

Chapter 4

Academic criticisms of the CEPG analysis

The work of the Cambridge Economic Policy Group over ten years has been conditioned by one overriding objective – to characterise the increasingly serious predicament of the British economy and to assess the strategic options for policy. In order to carry out this programme we have developed a computer model which gives reasonably good conditional ‘forecasts’ of the past and which can simulate future courses for the economy assuming alternative policies and other exogenous factors such as world trade.

The simulation model, although an essential tool for our own thinking, has by itself very little expository power, and we have therefore tried to set out its main analytic properties in a number of articles.* We have also published some econometric studies to establish as well as possible the model’s main empirical foundations.

Unfortunately our publications have fallen well short of a comprehensive statement of our views, their relationship to those of other economists, and their grounding in evidence. But because parts of the model have interested some economists and, perhaps, because our analysis and policy recommendations have been influential in public discussion, there now exists quite a substantial academic literature on our work.

It is impossible in the space of a few pages to argue through all the points that have been raised. We are certainly ourselves to blame for some confusions and misunderstandings. Our views have often emerged in a disorderly way (usually in the service of intervening, under a time constraint, in the public discussion of policy issues). There is no satisfactory solution other than to write, in due course, our own textbook. But meanwhile we have some obligation to discuss the more important comments that have been made. Those we have chosen to deal with here fall into three categories, relating to the effects of protection, to wage and price determination and, finally, to ‘New Cambridge’, of which accounts (sometimes highly garbled) are beginning to creep into textbooks.

Protection as part of a macroeconomic strategy

As the policy of protection in one form or another gains increasing support in the UK, the number of papers on the subject by economists has tended to accelerate. Perhaps the most noteworthy contributions

* For instance Cripps and Godley (1976) and Fetherston and Godley (1978).

to the debate during the past year have been a paper by Maurice Scott (1980), an exchange between Robert Neild (1979) and Deepak Lal (1979) and a recent survey of alternative macroeconomic strategies (Allsop and Joshi, 1980).

This debate is often much confused because the alternatives under scrutiny are not systematically distinguished. There is one crucially important question, namely whether a country in fundamental payments disequilibrium is better off with protection than without it. There is another, quite separate, question of whether a deficit country can regain internal and external equilibrium better through devaluation than through protection.

We shall examine the main issues under these two headings.

Deflation versus protection

Our starting point when attempting to think out macroeconomic strategies for Britain is the strong belief that, given the real rate of exchange for sterling which existed on average in the 1970s and given current arrangements for international trade, a serious and growing depression will develop throughout the 1980s which could reach catastrophic proportions towards the end of the decade when North Sea oil production declines. The reason for taking this view, which we have reiterated for years and which no one is really prepared to challenge explicitly, is that trends in Britain’s foreign trade in manufactured goods have been extremely adverse and that there are no good grounds for supposing that they will change spontaneously. According to calculations we made before the recent perverse strengthening of the exchange rate, continuation of past trends would have resulted in stagnation throughout the 1980s and unemployment rising to 4 or 5 million by the end of the decade.

It seems to us foolish and dangerous to sidetrack this main issue, which involves, first and foremost, recognition of the alarming predicament facing Britain, by confining the discussion to whether protection or devaluation is the better way of recovering and retaining full employment. It is this, essentially, which both Lal and Scott have done, as did Corden, Little and Scott (1975) in their original attack on the CEPG position. One question which must certainly be addressed is whether, supposing the predicament to be correctly diagnosed, protection is better than

nothing. None of the papers cited have anything at all to say about this.

In our view the answer to the question is quite clear. Growth of output and employment could be maintained by protection throughout the 1980s, and Britain would be enormously better off in every relevant way than if it is allowed to drift into ever-deepening recession. Apart from the gain measured in terms of employment alone, total production of goods and services would be vastly (of the order of at least 20%) higher and the gain to welfare through higher production would far exceed any loss through 'distortion'.* It is this point, incidentally, which renders wholly irrelevant the attempts by Greenaway and Milner (1979) and Batchelor and Minford (1977) to quantify the welfare costs of balance-of-payments adjustment by tariffs since their comparisons *postulate* given employment and output, thereby entirely begging the strategic question with which we have been concerned.

Protection versus devaluation

In the hope that people will accept that Britain's survival turns on improving our trade performance by one means or another, we now turn to the question of what is the best means of achieving this improvement. We note in parenthesis that all those economists who have criticised the CEPG from a 'devaluationist' position (in particular Corden, Little and Scott (1975) and also Williamson *et al* (1976)) should be in full agreement with us that government strategy since 1977, which has involved *increases* in the exchange rate, has constituted a disastrous shift in the wrong direction.

Discussion of the relative merits of devaluation and protection is haunted by the ubiquitous representation of the problem as one of comparative statics.

In other words, students are invited to inspect alternative situations in which external balance and full employment have been achieved and to prove (which they may easily do, provided the standard assumptions are made) that the devaluation solution will have been the better one.

