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THE AUSTRALIAN MACRO ECONOMY  
Stanley Fischer and Rudger Dornbusch  
DISCUSSION PAPER NO. B5  
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DISCUSSION PAPER SERIES B

THE BROOKINGS SURVEY OF THE AUSTRALIAN ECONOMY

Proceedings of the Conference held on  
9-11 January 1984, Canberra

Conference Discussion Relating to the Paper

THE AUSTRALIAN MACRO ECONOMY\*

Stanley Fischer and Rudiger Dornbusch  
M.I.T.

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- \* The papers delivered to the Brookings Survey Conference were subsequently revised and have now been published in: The Australian Economy - A View From the North (Eds. Richard E. Caves and Lawrence B. Krause), published by The Brookings Institution, Washington DC, and George Allen and Unwin, Sydney, 1984.

This paper contains the comments prepared by two discussants for the Conference Session on the Fischer-Dornbusch paper, together with the edited proceedings. The comments and discussion refer to the paper delivered at the Conference. A limited number of copies of that paper are available and can be obtained on request from the Centre for Economic Policy Research.

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THE AUSTRALIAN MACRO ECONOMY  
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FIRST DISCUSSANT: DR ADRIAN PAGAN, Australian National University

I propose to summarize this paper with the aid of the framework stemming from the theory of policy. This theory points to a number of issues that must be analysed in any policy discussion, six of which are relevant here.

1. The definition of targets, instruments and goals (the last having some variability constraint on them but with no specific quantity being aimed at) and the weights to be attached to each.
2. The policy model to be used in mapping the instruments into targets and goals.
3. Are there any key relationships underlying the model which demand special attention when calibration is performed?
4. How stable are the relationships?
5. Does the system decompose in such a way that specific pairings (or assignments) of instruments to targets can be made?
6. What is the impact of exogenous variables upon model solutions and how can one design policies to offset these influences?

I will regard the Dornbusch-Fischer paper (from here on referred to as D-F) as a response to these six items. In doing so I have not entirely covered their material, largely ignoring the historical first section which introduces some puzzles (OECD vs RBA trade shares) and which highlights some interesting facts (the huge decline in wool for Australian exports and our changing trade shares). Furthermore, although logically discussion on the first point should come before the others, I will treat it last, in line with D-F's approach.

1. The Model (pp.11-27)

D-F propose a model that, depending upon what one wants to determine, can be quite complex. The 15 equations below capture, I think, its major elements. Those asterisked do not appear explicitly in D-F. Two sectors are distinguished: the export (e) sector consisting of agriculture and mining (A-M), and the (d) sector composed of manufacturing and services (M-S).

- |       |  |                     |
|-------|--|---------------------|
| (1)   | $M/P = L(i, Y)$                                | money DD            |
| (2)   | $M = m(R+DC)$                                  | money SS            |
| (3)   | $qk = J(i-p, i^*+\hat{e}-p, i, v, Y, a)$       | real asset DD=SS    |
| (4)   | $a = qK+B/P + FA + (R+DC)/P$                   | wealth identity     |
| (5)   | $B/P = D(i-p, i^*+\hat{e}-p, i, v, Y, a)$      | bond DD             |
| (6)   | $Y_e(eP_a/W, k_a) = D(P_a/P^*, Y^*)$           | output A-M          |
| (7)   | $Y_d = A(q, i-p, G, eP^*/P, P_d/P, a, Y) + NX$ | output M-S          |
| (8*)  | $NX = NX(q, eP^*/P, P_d/P, a, Y, Y^*)$         | net exports         |
| (9)   | $P = P(eP_a, P_d, eP^*)$                       | agg. price level    |
| (10)  | $P_d = xW$                                     | prices M-S          |
| (11*) | $\Delta R = NX + \Delta FA + \phi(FA, i)$      | change in reserves  |
| (12)  | $Y = (P_d/P)Y_d + (P_a e/P)Y_e$                | agg. output         |
| (13*) | $FA = F_a(i, i^*, \hat{e}, v)$                 | net for asset hold. |
| (14*) | $p = P - P_{-1}/P_{-1}$                        | agg. inflat. rate   |

In keeping the system to this size the sectoral allocation of capital is not determined (although D-F p.21 clearly have this in mind). Within this system there are 17 potentially endogenous variables ( $M, P, i, Y, R, q, p, \hat{e}, a, B, FA, Y_d, e, P_d, NX, P_a$  and  $W$ ). Three extra equations are needed to close the system, and these are chosen to tie down exchange rate expectations ( $\hat{e}$ ), the nominal wage ( $W$ ) and the exchange rate ( $e$ ). The main possibilities are given below in (15)-(17).

- |       |                                     |                |
|-------|-------------------------------------|----------------|
| (15a) | $\hat{e} = \dot{e}$                 | rationality    |
| (15b) | $\hat{e}$                           | predetermined  |
| (16a) | lab. market clears, labour SS fixed |                |
| (16b) | $W = W( )$                          | Phillips curve |
| (17a) | $\Delta R = 0$                      | flexible rates |
| (17b) | $e$ fixed                           |                |

Choosing one of each alternative in (15)-(17) enables the solution of the D-F system. The model is therefore an interesting amalgam of the two streams of economy-wide modelling represented in Australia. On the one hand, the macro models such as NIF-10S and RBII have equations corresponding to (1)-(5), (9), (11-14), (15), (16b) and (17) but do not provide a sectoral split of output. On the other hand, simplified versions of ORANI e.g. the BOTE model, have the sectoral split of output in (6), (7) but neither nominal variables nor financial markets. As well, the latter do not have the mark-up pricing of (10) in the M-S sector, although there is a "Keynesian" version which does so. There is a lot to be said for integrating the two streams, as sectoral issues can sometimes be of importance for general macro policy.

All models are built for specific purposes and based on authors' prior beliefs, so that criticisms are very much in the eye of the beholder. Many would not like equation (10)'s infinite supply elasticity in the M-S sector and indeed it does have an important implication - as the A-M sector contracts in size the constancy of the mark-up means that one can never get a real wage increase in the D-F economy. In fact the D-F economy has the implication that the relative prices  $P_d/P$  are a function of the determinants of the Phillips curve (P-curve hereafter). As real wage rises figure prominently in their paper there is therefore something of a problem here with their model.

A second issue concerns the lack of a specification for  $v$ , the expected rate of return on real assets. Although animal spirits are sometimes in evidence, one cannot escape the fact that both policy-makers and theoreticians - Malinvaud (1980), (1982) - have been more inclined to treat  $v$  as endogenous in recent years. Suppose that, in a simpler model, we had real consumption depending upon labour earnings while the investment ratio was a function of the rate of return. With constant labour productivity,  $C = C(W/P, Y)$ ,  $I = I(W/P, Y)$  and  $Y = C + I + G$  gives  $\partial Y/\partial G = (1 - C_w(\partial W/P/\partial Y) - I_w(\partial W/\partial P/\partial Y) - C_y - I_y)^{-1}$ . Since  $I_w < 0$  there is the possibility that the multiplier can be made much smaller by endogenising the rate of return. Estimates of  $I_w$  in Pagan and Gray (1983) put it above unity, so that  $|I_w| > |C_w|$  and any induced real wage rise following from expansion acts to offset policy, even ignoring interest rate effects. Of course there will be timing differences, and it is not impossible that the initial expansion of output as real wages boost

consumption is followed by a contraction as investment slumps. Such a channel of crowding out sometimes seems to be more important than the traditional one through interest rates, and appears in the NIF-10 model for example - see Perrazelli and Perrin (1983). One interesting feature of the above is that the greater the flexibility in real wages, the lower the multiplier when  $(|I_w| > |C_w|)$ . The reason is obvious. As normally specified, the P-curve is a two-edged sword. Greater real wage response to contractions means greater responses to expansion and, unless one therefore believes in an asymmetric response currently, efforts to increase flexibility may actually be counter-productive. Not much attention has been paid to the question of symmetry in Australian P-curve research, so I briefly looked at it with Gregory-Smith's (1983) data. Although there is a difference to positive and negative changes, it was neither large in magnitude nor significant.

A further element missing in their model is confidence effects. As Argy (1983) has shown, introducing them can produce unusual results. Nevertheless, I am rather sympathetic to D-F on this issue. Treasury seem very keen to introduce psychological shift factors into equations whenever it suits them, but to adopt a strong rationality line normally. However, it probably should be borne in mind that it is the assumptions embodied in the D-F model which give it the standard neo-classical/Keynesian synthesis responses. Until recently, I didn't think we had a "Liverpool" or St. Louis model in Australia, but reading Sieper and Fane (1981) makes it clear that we do have some pretenders in that direction. Thus, the D-F formulation won't be everyone's cup of tea.

## 2. Crucial Relations (p.29-41)

As the new classical economists have demonstrated, it is possible for the results of any model to depend solely on one equation - in their case the surprise-based aggregate supply function. As this can be inverted to produce a P-curve, it is not surprising that D-F devote a lot of time to this relationship, stating that "the empirical issues associated with the Phillips curve are central to the Australian policy debate".

Traditionally Australian specifications of the P-curve have related the

real wage to measures of labour market tightness.<sup>1</sup> Measures of the latter have varied, with unemployment, vacancies or combinations of these variables being the most significant. The advent of the NIF-10S model marked a break with this tradition, using the level of overtime as the determinant of real wages, and showing that overtime was essentially determined by the change in unemployment. Gregory and Smith (G-S) (1983) seized on this shift and argued that the transition could be interpreted as one of labour market pressure in the economy and in firms. Perhaps another variant would be a dual labour market theory in which the real wage is determined mainly by those currently employed. Certainly, a lot of the argument buttressing the distinction concentrates upon the security of jobs.

