strategy one prefers. Certainly, the most important problem, that of simultaneous determination of variables, was assumed away by Sims through the recursive assumption. After setting out his generalized system Sims re-named the older “multiplier analysis”, “impulse response analysis”, and described some tools for determining how much of the variation in a variable was due to each of the impulses.

The VAR technology is ideal for what we wish to do in the remainder of this lecture, as it is necessary to allow for many impulses and to be flexible regarding the propagation mechanism. For these reasons we need to allow the model to have enough variables to represent many of the impulses described earlier - world demand, terms of trade, financial and confidence factors, demand shocks, supply side and monetary shocks - and to keep the dynamics as flexible as possible. The model I use consists of 10 variables, listed in Table 5, with potentially two lags of each variable in every equation. However, it differs from a regular VAR in placing

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26 In many instances a distinction is made between VAR's and structural VAR's. I think such a distinction is very unproductive. Except for prediction, all uses of VAR's presume a structural
some restrictions upon the interactions between variables and the dynamics e.g. the US q ratio affects the Australian q ratio but not conversely. Further documentation on its construction and features is given in Dungey and Pagan (1996). Some mention also needs to be made about the correspondence of impulses and the variables in the VAR. Items such as US GDP and the terms of trade are self evident. GNE is taken to capture domestic demand shocks, while supply shocks are traditionally sourced from the Phillips curve relating inflation to output movements. Financial and confidence shocks are measured in a number of ways. The US real interest rate is a direct one but the two q ratios also capture them, as they essentially fluctuate with equity prices. Domestic confidence and financial accelerator effects might also impact upon the domestic q ratio, so that this variable captures quite a range of effects. Monetary impulses are measured through an equation for the cash rate which has both exogenous and feedback components.\footnote{An exogenous shock is defined as that which is not predictable from the past history of the variables making up the VAR.}

<table>
<thead>
<tr>
<th>Impulse Type</th>
<th>Variable</th>
</tr>
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<tbody>
<tr>
<td>International</td>
<td>Real U.S. GDP</td>
</tr>
<tr>
<td>International</td>
<td>Real 3 month U.S. T bill Rate</td>
</tr>
<tr>
<td>International</td>
<td>Terms of Trade</td>
</tr>
<tr>
<td>International/Confidence</td>
<td>Ratio of Dow Jones Index to US CPI</td>
</tr>
<tr>
<td>Confidence/Financial</td>
<td>Ratio of All Ordinaries to the Implicit Deflator of Plant and Equipment</td>
</tr>
<tr>
<td>Demand</td>
<td>Real Gross National Expenditure</td>
</tr>
<tr>
<td>Supply(?)</td>
<td>Real GDP</td>
</tr>
<tr>
<td>Supply</td>
<td>Underlying inflation rate</td>
</tr>
<tr>
<td>Mon. Policy</td>
<td>Money market rate (Authorised dealers)</td>
</tr>
<tr>
<td>Confidence/Financial</td>
<td>Real TWI.</td>
</tr>
</tbody>
</table>

model, and a failure to think in these terms often results in remarkably silly applied work.
Using this model, what can we say about the variations in Australian GDP over the period 1980-1995? Two time frames are adopted. The first concentrates upon yearly changes in GDP; the second the change over four years, roughly corresponding to the period of the growth cycle. Table 6 gives a decomposition of the variance of GDP according to the contribution of a number of the important impulses identified earlier.

Table 6

<table>
<thead>
<tr>
<th></th>
<th>O/S Dom</th>
<th>O/S Fin</th>
<th>Tot</th>
<th>Dom Fin</th>
<th>Dom Dem</th>
<th>Dom Supply</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 yr</td>
<td>28</td>
<td>43</td>
<td>2</td>
<td>6</td>
<td>11</td>
<td>.4</td>
</tr>
<tr>
<td>4 yrs</td>
<td>39</td>
<td>39</td>
<td>3</td>
<td>3</td>
<td>7</td>
<td>3</td>
</tr>
</tbody>
</table>

Dennis Robertson began his monumental survey of business cycle theories with the comment,

"...this does appear to be a case in which, in the deathless words of the Dodo, everyone has won and all must have prizes".

He clearly wasn’t looking at Table 6. There is no doubt about the winners here; external influences. There is little that is novel in this conclusion. It has a long history, and a recent influential work, Gruen and Shuetrim (1994), documented the strong dependence of Australian GDP upon overseas GDP. An important qualification that they placed on the finding was that the influence was probably through financial markets as well as through trade, and that US GDP was just a proxy for the generalized effects. That argument was subsequently supported by de Roos and Russell (1996), who found that overseas financial factors were indeed

\(^{28}\)The sample period was chosen to correspond with the period of time in which we have had exchange rates strongly influenced by market forces.
important in a single equation model for GDP, and the above results echo that conclusion in a systems context.

Turning to the domestic shocks there is a very small contribution from supply and confidence effects. Regarding the latter, I have experimented with a direct measure of confidence, the balance statistic formed from the ACCLI-Westpac survey question asking whether conditions are expected to improve in the next six months, and I found that it produces results that are virtually identical to those above. Such an outcome might be expected, given that the balance statistic is well explained by (lagged) inflation, output growth and the real cash rate. It is only at the peaks of booms (bottom of troughs) that confidence is under (over) predicted by the equation.