D-F mount an attack upon this view on a number of fronts. The first sally involves data. They replace the AWE definition of NIF-10S/G-S with an index of hourly wage rates in manufacturing. A more precise definition would be helpful e.g. is it award wages or earnings? Justification for the change is weak, revolving around the need to avoid compositional and hours of work effects. To the extent that these are unrelated to changes in the labour market, fair enough, but part of a recession is to substitute part-time for full time work and to reduce the length of the working week. Provided compositional responses are symmetric there seems no really good reason for abandoning AWE. It may be of interest to observe that estimating equation (16) with AWE i.e. doing G-S's unemployment/AWE regression with a time trend added, results in a t-value of unemployment of over 4, so that AWE seems more sensitive to unemployment.

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1. I will ignore completely the question of whether price inflation expectations in a nominal wage inflation equation should have a coefficient of unity or not. Almost all protagonists seem to accept this, so we might as well look at real wage equations.

As well, D-F make something of the fact that the addition of a time trend is needed to make unemployment significant (f.n. 35) and that G-S's work is deficient because of this omitted variable (or at least some omitted variable). I am rather sympathetic to them on this matter as Ramsey's RESET statistic also indicates mis-specification in the corresponding G-S equation. However, when one replaces unemployment by overtime, the time trend becomes totally insignificant (a t-value of only .6). Hence the D-F strictures do not apply to G-S's preferred equations.

A more appropriate response to G-S is to change the list of regressors. First, D-F argue that unemployment is significant post-78(4), in contrast to G-S's claim of insignificance. As their assertion revolves around a t-value of -1.97 (D-F) versus (-1.87) (D-F) it is not very convincing. What is more interesting about the D-F regression (Table 7, eq. (18), p.33) is the magnitude of the inflation expectations coefficient. As the data is presumably identical except for the expectations series (although the D-F sample starts in 66(1) instead of 66(4) the coefficient on  $P^e$  in G-S of .788 vs. 1.31 for D-F makes the measurement of inflation expectations crucial to the argument. Neither group is very good in describing (and justifying) their measures. D-F say that they use "a forecast of the rate of CPI inflation, derived from a prediction equation based on lagged rates of price and wage inflation", while G-S use "the rate of change of the CPI, from one quarter to the next, lagged four quarters". Both are a bit of a mystery to me.

As can be readily appreciated the differing views depend upon the pressure of an unemployed pool making employed workers willing to accept real wage cuts, the nature of Australian labour markets making it hard to engineer them otherwise (although by substitution some of the effect might be obtained) D-F doubt that existing workers are not affected.<sup>2</sup> This seems an area in

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2. It is implicitly accepted by D-F that it is either unemployment or overtime that should be the regressor. There seems no reason why both should not enter, allowing both the employed and unemployed a say in real wages. The only study I know of doing this - Tony Jowett in the M.Ec degree this year - found that both overtime and the change in unemployment were significant.

which we could do with some good survey material on attitudes. Occasionally one sees reports of surveys done of union leaders indicating that they feel there is pressure upon them by their employed members to seek nominal wage rises, even though there is substantial unemployment generally. In lieu of such information it is interesting to look at the results of two questions from the Morgan Gallup Poll.

TABLE 1: Security of Jobs

	<u>75</u>	<u>76</u>	<u>77</u>	<u>78</u>	<u>79</u>	<u>80</u>	<u>82</u>	<u>83</u>	
							<u>Oc.</u>	<u>Dec.</u>	<u>Oc.</u>
Present Job Safe	76	78	82	79	77	73	74	72	79
Chance of Unemployment	21	17	15	19	20	23	22	25	18

Source: Morgan Gallup Polls

TABLE 2: Employment Offering

	<u>75</u>	<u>76</u>	<u>77</u>	<u>78</u>	<u>79</u>	<u>80</u>	<u>82</u>	<u>83</u>	
							<u>Oc.</u>	<u>Dec.</u>	<u>Oc.</u>
Take New Job Quickly	57	57	55	50	54	50	52	44	51
May Take Longer	33	33	35	41	36	39	39	46	41

Source: Morgan Gallup Polls

Comparing the end-points, one notes that in 1982 the fear of unemployment seemed no greater than that in 1975 among those with jobs, although it had been subject to quite a deal of fluctuation (a correlation with the change in unemployment would seem a reasonable hypothesis). Those who would take a new job quickly had actually declined and seemed to be engaging in longer searches. Of course it is hard to be certain about respondents' attitudes to these questions. Hesitancy in taking a new job might reflect a fear that one could become unemployed in a novel environment - better the devil you know. However, for what they are worth, these tables seem to support the NIF-10S/G-S line of argument. The employed are pretty insensitive to the rate of unemployment. What we really need is much better data on this question, and it seems that the collection and analysis of such material should be a prime task. This would seem a better alternative than attempting to infer the extent of sensitivity from aggregate macro data.

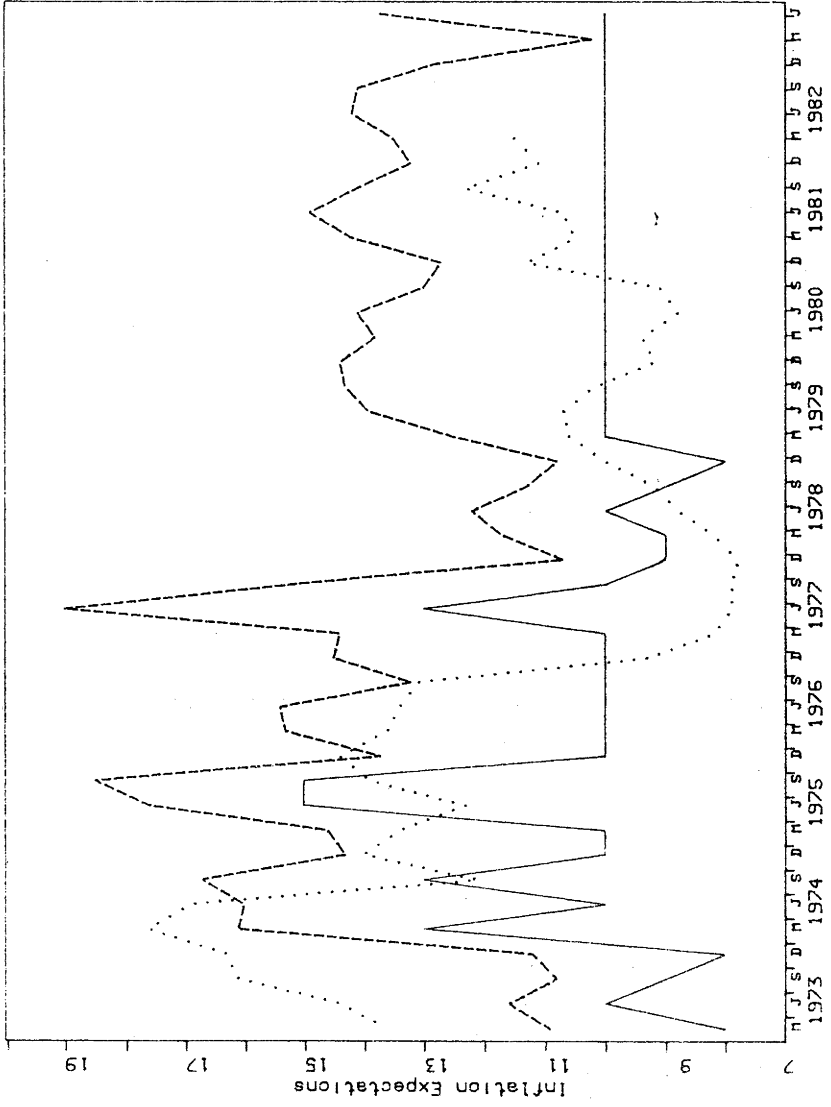
D-F move on to look at an expectations-based account of why real wages rose post-78(4) in the face of substantial unemployment. They observe the overstatement of actual inflation given by the mean of the IAESR's survey of consumer responses to an anticipated inflation question, and infer that this alone could explain a lot of the nominal wage behaviour of this period. Below I graph the mean and median of the IAESR data, as well as the actual inflation rate (note that the actual rate has been translated to match up with the question being asked of anticipated inflation over the next four quarters). The divergence between the mean and median does, I feel, make the former very suspect as a measure of central tendency. The reason for the divergence is twofold - some large outliers caused by the nomination of negative and 100% inflation rates, and a bunching effect whereby anticipated rates centre on multiples of 5. Given the relative stability of inflation over this period, I believe the median is a better measure.

Perhaps the most challenging part of D-F is their development of a new P-curve emphasising wage resistance (p.40). They augment the P-curve with the ratio of the previous peak real wage to previous quarter real wage less unity (RWR), concluding that "The real wage variable appears to solve the Phillips curve puzzle". A first comment on this claim is that the greatly improved  $R^2$  of .82 stems partly from the fact that, until 74(4), the RWR variable was almost always zero (I measured RWR with AWE as I didn't have their data and it peaks in 76(1) rather than the 76(3) they have in their series). Now 74(3) represents an outlier in the data, exhibiting a nominal wage rise of 10.97%, double that of any other quarter. Taking it out of the data causes one of G-S's regression  $R^2$  to increase from .24 to .52 for example (taking it out reduces the effect of the overtime variable substantially). Hence, a good part of the increase in the  $R^2$  D-F obtain is due to the fact that they've essentially allowed a shift at that point.

Given D-F's success it seemed worth repeating G-S's analysis with a real wage resistance term constructed in the same way but with AWE. When added to G-S's equations, however, either it was not significant or, if significant, it had a negative sign. I find eq.(23) on p.40 of D-F therefore a bit puzzling. Looking at my numbers, the variable RWR is basically zero up to 1974.

Fig. 1: Inflation Expectations  
1973(1) to 1983(2)

Median 1  
Actual ..... 2  
Mean ----- 3



Thus, until then the rate of change of the real wage would be  $-.13 - 1.91UN - .23 INNFOR$ . As all these variables are positive, a decline in real wages would be predicted up to 1974. A similar problem occurs at the other end when  $RWR = 0$  with the rising real wages of  $80(4)-82(4)$ . I find it hard to reconcile these facts with D-F's results. During the indexation period  $RWR$  was high but nominal wage growth was low (this is presumably why I get a negative sign to  $RWR$  with G-S's data). The only explanation seems to be that the extensive serial correlation in D-F's equation takes account of the under-prediction of nominal wages pre-1974 and post-1980 and the over-prediction in the indexation period. Obviously some reconciliation between (40) and results based on G-S's data seems called for before one can accept that the P-curve puzzle is really solved.

3. Stability (p.12-13, p.42-47)

D-F raise two stability issues. One of these relates to the money market equations (1) and (2). Elsewhere, Pagan and Volker (p.11, 1981) - I've argued that instability in (1) (for  $M1$ ) was not in evidence over most of the 1970's, and that those "discoveries" of it are a consequence of equation mis-specification. However, the 1980's may be a different story. Mike Holmes, in his M.Ec Case Study this year, observed that the P-V money demand function was not completely stable in the face of the financial innovations of the 1981/82 years, and the presence of Bankcard could have resulted in a slow change in elasticities difficult to pick up without a substantial length of data. My own feeling is that any instability in (1) is only transient, and that (2) is likely to be more of a problem. I don't think I've ever heard of a stable money multiplier estimated from Australian data.

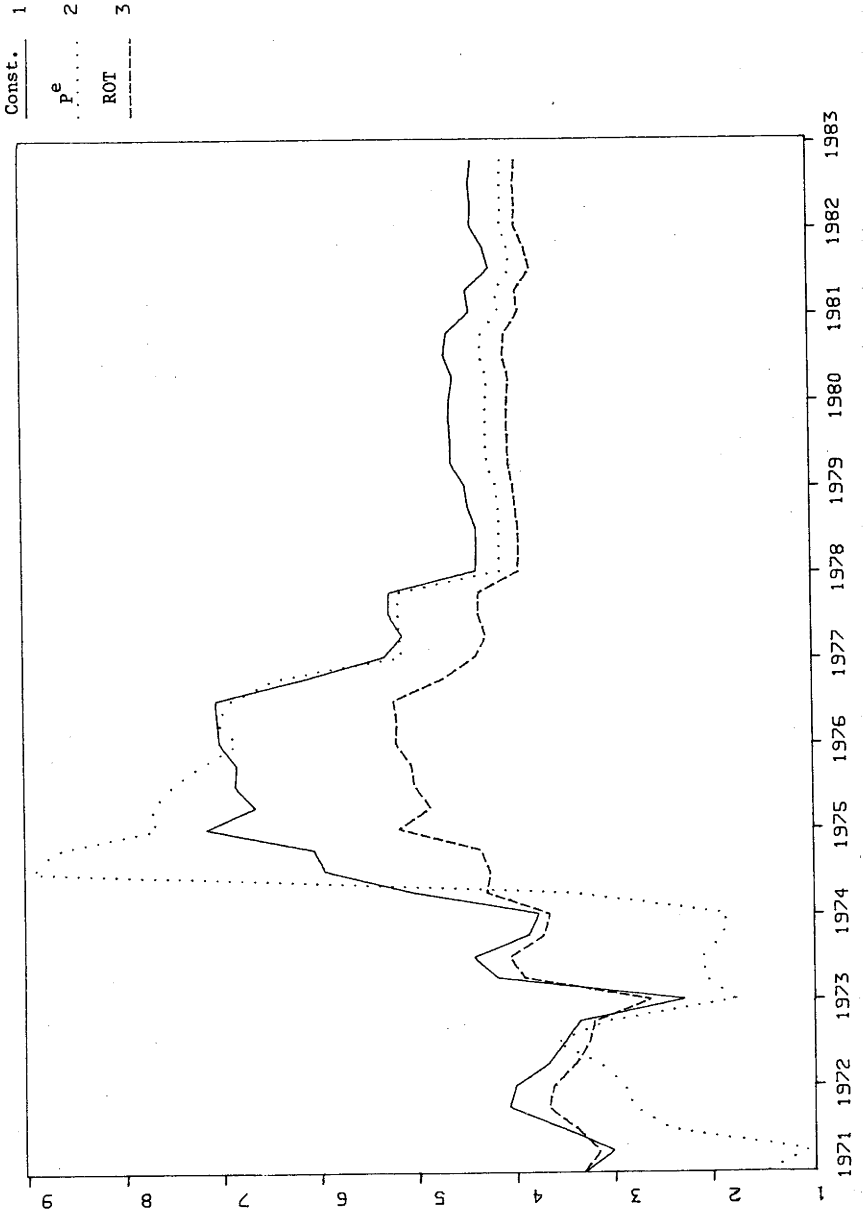
The second aspect of stability concerns the P-curve and the extent to which the Arbitration Commission (AC) can administer shifts to it. D-F spend some time reviewing the assertions and evidence on this, concluding that "on balance the weight of the evidence is that the AC does contribute independent noise to wages". Presumably the use of the word "noise" connotes a transient rather than permanent shift, although they do seem to view the AC's 1972 equal pay decision as a permanent influence. This is an important question, which will probably be taken up in discussion on Mitchell's paper.

My own belief is that the timing is almost certainly affected by the AC's actions, but I am less convinced that there is any more to it. One way of getting some idea of the impact of the AC, is to ask if P-curve coefficients remain stable across the indexation period. Fig. 2 below shows recursive estimates of the coefficients over 1971/1 to 1982/4 in a G-S equation that has a constant term, price expectations and an overtime variable. There is evidence of two sharp changes in the P-curve over the whole period. The first jump occurs around 1974/2 and the second about 1976/4. After 1977/2 the relationship is a very stable one and the influence of demand is the same in 1982/4 as it was in 1972/4. One doesn't find a lot of evidence here that the AC have shifted the P-curve. One would either expect the constant term to have a smaller negative value or the overtime coefficient estimate to become smaller. The only way one might rationalise it is if one thought that the 1974/5 estimates represented a new P-curve, so that the decline in the effect of overtime could be attributed to the AC. However, one still has to explain why the relation is stable from 1977 forward, which covers some 4 years of indexation and 1½ without it.

4. Assignments(p.48-54)

The major assignment D-F look at is that of real wages to unemployment. Here the question posed is whether unemployment is classical or not; if it is we can assign the real wage to unemployment. D-F distinguish two aspects in any debate over the existence of classical unemployment. Firstly, there is the question of whether a demand expansion would lead to a rise in employment without a real wage reduction. Secondly, there is the argument over whether the rise in employment engineered can bring us back to *full employment* without a concurrent real wage reduction. To separate classical and Keynesian unemployment they recommend a comparison of marginal product with the real wage. Such a test sits uneasily with their own model, as they observe (p.24), "in the manufacturing and services sector.....aggregate demand determines output". Very elastic supply of course underlies almost all proposals for demand expansion. D-F's classical unemployment case would, in any open economy, be better distinguished by a loss of competitiveness, as it is this which determines the level of demand (ignoring non-traded goods).

Figure 2: Recursive Estimates of G-S Equation



Is it reasonable to argue that an expansion of demand either government or export lead, would not cause a rise in output? I think it is very hard to do so currently. Burns and Mitchell (1983), to whom D-F give only scant attention, constitutes the best theoretical case for demand expansion constructed from the standard rationing viewpoint. To support their theoretical case they present data from a VCM survey showing that 42% of companies are currently operating below 70% of capacity versus 23% in June 1982. They quote the VCM's interpretation of the survey "that declines in production levels ..... are largely due to depressed levels of consumer and industrial demand" and that "any increase in orders will swiftly be translated into increased production". This survey echoes a persistent theme of other such surveys in recent years; it is a lack of orders that deters output expansion.

By and large D-F ignore this debate, concentrating instead upon whether the real wage is too high for full employment i.e. whether macro policies need to be of the sort in Dixon et.al (1979) which feature an expansion of output and a real wage reduction. To answer this question they present indexes of real labour costs, arguing that such series "show how much the marginal product of labor at full employment would have had to rise". This criterion effectively demands the restoration of the marginal return to capital to that prior to 1972. Neither technical change nor factor substitution was of sufficient plausible order of magnitude as to suggest that this was possible, and hence they conclude that we have a real-wage problem. Exactly why we need to restore the old rate of return is not at all clear; as Malinvaud (1982, p.11) says "no compelling reason exists to believe that any of the remuneration rates ought to be today at the level achieved in 1973".

In all of their discussion over the effects of a real wage rise D-F never explain exactly how a real wage rise comes about. After all both numerator and denominator are normally endogenous. Hence the explanation of the emergence of unemployment "over the two years 1973 to 1975" as "quite likely classical unemployment" needs to be looked at more closely. As we observed above, one method of assessing the existence of classical unemployment in an open economy is to examine indexes of competitiveness. But these are

composed of two parts. If the price part deteriorates badly it seems reasonable to conclude that we do have classical unemployment. If, however, it is the nominal exchange rate part that exhibits the adverse movement, we probably have to conclude that the unemployment is initially induced by policy reducing demand for domestic output by switching it to imports. Of course this just deals with the genesis of unemployment, and not necessarily why it is sustained.

Table 3 below shows the change in competitiveness over the years 1973-75 and 1980-82 (the second series to look at the major recession emerging in 1982). The contribution of exchange rate changes is also shown. Notice that two different bases are used - 1971(2) for the first recession and 1979(4) for the second. These are taken to be "normal" periods.