Performing a decomposition on the average variation in output over a year or the cycle may not capture the importance of particular impulses during specific cycles. To address this broader issue we need to dissect movements in GDP into the relative contributions of each of the impulses. This is done in Fig 5.\textsuperscript{30} One notable item pertains to the length of the last recession. Our model attributes this to negative external influences, with little contribution from the influence of domestic debt levels. In particular, the role of credit in this recession does not seem very marked. It is also apparent that monetary impulses are more important at certain times, and the fact that they contribute a small fraction to the total variation- less than 2%- is deceptive. For this reason it is worth considering the role of monetary policy in the business cycle over the late 80s and early 90s.

\textsuperscript{30} I also argued this in Pagan (1987), comparing the Australian growth rate from 1954 to 1984 with a trade weighted average of the growth rates of our trading partners (see figure 5.1 in that article).

\textsuperscript{30} A complete analysis of this figure is beyond the lecture, but there are some interesting points to be made about it. First, a standard interpretation of the 1986 growth cycle as due to a terms of trade effect needs to be qualified; the downturn is not the consequence of such effects although they certainly make a contribution at the bottom of the recession and in the recovery. Secondly, the influence of US equity markets may be regarded as surprisingly strong after the 1987 crash. To explain this we note that the fall in the market at that time only returned the US q ratio back to its trend value, while the sizes of the post-87 shocks were close to zero. So these contributed nothing to GDP variations. However, the earlier rise in the stock market had caused a rise in the local q ratio which, due to lags in expenditures, resulted in an investment boom in 1988.
Figures 6 and 7 contain the information needed for such an assessment. The solid line represents the deviation of the log of output (expenditure) from a deterministic trend, the dashed line shows what output (expenditure) would have been if monetary policy had been switched off, and the dotted line shows what the output (expenditure) effects of monetary policy were. To switch off monetary policy we have had to eliminate both the impact of exogenous movements in the cash rate as well as changes in it that are induced by any “feedback behaviour”. Exactly how this was done is described in Dungey and Pagan (1996). With the aid of these figures we can now study the last few cycles and the role of policy in them.

One problem when considering the impact of monetary policy is to decide between the effects on expenditure or output as the proper measure. If we first concentrate upon GDP, monetary policy in the recession of the early 1980’s seems to have been mildly expansionary, with any expansion becoming contractionary from 1985Q1 onwards, remaining so during the downturn phase of the growth cycle experienced in 1986. As the strong recovery from that event got under way monetary policy was in the proper contractionary mode, except just at the peak of the cycle. Although the right sign to policy was achieved it didn’t take much off the boom. It wasn’t until 1990Q1 that monetary policy made its first negative contribution to output, at which time the peak of the cycle had been reached. To some extent this outcome stems from the fact that the October 1987 stock market crash seems to have delayed a tightening of policy until around May 1988, but it also owes something to the slowness of monetary policy to have an impact upon the economy. What is evident from the figure is that “the recession we had to have” is more appropriately labelled “the recession we would have had”. Monetary policy does not seem to have initiated the recession and a soft landing i.e. output returning to trend (zero on the graph), or just below it, was never on the cards. Monetary policy did act to bring activity back towards trend faster than would have occurred naturally, but, other events, initially domestic demand but, increasingly, external shocks, are the main driving force of this episode. Interest
Fig 6: MONETARY POLICY EFFECTS ON DOMESTIC GDP

Detrended log real GDP
- no monetary policy effect

Monetary policy effect

Detrended log real GDP
rate cuts beginning in early 1990 also took a long time to flow through to a positive stimulus to output; the first such effect being recorded in 1992Q1. Nevertheless, although delayed for a long time, monetary actions clearly assisted the economy to climb out of recession much more quickly than if policy had been neutral.

Although the story is much the same for GNE it is clearly the case that the effects of monetary action are far greater, both in reining in excess demand and stimulating it. The contrast between the effects of monetary policy on GNE and GDP must of course be due to the response of the current account to monetary actions. Crudely, one can account for the discrepancy with the following conceptual experiment. Imagine that monetary policy only impacts upon investment and that all investment goods are imported. Then any effect of monetary action upon GNE will occur alongside no effect upon GDP. Consequently, as the task of monetary policy is really to manage aggregate demand, one feels that Figure 7 is a better guide to its effectiveness.

5. Conclusion

Most researchers study business cycles for the insights available for policy design. That connection was not covered in this lecture but, returning to the topographical analogy we began with, it is an unfortunate fact of life that the mountains and valleys of the economic landscape are frequently covered with fog, and the challenge is to complete a guide book that enables us to know better what is in that fog. Such a guide must of necessity be rooted in the terrain we have passed over, and a study of the latter is an indispensable beginning to the construction of the former. Many have already contributed to the formation of such a guide book and Shann would score a prominent acknowledgment in any Foreword to it. In this lecture we have traversed the history, theory and econometrics of business cycles with the aim of making some suggestions about what should appear in any guide book. It is painfully apparent that much needs to
be done in all these areas in order for us to fully comprehend the dimensions of the
Australian cycle, and I look forward to a future Shann lecturer completing the task.

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