TABLE 3: Competitiveness Changes

		<u>Compet</u> <sup>(a)</sup>	<u>Exchange rate</u> <sup>(a)</sup>			<u>Compet</u> <sup>(b)</sup>	<u>Exchange rate</u> <sup>(b)</sup>
1973	M	-11.3	-11.1	1980	M	-.6	-2.1
	J	-8.4	-8.7		J	.75	-1.5
	S	-9.8	-14.3		S	.47	-2.7
	D	-13.2	-19.0		D	-.5	-3.8
1974	M	-9.6	-18.0	1981	M	-2.1	-6.2
	J	-9.7	-18.3		J	-6.0	-11.2
	S	-6.4	-7.0		S	-6.1	-12.6
	D	-6.8	-7.2		D	-3.5	-8.5
1975	M	-7.4	-7.0	1982	M	-2.8	-7.7
	J	-7.6	-7.2		J	-2.8	-7.3
	S	-7.0	-8.0		S	.09	-2.1
	D	-9.4	-7.6		D	-1.1	-1.5

Source: Constructed by National Australia Bank, AIDA Bulletin, Aug. 1983.

(a) Relative to 1971(2)

(b) Relative to 1979(4)

One finds little support for classical unemployment ideas in Table 3, exchange rate movements absolutely dominating changes in the competitiveness index in both recessions. Of course, this index is not without its problems being based on prices and not wages. For the 1973-1975 period, in which there were movements in other costs overseas not felt to the same extent in Australia, this may be reasonable but, as AIDA argued in their April 1983 Bulletin, on - costs for labour have made them increase faster than earnings over the 1970's. Nevertheless, proponents of classical unemployment at the initiation of these recessions need to build up a better case than currently available.<sup>3</sup>

5. Exogenous Variables and Policy Design (p.55-64)

Much of the analysis produced by D-F under this heading concerns the impact of exogenous variables such as the quantity and price of exports and the appropriate design of policy to such shocks. A lot overlaps with Heliwell's brief and I won't say much about it. They do however make important comments about monetary policy in a flexible exchange rate regime, which we are now in. As they note in discussion of the overshooting problem, the discarding of the exchange rate as an instrument makes it imperative that a lot of attention be paid to the monetary policy. The only disagreement I would have with most of the material is the inference that the mining boom of 1979-1982 was perhaps not that important, as mining investment was below 3% of GDP. From 1979/1982 mining business investment rose by approximately \$1100 million in 1979/80 prices while business investment in the base metal part of manufacturing rose by around \$1500 million. So the associated investment was really quite important, particularly when one notes that the non-base metal investment actually declined by \$200 million.<sup>4</sup> Without this investment stimulus, largely autonomous, we would probably gone into a recession with the rest of the world.

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3. It is entirely possible that the initial increase in unemployment in 1973-1975 was due to demand deficiency but that the second jump in 1977 was classical.
  4. Figures supplied by Alan Hall. Constructed from the Survey of New Fixed Capital Expenditure with allowance for leasing.

6. Instruments and Targets (p.64-75)

The first problems are to define targets and instruments. D-F infer, rightly I think, that by-and-large Treasury only have one target, inflation, either because they give a low weight to other objectives or because their model allows one to hit multiple targets with a single instrument, tight fiscal policy. They read the "inflation first" idea as coming from perceived negative impacts of inflation on demand, but are of the opinion that this is either exaggerated or is itself the product of failing to encourage indexation. My only disagreement on this score is their comment that Pagan and Gray (1983) attribute most of the effects of inflation upon investment to the interaction of taxes and inflation. Most of the reduction in investment during the 1970's was not in fact explained by this interaction. It could be that inflation does produce a shift in the rate of return and David Andrews in his 1983 Mec Case Study finds a very stable relationship between profit share and inflation over the whole period since 1953/4. This may of course reflect the fact that, with fixed nominal exchange rates, exporters and the import-competing sector experience squeezed margins with inflation, and that this might be offset if the real exchange rate had been fixed, but the question is certainly still open.

Other instruments and targets are discussed. Perkins gives a 2-instrument, 2 target solution while Parkin puts all the instruments into zeroing inflation. They point out that his strategy of sharp disinflation can have nasty exchange rate effects and warn against it.

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**SECOND DISCUSSANT: DR PETER JONSON, Reserve Bank of Australia**

This paper seeks to show that Australia is a "textbook" example of a small open economy. It succeeds to such an extent that now no Australians will need to buy the famous textbook by Dornbusch and Fischer (from here on referred to as D/F) - why buy the book when you can get all the messages in the shorter version with local characters in the main parts?

It seemed to me almost as if George Lucas had made a local TV length version of Star Wars. Although some of the characters have suffered some interesting transmutations in the process, most are clearly recognisable. In particular, as Dornbusch/Fischer recognise, procedures for setting wages (is this the Darth Vader character?) are different here - although how much difference this makes is not all that clear to me from the paper. I was also struck by the relative unimportance of the money supply in the local version of the epic - although perhaps the role of money in generating, or at least validating, inflation is regarded as being so obvious that the authors do not see the need to instruct us on this issue. My knowledge of Star Wars has just about run out so I am unable to provide the telling analogy with the missing character. But I shall return to one or two points about the role of monetary policy.

Before moving on from these introductory fantasies, however, I might register a modest query about the style of the production. Even TV length treatments can drag, especially when the advertisements are dull. In discussing the world, I prefer that the author's theory be implicit, as it was in the brilliant practical writings of Harry Johnson, for example, or articulated

properly in the form of a fully specified macro-econometric model. I did not expect Dornbusch/Fischer to provide the latter. I do, however, wonder at the usefulness of the section of their paper which provides a highly simplified, in my view over-simplified, theoretical model, especially since this model is really rather disconnected from the practical discussion. I for one would be more or less content to accept the D/F assertion that "the key structural equations and mechanisms are the same in Australia as they are in the standard textbooks". But if there are any sceptics on this subject a few highly abstract equations won't do much to convince them.

My substantive comments are organised under three headings:

- . The Australian Phillips Curve and Inflationary Expectations
- . Monetary Policy and its Analysis
- . Other Issues for Policy.

In each case I shall table some questions which I hope will be taken up either in discussion at this conference or by the authors in revising their paper.

#### **The Phillips Curve**

Many strong men and women have grappled with the Australian Phillips Curve. Indeed Bill Phillips followed up his famous 1958 article in *Economica* with a monograph for the New South Wales branch of our Economics Society called "Wage Changes and Unemployment in Australia: 1947-1958". His paper covered the Korean War episode in which wage and price inflation went close to 30 per cent and provided him with plenty of useful variation in the data.

Phillips was, of course, highly interested in the effects of demand pressure on wage settlements but he also took pains to allow for imported inflation and the effects of the Arbitration Commission. His regressions were ingenious but modestly tucked away in footnotes. Even though I have contributed, probably excessively to the subsequent literature I do not think that the state of the art has advanced much. D/F have also been ingenious. Perhaps their most interesting construct is the variable purporting to represent the effects of "real wage resistance", but as they recognise, it is effectively what the statistical pedants would call a "synthetic variable" and therefore open to all the usual objections. However, their econometric results, and their robust commonsense, leads them to conclude, as most others have, that the "unusual" method of setting wages in Australia has had a noticeable effect on the inflationary process.

However, to my mind the classic questions about this remain unanswered. They might be summarised as follows:

1. How does the Arbitration Commission influence the inflationary process in Australia?

Our own modest efforts to answer this sort of question have mainly sought to unravel the effects of inflationary expectations both on centralised wage judgements and on what used to be called "earnings drift". One early paper, also an Economic Society monograph, found a role both for a measure of price expectations and "union aggressiveness" in explaining the growth of earnings in Australia. However, there was also a strong independent influence of minimum wage rates, as in the Phillips paper and in most subsequent work on the subject. Incidentally, the particular

measure of price expectations was explained in another study partly by the growth of the money supply. This early and somewhat fumbling attempt to explain inflationary expectations has been followed up in some other interesting Australian papers. There is also that very impressive recent literature on "Rational Expectations". I was in fact, rather disappointed that not much in this vein appeared in the D/F paper. It leads to my second question:

2. What determines inflationary expectations in Australia and how do these expectations influence the inflationary process?

There is also the question of quantities in the labour market. In this area, I found much stimulus in D/F's discussion. Obviously the recent work of Bob Gregory and others on implicit contracts is important and should be pushed further. My impression is that, after a bit of hopping from foot to foot, D/F have accepted that increases in real wages have had a pretty big impact on unemployment in Australia. Perhaps, however, this should be listed as a question so that we can get the answer as clear as possible. My third question is therefore the following:

3. How important have increases been in real wages in excess of productivity in generating the current high rate of unemployment in the Australian economy?

#### **Monetary Policy and Its Analysis**

On this subject I begin by giving top marks to D/F for their conclusion that they "see little alternative to floating exchange rates". In this judgement they have already been vindicated by local history - indeed in true Star Wars fashion they could

probably claim that their drafting had a telepathic impact upon Australian policy making. They go on to say they think any floating exchange rate should be managed. Of course, exchange rates can be managed in many different ways. My next question therefore is:

4. What principles should be followed in managing our new floating exchange rate system?

D/F suggest that with a floating exchange rate domestic monetary policy should be used "flexibly" to offset external disturbances. It would, in fact, be very helpful if the revised version of this paper provides some guidance on what a floating exchange rate means for economic policy. We all remember our Mundell but in the real world of the Australian economy with a centralised wage system (which D/F endorse) and full indexation, how are the precepts of simple theory altered? If I put this in the form of a specific question it would be:

5. What are the implications for Australian economic policy of the floating exchange rate?

I suspect in analysing this subject it will be necessary to get away from the simple equilibrium model of the money market which D/F more or less endorse. I have noticed, of course, that they have not ruled out disequilibrium in the sense used in the RBII model - that is, situations in which money supply is not equal to the long-term demand for money. I would also wish to comment in passing that D/F's preference for a money multiplier approach makes it unnecessarily difficult to analyse the influence of the budget deficit and open market operations.

However, I wish to come to a more substantive matter. It is a point which I think needs to be applied equally strongly to

Andrew Carron's paper. There is a fundamental sense in which the Australian financial system is currently in disequilibrium. In recent years we have seen some major innovations in financial markets and many changes to regulations (the latest are the changes to exchange controls and the move to a floating exchange rate). This situation will affect both money demand and money supply and financial relationships more generally. Any guidance which can be given on the properties of a financial system undergoing rapid evolutionary change will be helpful. I do not see any hint of an analysis of this crucial issue in the textbook expositions before us. My next question may be unanswerable but I put it on the table as a major challenge for economic research in Australia:

6. How can a rapidly evolving financial system be analysed?

#### **General Issues for Policy**

The first of the general issues for policy concerns the ways in which we set wages and other incomes in Australia. D/F come down in favour of some sort of incomes policy. It seems to me that they reach this conclusion somewhat reluctantly, based on arguments which are not as tight as in some other areas. For example, they are of the view that if the Arbitration Commission were "somehow abolished" then Australian wage determination would show "all the discipline of the British system" - whatever that means. It is interesting to contrast their views with the more traditional approach of Bill Phillips. The final sentence in the monograph to which I referred earlier reads as follows:

"Replacement of the practice of negotiating agreements covering wide groups of workers by a more flexible

system of local negotiations would not only help to solve the problem of reconciling the objectives of full employment and stable prices, but would also lead to wage differentials more conducive to economic mobility and so would help to promote economic efficiency and economic progress."

It is fashionable to assert that change in this area is well nigh impossible. However, until very recently, similar statements were common about the possibility of reforming the financial system. It is interesting to note that the labour market in Australia is beginning to stand out as much more regulated than other markets. If deregulation is sensible elsewhere perhaps we should ask why similar arguments do not apply to the labour market. I personally think attempting to grapple with this question would be more helpful than some ad hoc conjectures about the attractiveness or otherwise about specific forms of income policies. This leads to my next question:

7. What is the best system for setting wages and how should we go from where we are to there if a change is desirable?

Another major issue on which I would have liked to see more discussion concerns the role of budget deficits. My impression is that D/F have over-reacted to what they see as excessively simplistic views from our most conservative research institute. Their rhetorical flourish about "frightening the bourgeoisie" is no substitute for hard analysis. D/F do point out at one stage that "The data immediately suggest the question of whether budget deficits and money growth, and in particular the latter, are the main culprits in the inflation story". However, I would like to see some tighter reasoning and a clearer answer to the policy questions about sustainable budget deficits. D/F should be well placed to point the way on this subject since the American

economy is moving into what someone recently called "uncharted territory". This commentator pointed out that never before had the American economy had both a strong recovery in private spending and budget deficits of around 5 per cent of GDP. My question is:

8. What is the appropriate framework for analysing the relationships between budget and the economy?

I note in passing that D/F assume in one part of their analysis that money stock is held constant when a budget deficit is increased. This may be just an analytic convenience but I suspect it is, in part, based on a view that monetary and fiscal policy are more independent than I believe they are. However, it might be useful if D/F spelt out more precisely their views on this issue in a non-polemical context. The question, of course, was probably asked many years ago by Carl Christ but it bears repeating since nobody seems sure about the answer:

9. What are the consequences for rates of interest and for the exchange rate of holding to a monetary objective when a budget deficit is expanded by X per cent? (The authors will need to specify other preconditions but I would like to see the question answered in the context of a rapidly expanding private sector.)

A relevant footnote by D/F (on page 70 of the original version of the paper) is formally correct; but does not help much. It says:

"But there should be no automatic link between the deficit and money growth - and besides, deficits at times of recession are typically associated with high, not low interest rates."

A final issue is completely general. For if we return to the opening paragraph of D/F's paper where they note that, in common with the rest of the OECD countries, Australia has suffered a marked deterioration in its economic performance since the early

1970's. As they point out, the deterioration in Australian performance was somewhat greater than average. Indeed, if a longer span of history is considered this point becomes much stronger. At the turn of the century Australia's income per head was among the highest in the world. During this century Australian real income per capita has grown on average faster than in the UK, a bit slower than in the USA and much slower than in Japan, Germany, Switzerland and (more recently) many of the so-called "newly-industrialising countries" of South-East Asia. By 1980 Australian income per head ranked about fifteenth amongst OECD countries. On some measures our income per head is not much more than that of Japan and, if recent trends persist, we will be passed by Singapore within the next ten years. My final question is:

10. Why has Australia's relative economic performance been so disappointing?

**REPLY BY PROFESSOR DORNBUSCH:**

I have had the advantage of two very thoughtful and generous discussants, one every eclectic and the other less so. What I want to do is put in perspective what we thought our assignment was and then take up some of the specific criticisms that have been raised. When we wrote the paper the concept was that it was mostly for consumption outside Australia, and that whatever comments we are getting here are for our own betterment. Writing in that way inevitably means that we will have to strike some balance between saying what is generally agreed here, and therefore boring, and being specific and, with that, controversial.

I think we have succeeded on the controversial side, and what I want to do is try and make that survive. We begin by briefly reviewing how Australia compares to the OECD, and note that Australia is slightly below average over long periods. We then ask in what particular ways does Australia stand out. There are two issues. One is on the composition of output and trade with large on going structural change. The other is whether the Australian wage-price institutions are special; and, in particular, whether the large real wage gains in the early 1970s must be assigned a special role. In policy discussions, if one comes to where we are now, on one side are the questions of inflation and unemployment. On the other side, matching those questions, are the role of the budget and the role of the real wage. In the discussion these stood out, so what I want to do next is look at some of the specific issues that have been raised.

I'll start with the point that Adrian Pagan raises. The first question is on the general model that we chose. We disaggregate output to separate out the agricultural and mining sector on the one hand and the manufacturing and service sectors on the other. As we try to emphasise, we think of the agricultural and mining sectors as something nearly neo-classical on the production side, and very much world market linked on the demand side. Hence we chose to write down equations coming out completely that way. I want to emphasise that the manufacturing and service sectors are different in their pricing behaviour and in having more non-traded aspects. We go all the way there in

having mark-up pricing. The structure we have then has Keynesian features for manufacturing and services and a very neo-classical sector elsewhere. Because these sectors are so different we want to make the point that in any macro model these should be distinguished, both because of the employment-pricing differences and because shocks will affect them differently.

In our model of the financial markets we introduce the return on capital and Adrian Pagan has raised the question whether investment should not be determined by that. Furthermore he says that, to the extent that the real wage affects investment, we might have to be sceptical about the impact of real wage increases upon output and employment. Particularly he says that there is a significant dampening effect to an increase in government spending. Let me just put up a graph that summarises his argument which, I think, has a bearing on some of the questions that Peter Jonson raises. From the factor markets we have links between the real wage and output; the higher output and employment, the lower the real wage. Now Adrian finds from his econometric work that there is a negative link in the goods market between output and the real wage, since a rise in the real wage, while it promotes consumption, has an adverse effect on investment that dominates. Consider now an increase in real government spending shifting the aggregate demand out at a constant real wage. We would then get the standard multiplier effect. Adrian points out that, because the real wage would rise due to the gains in employment, there is a dampening effect. This raises a number of empirical questions. One is how strong is the output-real wage link? Is it a very steep or flat

schedule? If it was totally flat we'd be back to where we started. If it is very steep then the real wage dampening effects are potentially important. The other question is how big is the difference between the consumption and investment responses to real wage rises.

Well, I think these are empirical issues, but I am not persuaded that the two were true at the same time - a very steep real wage employment link on one side and, at the same time a significantly adverse effect of the real wage on aggregate demand coming quickly. Certainly these important questions should be raised, particularly because we haven't specified the equations relating to the profitability of capital, but they are more issues of extension than of error.

Adrian Pagan raises two other questions about our model. One is whether it is appropriate to have a fixed mark up assumption, or whether we should have made the manufacturing/service sector more neo-classical, allowing for import competing effects. The other relates to confidence effects. On the first question I really have no quarrel with him while, on the confidence effects, I think I share with him an aversion to that. But I will come back to the last.

A lot of the comments that Adrian presented centred on the question of the Phillips curve. We thought it would be very important in a paper on Australia read by outsiders to ask whether Australia has a Phillips curve or not, because that is one of the key building blocks in any macro model. What is crucial is whether there is a stable link between unemployment

and wage inflation. The straw man we set up was to estimate a standard Phillips curve and we found it was necessary to include a time trend. That means to predict wage inflation you need something else beyond unemployment and a forecast of expected inflation. And that something else of course takes us immediately into the unending Australian literature on what it might be. We take issue with the Gregory-Smith formulation - a successful one - on two grounds. One is the theoretical rationale based on implicit contracts. We really don't believe that implicit contracts mean you pay a wage independent of the state of unemployment. The wage that firms do pay to the workers they keep cannot be completely oblivious of the fact that identical people are outside and unemployed. There should be some link to labour market conditions. The second is that, contrary to Gregory and Smith, we find post 1978 unemployment to be a determinant of wage growth. Empirically we cannot reject the unemployment variable. We go from there to saying we don't believe that the impact of overtime is really the answer or, more carefully, the full answer. It is here that we introduce the real wage material following the discussion that has been run in Britain, by just including the variable we call real wage resistance. It is defined as the previous peak real wage relative to the current real wage lagged one period (less unity).

We need to say something about data. We were very worried that, because we cannot do an infinite number of regressions and at the same time read all the papers that have been written by everybody else, we would be getting endless comments on not

allowing for part time work and things like that. Therefore we tried to use variables that have little to do with part-time work and are not affected by changes in the composition of the workforce. That's why we don't use average weekly earnings but male wages in manufacturing. I'm afraid I can't say whether the series represents awards or earnings. The printout I looked at merely says wages in manufacturing, but I will certainly follow up on that. So we chose that particular series in order to have the minimum of problems with composition of the labour force, and with part-time effects.

Now I have looked at our print out and the print out does not suggest a constant real wage variable until 1979, but there is a lot of action in the 1960s, so I do believe that Adrian's suggestion that we are implicitly introducing a post 1979 dummy may be right. But I cannot say what happens in the Gregory/Smith regression with average weekly earnings as I don't have it with me. So where do we stand on the Phillips curve? I do think that the real wage variable works very well in the way we have introduced it. It does not seem to be just a post-1979 dummy. Nevertheless, it would be worth seeing whether it survives once overtime is added and all the things that can be done to the Phillips curve are done to it.

The second area where Adrian Pagan takes issue with us is on the real wage. Let me briefly sketch the argument that we make. Suppose we have the real wage and suppose we have the aggregate demand schedule. We could look at the schedule XX, along which the output firms are willing to supply is equal to the level of demand. If demand increases the real wage has to fall in order

to make the firms willing to produce more. Along that schedule firms are happy to supply the amount of output that is demanded. Suppose here is full employment. We are going to look at two kinds of points: those on the schedule and those to the left. Points to the left we call Keynesian unemployment and points on the XX schedule classical unemployment. Suppose the real wage is  $w^*$  for the economy to be fully employed and for firms to be happy to produce the output that is being demanded. Suppose the real wage rise is to  $w'$ . If the economy is at a point like A then there is Keynesian unemployment. Simply expanding demand could get us all the way to B and remove some of the unemployment before we come to supply constraints. If we are at B further increases in aggregate demand will not do anything. It will lead to excess demand as firms are not willing to supply more. One of the questions we try to raise, and to which we don't have a definite answer, is whether the economy is at a point like A, whereupon demand expansion would provide more employment, or is it at point B? We try to present some evidence on this. Here, of course, the parallel with Europe is completely clear. Exactly the same question is being asked there - whether the unemployment is entirely due to excessively high real wages, in which case firms are simply not willing to meet any increase in demand and it would simply be purely inflationary to increase demand, or whether, on the contrary, an increase in aggregate demand will go some way to remove unemployment, even if not fully. Where we come out is to sit on the fence and to say there is some Keynesian unemployment clearly, but when you look back over the

1970s you surely can't ignore the fact that there has been growth in real wages relative to productivity, and that, as a consequence, some part of the 10% unemployment is certainly classical. To go back to 1.7% unemployment as a measured full employment rate, you would have to have real wage cutting. But there is no known test of what is classical and what is Keynesian unemployment, and this is equally true for Japan and Germany where exactly the same arguments exist. So, not surprisingly, we also don't say that, of the 10% unemployment, 3.5% is Keynesian and 6% is classical.

The last issues are the questions of monetary policy in the context of flexible exchange rates and the assignment problem, and that takes me much more to Peter Jonson's comments. Peter Jonson looks at our paper in two ways. He gives us top grades for all the points to which he is inclined to agree from his previous work, and he takes as unresolved all the issues where we don't agree with what he says. It turns out we only got one top grade and we had ten questions. We left some blank and the others we answered falsely, so I am at a bit of a loss as to how to reply to all of these. One is an easy confession. We really haven't spent time analysing the inflation expectations formation mechanism. What we do in our paper is simply place in our Phillips curve an inflation forecast that is only a distributed lag of actual inflation. To get to the Gregory measure we add 4% to this series because we didn't have his data and we measured the discrepancy from his diagrams. Generally, however, there is no treatment in our paper of the inflationary expectations mechanism.

On the Arbitration Commission Adrian Pagan has summarised our attitude right. We do believe that it does add its own noise and we do believe that, on occasion, it gets out of line with the fundamentals of the Phillips curve. In that sense it could serve as a dummy variable. Exactly how to disentangle the effect I am not certain. Because Adrian Pagan's diagram didn't have a scale, I couldn't figure out exactly how to think about it.

The one issue where I very much disagree with Peter Jonson, and to some extent with the reading I have done on Australia, is in the area of fiscal policy. Since 1979 there has certainly been an enormous amount of writing that says that expansionary fiscal policy is in the nature of a boomerang: it really doesn't work. The way that literature is written, certainly coming from the Treasury, is to say: of course there is a direct multiplier effect and we know it exists. Then in the next two pages they show that other effects are also there - confidence effects through various channels, monetisation effects, exchange rate effects - and when they are all summed together fiscal expansion turns out to be a bad idea. Well, we have an example of fiscal expansion in the United States at the moment. We have a monetary policy that, adjusting for institutional change, is tight, and we have an extremely expansionary fiscal policy. What we observe is very dramatically declining inflation, output growth and excessive exchange rate appreciation. We observe exactly the opposite of what is being argued in Australia.

I think the US experience is really worth looking at because it says that what the Treasury has been arguing here surely

cannot be right. Peter Jonson accuses us of not doing hard work. I think that the first piece of hard work to be done is to look for actual examples, and the actual examples on tight money and easy fiscal policy are very inconsistent with his interpretation. The big risk with such a policy is exchange appreciation and overvaluation, certainly not the opposite in the initial phases. He asks what would be the guide lines for monetary policy once an economy is on a flexible exchange rate? There I think we do have very specific ideas. First, on monetary policy, one needs to get away from strict targets to at least adjust for velocity change. The second is to recognise that the exchange rate can get away from purchasing power parity for very extended periods, as it is totally dominated by the capital account. Under an expansionary fiscal policy, with relatively tight money, exchange appreciation is the rule that helps stabilise and even disinflate. This is a very tricky thing because you build up overvaluation. So it is very important that when you do have a flexible exchange rate, not only to make monetary policy inflation adjusted (and that means perhaps using nominal income targets) but at the same time to have an extremely strong incomes policy. Without the two the flexible exchange rate turns out to be an immense policy problem.

I want to make one final remark that pertains to Peter Jonson's comments endorsing deregulation in labour markets and financial markets. I think there is experience with comprehensive deregulation of an economy in the case of Chile. If you look at Chile they have done everything you could possibly do to deregulate the economy, and you couldn't possibly think of

an economy that has gotten further away from equilibrium in the course of action. That surely does not suggest that not deregulating is the course that you should follow, but it also means that deregulation is an immensely tricky business. Making the exchange rate flexible when all other wages and prices have a large institutional component is very, very risky, and if you cannot quickly get everything else flexible - and you certainly can't - then you need a significant incomes policy to make the flexible exchange rate work.

I haven't really answered all the issues that are raised, but perhaps in the following discussion I can take up some more. I am grateful to both discussants for the time and effort they have spent and might take this opportunity to summarise our general views. One, on the Phillips curve literature we believe that the answer from overtime is not the right one or the full one. Second, on the real wage, that ruling out a good part of classical unemployment is certainly the way. Third, on fiscal policy, here in Australia there has been an immense exaggeration of the harm that fiscal policy can do, considering that the country has had an inflation adjusted budget surplus for twenty years and a debt/income ratio that is vanishing. Finally on the flexible exchange rate you certainly have an exciting experience ahead.

#### **DISCUSSION FROM THE FLOOR**

**Professor Wolfgang Kasper, University of NSW**

My comment is aimed at cautioning our visitors from the US against transferring implicit assumptions about how the economy

responds from the US to the Australian scene. In contrast to the US economy, where there has been deregulation, where there is rapid innovation and active market competition, also in the labour market, additional demand in Australia is met rather inflexibly by the production/supply system. More demand in Australia is met with relatively large price and wage increases, because the Australian economy is rather rigid. This is so partly because international and national competition is confined by much regulation, partly because the administered wage-fixing system freezes real wage levels in the ways which Professor Mitchell describes in his paper. I suspect that the inelasticity of supply has increased in Australia over the past ten to fifteen years so that econometric estimates of past quantity and price reactions to demand expansion underestimate the price-level effects - and overestimate the quantity and employment effects. In any event, I believe that Professor Dornbusch's recipe for employment creation by simple, paleo-Keynesian fiscal and monetary expansion would not work in Australia, if we do not first deregulate factor and product markets and break inflationary expectations.

**Dr David Morgan, Treasury**

Let me associate myself briefly with some of the views of the discussants and also with those of Wolfgang Kasper. I too, think the paper did over react to excessively simplistic views, although I don't believe that they are ones held by Treasury. I think the emphasis which Peter Jonson placed on sustainable budget deficits and sustainable recovery in output is the issue,

not the short run issue - which is not in dispute. I also strongly concur with his view that Dornbusch and Fischer - at least in the Australian context - are assuming that the money stock and budget deficits are more independent than they in fact are.

Let me briefly suggest that the caricature of the Treasury position is a straw man, at least as put up in the first full paragraph on page 67, where it is suggested that the transmission mechanism is simply that an increase in the budget deficit will lead to an increase in the actual and expected rate of inflation and produce crowding out in the private sector more than one for one. Statement No. 2 in the 1983-84 Budget has a very extended discussion of the transmission mechanisms that the Treasury sees between fiscal policy and private sector spending and I must say that the paragraph quoted above is a grotesque caricature of that position. An increase in actual and expected inflation is seen as one of the transmission mechanisms; so too is the transmission by interest rates. How you rule that out in that paragraph surprises me. So too is the effect, which is more potent in some circumstances, and which we refer to, via exchange rate appreciation. I was perplexed by your comment that the big risk is exchange rate appreciation and not the opposite "as asserted". We didn't question the direction of that effect: that exchange rate appreciation is a very potent risk in the current circumstances. One last part of the misleading Dornbusch-Fischer caricature, which Adrian Pagan has pointed out is the transmission mechanism from higher inflation to depressed investment does not come about primarily by the interaction of

inflation and an unindexed corporate tax system.

Wolfgang Kasper anticipated what I would see as the major distinction in drawing lessons between the American and the Australian experiences. But there were also some rather different prior conditions operating around 1983 between the US and Australia. These included deeply entrenched inflation and inflationary expectations, including actual inflation almost twice that of our major trading partners, a deeply depressed profit share, high nominal and real interest rates, a complete lack of any credibility in monetary policy buttressed by five years of over-runs and great uncertainty in the economy - including as to the macroeconomic policy approach. I would be interested in your views as to whether you see those prior conditions making any difference at all to the efficacy of an expansionary fiscal policy.

A couple of questions in conclusion. First, what is the relevance of the observation at the top of page 69, that the OECD data show that Australia has had smaller deficits by international standards? Secondly, I would invite you to extend the final column of your Table 13. You said the debt problem is vanishing in Australia. It was noted that there was a severe decline in the real value of debt up until June 1982. The data since then show a dramatic turnaround. Finally, I would invite others here, including Dr FitzGerald - who has done the calculations - to comment on the assertion on page 70 that "there is no case for believing that the 1983-84 projected 4.7% of GDP

deficit is there for long". If I recall the scenarios produced for the Summit, they showed that there was indeed a very significant structural component to that deficit. I would also stress that in making those projections at a time of high inflation, one has to take care not to assume an unchanged nominal personal tax rate structure, because it gives a completely phoney impression of the automatic reduction in the deficit.

**Dr Neville Norman, University of Melbourne**

Everyone knows I am an agent of the Treasury, so let me briefly defend them. I think it is particularly unfortunate that this point - that the discussion of the deficit is overdone - which I think has a lot of substance, should be tied to the Treasury. The Treasury is a particularly difficult animal on which to pin anything. The comments that issue from it are either comments of an individual, not to be attributed to the organisation, or comments of an organisation that do not entirely go to advocacy. If you do want a reference perhaps Statement No. 2 to the 1977 Budget Speech is the closest you will get to evidencing this, but I think the Treasury has been much more careful in recent days. I would add while we are on this topic, that the Treasury has shown an awareness of the important difference between wages and labour costs created by on-costs in its Information Paper of two to three months ago. It is a point that - with respect - I think is missed in the Dornbusch-Fischer paper and not surprisingly, missed by most Australian economists. It is picked up, somewhat obliquely, on page 15 of Adrian Pagan's paper and no doubt that will be discussed in the labour market

paper. Secondly, one of the hottest issues in the Australian debates for 20 or 30 years is the modelling of the price equation. I would totally agree with Adrian Pagan that it is much better and not analytically costly to incorporate the endogeneity of the mark-up. But if you are a single-cause-explanation person, I would side with these authors in picking costs, rather than the traditional Australian literature in picking import prices. I suggest that this is a point we should bear in mind when we come to discuss the trade paper tomorrow, because the standard international trade theory normally postulated is implicitly being challenged by the Dornbusch-Fischer model.

**Professor Dornbusch:**

We actually hadn't meant to be controversial in taking issue with the fiscal policy literature. My reading of an essay by Mr Stone and of page 57 in the Budget Statement 1983-84 - which turn out to be virtually identical - is that somewhere there must be the firm belief that there is strong and perhaps dominating, adverse effects. I read one paragraph on the multiplier and a page and a half on the adverse effects. Now, the evidence we have seen on tight money and expansionary fiscal policy is that clearly it is not PPP expectations dominating the exchange rate, and expansion leading to cost and price increases and therefore to expected depreciation, and therefore to more inflation. On the contrary we get very, very, sharp deviations from purchasing power parity as the exchange rate appreciates because the country that goes for a high interest rate policy for quite a while is a good place to put your money - the capital account effect is

very, very strongly dominating. So the basic story that fiscal expansion will re-ignite inflation and with that produce a lot of adverse effects, can be exactly opposite under a flexible exchange rate. I take the US example because the real appreciation and the disinflation has been so phenomenal. If you go back to 1980 initial conditions were very much like those in Australia: complete lack of credibility, high inflation - that was expected to be high core inflation - any of the things you can look at, except for one thing: that the US had a longer history of larger deficits than Australia. Now the comparison of the Australian deficit numbers with the OECD numbers is relevant because, for example, if you look at the US they have had disinflation, and then recovery with much larger deficits and deficits that are expected to grow over the next ten years. I looked at scenario 2 as revised on December 2nd and it does not show persistence of the deficit. It shows that the nominal deficit comes down in an economy that is expanding in nominal terms at 10% so I wonder whether there are very large, long term structural deficits. If you add to this the possibility of a resources boom taking place, I really can't see why everyone has to be so worried about unsustainable deficits. Now the literature in the US context on sustainable deficits is important: it says that ultimately you cannot run deficits at 5% of GNP, but I cannot really see how that is a danger in the Australian context. There is nothing in the current fiscal programmes that persuades me that they are persistently deficit oriented. The debt is extremely low so that the risk of real interest rate increases leading to explosive debt service that deteriorates the

budget, is really not an issue. If you look at major OECD countries I think among all of them Australia may be the best placed to try a few fiscal escapades before looking like Denmark. I think that is why the comparison is worthwhile. I am sensitive to the issue because in advising the European community there is a lot of fiscal conservatism and we spend a lot of time identifying exactly who should be tight - and Denmark and Italy certainly should be - but if you look at Australia the story is exactly the opposite. Because that is so striking an international comparison we spent time on it and we were surprised that the view we give here is not one that was more widely shared. This is the reason why we have given it prominence. I guess we have not gone very far in persuading anyone but we hope that at least the discussion is lively.

Now, on the aggregate supply curve, which is very different here from the US. I really will not pursue it because in all the papers I have read I have seen no evidence that the move to 10% unemployment means that the natural rate of unemployment has risen to 10%, and that any expansion is inflationary. I think that is a myth. I am not aware of any empirical evidence - of course I have only read 50 papers - but I have not seen such evidence. I think it is an extremely important issue to document that fact if it was the case in Australia, I am not persuaded of it. If it is the case then it just reinforces what I said about incomes policy. It does not make a case against the use of fiscal policy: it makes the case for incomes policy.

**Professor John Nevile, University of New South Wales:**

I will be very brief. I know you asked for people who want to challenge the views put forward in the paper but I cannot let it appear as if nobody in Australia is in favour of expansionary fiscal policy under any circumstance. I think I can make my point by commenting on the comments. Wolfgang Kasper's arguments that expansionary fiscal policy would lead to increased prices and not to increased output, seem to me apply equally to any expansionary impulse in the Australian economy and you then have to explain how we ever get any real expansion at all. The picture sketched in Wolfgang Kasper's remarks seems to me to be a rather non-sensical situation. David Morgan argued against expansionary fiscal policy in part, as I understood it, by saying if you get a large structural deficit, if you're silly enough to let yourself have a large structural deficit, then you are always stuck with it because it is politically impossible to reduce it. We changed a structural deficit into a large structural surplus in the three years ending in 1981-82, and showed that it was politically possible to do this in Australia.

My final point is that there seems to be an assumption in Australia that it is very dangerous - very risky - for the government to borrow to finance productive investment, but presumably it is all right for the private sector to do this. Why the difference?

**Professor Max Corden, ANU**

A brief word on the deficit. I think most people, other than the Treasury, would agree that at a time of severe recession

there is a justification for some short term fiscal expansion. I will make a more general point. Although Treasury has used some pretty weak arguments over the years, there is a good argument - simply the good house-keeping sort of argument - that once you get a deficit it gets bigger and bigger and our politicians can't be trusted so we end up like Denmark, not to speak of Brazil. You could take the view that we have had a basically good history because of all these sorts of arguments that the Treasury have used. Some of these arguments are not very good but they have had the net result that we have not got a big deficit. The fact that we are better on this score than the average OECD country simply tells me: "let us enjoy our sunshine; let's be glad that at least this is a problem we have not got". When you look at some European countries - not to mention some of the developing countries - we should be deeply grateful that we haven't got ourselves on this particular road. Sweden is another country which has got into this difficulty. However it doesn't alter the fact that there is a case, it seems to me, for short term fiscal expansion at a time of severe recession such as we have recently experienced.

Now I would like to add something on the central issue: is our unemployment Keynesian or classical? Wolfgang Kasper assumes that it is classical. Some people imply that it is mostly Keynesian. This is really a central issue of economic policy in Australia. Let me just take the famous 1974-75 episode. Nominal wages increased and nominal demand did not expand much, or even contracted; the net result was that real demand fell, so that we have a Keynesian explanation of unemployment. But the real wage

also rose - so we have a classical explanation. That is looking at the history, not at what the policy should have been from 1974-75 on, but how did it all come about? Do we describe the episode as Keynesian or as classical? We have to ask ourselves what the motives were for the nominal wage increase. Did the unions intend to get that extent of real wage increase or did they get it by accident? If they got it by accident we have to say that some part of that unemployment actually was Keynesian because it wasn't intended; it came about as a result of false expectations about prices, and so on. So we had to analyse the origins of the wages explosion.

But more important is the policy issue once you've got there. Let's imagine that we are in 1975 and we have had a big real wage increase associated with the fall of real demand and a big rise in unemployment. Now if it is Keynesian unemployment then presumably a further expansion of nominal demand - let me stress that all governments can do finally is expand nominal demand - would increase employment with a constant nominal wage. But if the unions have actually decided at that point in 1975 that now they must insist on indexation, i.e. that they will not allow the real wage to fall under any circumstance, I think it is reasonable to say that from then on the new level of unemployment is more or less classical, even though its origin may be partly Keynesian, or sort of accidental.

I feel this doesn't come out very well in the diagram that Rudi Dornbusch has used. To digress a moment, I think it is much better to have a diagram where you show the product price on one

axis and 'y' on the other axis. One shows the firms' supply curve, and a nominal wage increase shifts the supply curve. One focuses then on product prices, on nominal demand (an ordinary demand curve) and on nominal wages, because it is nominal things that can be acted upon. The unions can't decide on real wages in the short run. If there is Keynesian unemployment that must mean that firms have kept prices up artificially. That is to say, if they lowered the prices they charge, then for given nominal demand and given nominal wages, they would get more profit, so that it implies disequilibrium in the product market. Those are the conditions for Keynesian unemployment.

Considering the Australian case, I think it is highly likely that there was disequilibrium in the product market in 1975 and again in 1982 - in other words the years of recession. It seems to me also highly likely that by 1977/78-1979/80 firms had pretty well adjusted their prices to where they wanted them to be. After all there are no rigidities in our prices, we are living in a general state of inflation where people are continually adjusting prices. Why should firms not be in rough equilibrium? Why shouldn't the product market be in rough equilibrium (not day by day of course)? Therefore I would submit that for the period 1976 to the next period of trouble - mid-1981 - we were more or less in a product market equilibrium and the unemployment was classical. From which I conclude that the unemployment rate that we had during that period of 5.5% to 6% was our natural rate. Our natural rate was set by the unions - implicitly of course. Not that they like that rate of unemployment, but they wanted a real wage which yielded that unemployment. Then in 1982 we again had a

Keynesian episode, and now we are still in that episode. But we are moving into a new equilibrium situation. Presumably we will know by 1985 what our classical unemployment rate is. I am guessing that it has probably leapt up to around 8%, but we can't tell yet.

**Mr John Langmore, Office of the Minister for  
Employment and Industrial Relations:**

I would like to reinforce Peter Jonson's final point about Australia's relatively deteriorating economic performance. The paper begins by emphasising the similarities between Australia and OECD experience. However it could equally well begin by emphasising the differences between the Australian and OECD experience which are shown clearly in Table 1 when you look at the growth rate of GDP per employee. In the period 1960-73 the Australian annual growth rate was 2.4% and the OECD annual average growth rate was 3.9% - a more than 50% difference. I think there is a very important question worthy of a great deal of study and discussion as to why, over that period which ended with the first resource boom in Australia, was the Australian growth rate so slow? It could be that it is linked with other differences between the Australian economy and other OECD countries, such as the relatively small proportion of trade in the Australian economy compared with OECD countries; the prolonged government surplus which has been emphasised; or perhaps even the relatively small size of the public sector. No doubt there are a score of other factors that are equally important, but those differences deserve some very careful discussion.

**Dr Neil Johnston, Treasury**

I would like to add a brief comment on the efficacy of stimulatory fiscal policy. As some of the discussion has already emphasised, so much of that efficacy depends on the credibility of policy. In particular, looking at the experience in the US over the last couple of years the conventional belief has been that the Federal Reserve would maintain a tight monetary policy stance notwithstanding the stimulatory stance of fiscal policy. If I go back to Neville Norman's earlier comment, I am not sure that you can tie down a single Treasury view at any point in time. Certainly if you surveyed the Treasury people that are here today you would find a range of view points. However, if refer to Budget Statement No 2 over the last five to seven years you will find that one of the underlying concerns throughout that period was a doubt as to whether you could put in place stimulatory fiscal policy in conjunction with a disciplined monetary policy. The major distinction that you need to make here between the Australian and US experience is that in the US you have a degree of independence between the Federal Reserve and the government, whereas here to a large extent the stance of monetary policy is determined by the government of the day and it is a much different environment because of that.

**Professor Michael Porter, Monash University:**

I really wish to comment on the fact that in hyper inflations we observe real money stocks tending to vanish but this does not, of course, mean that there is a shortage of money, quite the contrary. So too if the real bond stock happens

to be vanishing that could mean that there is too much debt. I am not quite sure what the evidence has to do with the point. I presume Rudi Dornbusch does not really want to associate with that sort of evidence.

I would like to ask a more substantive question essentially about the relaxed attitude of Fischer and Dornbusch to the Australian wage regulatory mechanism and link that to what is behind Treasury's view, and the view of many of us, that where the wage regulatory structure is wildly out of line with economic rationality there is a very great danger that there will be perpetual attempts to use government stimulus to do what cannot really be done other than by allowing competition in labour markets. I think Wolfgang Kasper referred to output supply elasticity. I think coming from the United States one can have much more confidence in the Keynesian policies precisely because relative prices can adjust all over rather more than in this country. Your academic institutions aren't blessed with the uniform wage structure, nor are your managerial institutions. Your bureaucracy is, and that explains quite a bit. However it does seem to me that Professor Dornbusch from Chicago would be somewhat fascinated with the views of the new Dornbusch if you like, regarding the virtues of a wage system which from Grand Central lays down everybody's wage. It seems to me that the reason some of us are scared of growing deficits and growing government, is that if we are not allowed to re-allocate labour according to comparative advantage, and if we don't have a mechanism for adjusting, then we are in trouble. New Zealand is the most obvious case: all relative prices there appear to be

fixed and the only thing that varies is the number of people travelling across the Tasman - and the direction in which they travel.

**Mr John Macleod, CRA:**

We have been given a very interesting outside view of the Australian economy. I think it is of particular interest to note those aspects which have been highlighted when looking at our long record of not being competitive.

I was disappointed that this paper and others, tended to look at resource booms or external shocks in the form of large increases in export income, rather than the opposite. I would like to support other speakers in asking the authors why they did this. Peter Jonson for example, mentioned the way in which Australia had slipped in comparison to the rest of the world and if you look at our export performance it is quite evident that we have performed very poorly. In particular we have performed poorly in the manufacturing export sector - I don't mean in comparison with the giants or the NICs - but compared to countries with similar economies. I wonder why they chose to look at booms rather than at our shocking performance.

Secondly, I would like to make a plea to the authors, that when they are talking to or writing for outsiders, that the economic shorthand of real wages is defined somewhere, so that people understand what is being talked about. Neville Norman raised this point but let me just say that my experience is that Australians often mean different things when they talk about real wages. If you are an employee you mean an after-tax income

plus a set of conditions laid down in an award - conditions which may or may not have a monetary benefit. If you look at the Accord in particular and at the things that the unions have put in there, you will see what I mean by non-monetary factors. Any ordinary wage agreement registered with the Arbitration Commission lists hundreds of these issues which apparently are important to employees. If you look at real wages from the point of view of the employer you get a very different picture. This is in fact a gross wage plus the on-costs and those on-costs are not typical of the rest of the world. Workers' compensation, for example, is very different. Superannuation, payroll taxes etc. are the same. But I do think a distinction ought to be made here, rather than assuming that there is some mythical thing called a 'real wage', whether it be average weekly earnings or something else.

**Professor Rudi Dornbusch:**

I want to take up a few specific points. First, on Max Corden's exposition. I would fully endorse what he said and perhaps differ with Adrian Pagan - we thought that there was some classical unemployment. If you tell us that 3.5% of the unemployment is classical, that is the actual difference between 4.7 and 1.2%, we certainly would agree that is a sensible number. I am not quite sure that I agree on the distinction between origins and the aftermath of a real wage increase. What matters is, once the real wage has increased (whether by luck or by accident) the question is "do they want to keep it?". What we find is that the real wage gains do show up in a sticky way - whatever the reasons for the gain. Once they are there they do

appear to influence unemployment and wage inflation, therefore it is an important issue. There is some indication we feel, that perhaps a third of the unemployment is classical.

Turning to the issues of credibility and fiscal policy raised by Neil Johnson. If you had followed the US discussions on monetary policy you would not be persuaded that the Federal Reserve had established itself as an independent monetary authority. For one thing money has grown far more than the targets, and the extent to which the Federal Reserve has used changes in the definition of money to obfuscate highly accommodating monetary policy between last August and now, would make any one doubt. What it really suggests is that the credibility issue is quite a bit overvalued. I think the US experience shows that you can have murderous fiscal expansion; you can print a lot of money - as long as people believe that the monetary expansion is not indefinitely out of line and the fiscal expansion is there to stay. This is what has been happening in the US economy.

The very intriguing question which I will try to answer, even if that gets me into trouble, concerns the difference in productivity growth between Australia on the one hand and Japan and the European countries on the other which account for the high OECD growth. I think one answer may be that the high growth European countries and Japan have used a systematic exchange rate undervaluation to get export led growth throughout the 1960s. By the end of the 60s the US could not do that for them any more. Australia in that accounting may well have been like Britain - as

it is in many other respects - and did not have the high productivity growth, and may well have gone further in the direction of Britain.

Michael Porter asked about hyper inflation and the vanishing of the real debt. I raise the real debt question as a technical issue. One of the big problems with an expansionary fiscal policy is that it increases the size of debt relative to normal tax revenues. Then, when by some accident the real interest rate increases, debt service costs raise the deficit so much and causes the debt to grow so rapidly as to be difficult to control. That is the question of sustainability. When the debt ratio is very small it is not an issue - that is why the comparison with other OECD countries is important. When the debt/income ratio is relatively small - 20% to 30% - you really don't have to worry that a real interest rate shock will throw the budget into unsustainable deficits. Therefore you really don't have to worry about restoring the budget deficit to zero within two or three years, you can afford five to six years. Ultimately of course you can't run 5% deficits, but how long you can run them depends on how much of an increase in the debt/income ratio you think you can afford before the possibility of interest rate shocks makes that debt ratio very risky. In Australia's case I think the initial debt position is such that it is really not a stricture on fiscal expansion.

Lastly, on the regulation of labour markets. If you look at the US labour market, the striking fact is the extent to which relative wages are fixed. Even though there are a lot of

independent unions bargaining, relative wage patterns are very firm. Only when the real exchange rate became quite vastly overvalued, giving rise to immense sectoral problems, from airlines to steel to automobiles, did the relative wage structure - in the last two years - get to be flexible. So until 1980 I don't think there was a big difference between what regulation achieved here and what the unions did in the US but that view may get me into trouble with US labour experts present.