As Allsop and Joshi (1980) point out in their survey of the main alternative views currently held in the UK about macroeconomic strategy, we have always conceded that devaluation would provide a solution to the main problem of worsening recession if it could be successfully implemented. As far as we are concerned, the comparison between devaluation and protection turns essentially on the feasibility of introducing each policy on a sufficient scale and on the nature of the difficulties which would be encountered during the transitional period.†

* For a rigorous exposition of the main point see Hemming and Corden (1958) especially pp. 486 and 487 where, under their assumptions, while 'it is true that the import cut causes some distortion of demand', the addition to real income generates a net gain to welfare 'so long as home produced goods constitute *any* sort of substitute for imports' (our italics).

† For a fair summary of these same points see Allsop and Joshi (1980, p. 98, col. 1).

Although we may at least join common cause with those who favour devaluation against the policies of the present government which favour a stable high exchange rate, it is not legitimate simply to assume with Scott and others that devaluation exists as a straightforward, independent instrument of policy. Before the early 1970s, when there was still a regime of fixed exchange rates, countries could within limits dictate new parities for their currencies. Today when exchange rates are no longer 'fixed' they cannot be changed with the same certainty. To devalue on a large scale would require some complex combination of official sales of sterling, relaxation of fiscal and monetary policy, and inspired rumour. The size of the fall engendered would be unpredictable and could be excessive, threatening severe inflation and risking collapse. There is an onus on those who favour devaluation to spell out exactly how they would proceed. It is not enough to argue that devaluation is a 'market solution' to the problem of fundamental imbalance since, if domestic recession has seen to it that the current balance of payments does not deteriorate in spite of adverse trends in trade, there may be no impetus at all from the foreign exchange market for the sterling parity to adjust in the right direction.

Furthermore it does not seem to have occurred to Scott (and others), who object that tariffs would have to be high and permanently rising if the domestic recession is to be permanently cured, that their position is open to the same objections. By how much and how frequently do they think that sterling should be devalued?

Supposing devaluation to be, indeed, a manageable operation in itself, the differences between the transitional periods following implementation, on the one hand of devaluation and on the other of protection,* turn on what will happen to aggregate output, the terms of trade and the distribution of national income between wages, profits and the government. In our view the devaluation strategy necessarily requires that prices initially increase relative to money earnings and that the implied cut in real earnings must be tolerated for some years if the policy is to succeed. Unless real earnings are held down by a successful incomes policy, devaluation will add to inflation without necessarily restoring full employment with an acceptable balance of payments.

On the other hand, the protection strategy can in the short term yield higher output and a better terms of trade than devaluation with no shift to profit other than that normally generated in a cyclical upswing. Therefore protection need at no stage lead to an increase in prices relative to money incomes; all the impulses are positive from the start and there is no reason to suppose the policy will be blemished or even destroyed by an inflationary spiral.

* The literature appears deficient on the dynamics of the adjustment process. These are treated rigorously in Godley and May (1977). Some, as we point out in the text, have questioned the parameter assumptions used. We certainly cannot accept Lal's statement (1979, p. 29) that Godley and May were merely 'juggling with national income identities on the basis of assuming the very relative effects that are at issue', since the parametric assumptions are based on the extensive empirical literature concerning trade elasticities.

Our grounds for taking these views are spelt out in greater detail in Allsop and Joshi* and, for those who have the patience to go through it, in Godley and May (1977).

Prices and wages

For over 20 years now one or other of us† has argued as a matter of fact (albeit a fact searching, not altogether comfortably, for a theory) that domestic prices charged by British businesses are determined by the previous movement of 'normal' costs regardless of conditions of demand and, in our more recent contention, regardless of changes in the price of competitive imports.

The normal cost theory of pricing, which is extremely indigestible if not entirely lethal to those who believe or hope that a reduction in nominal

*We may here give a reply to Allsop and Joshi's concluding comments on our position (1980, p. 99).

(1) They seem to be allowing in an assumption that a worse balance of payments could be tolerated, at least for a time, with devaluation than with protection. This destroys the *ceteris paribus* assumption which is essential when comparisons of this kind are made. Recall that Corden, Little and Scott criticised our 1975 calculations (justifiably) on precisely these grounds.

(2) It is true that if the wage bargain could be controlled or influenced then the devaluation strategy would be feasible, but it is untrue that the deleterious effects of depreciation depend upon 'some form of wage resistance'. The deleterious effects are there certainly enough, at least temporarily, in the form of lower real take-home pay.

(3) It is not at all clearly true that the differential effects on income distribution depend on our particular 'controversial' assumptions concerning the pricing of import competing sectors. If domestic prices of home-produced traded goods are influenced by the price of competitive imports this will be true under devaluation as well as under protection, so it may not alter (or not greatly alter) the comparative position. We shall show in the following section that our views about domestic pricing are confirmed by the most recent evidence.

(4) They seem to entertain the possibility of a steady *slow* depreciation under which there is no real wage squeeze. The problem is that slow depreciation will not reverse a situation which is continuously and quite rapidly deteriorating.

†Godley (1959), Neild (1963), Godley and Rowe (1964), Godley and Nordhaus (1972), Coutts (1974), Coutts, Godley and Nordhaus (1978).

demand will eventually reduce inflation, has survived with flying colours the past few turbulent years of greatly accelerated and fluctuating inflation combined with depression. As Chart 4.1 shows, wholesale prices charged by manufacturers for home sales have continued up to 1979 to follow closely the lagged movements of costs and there is no sign whatever that sluggish demand has had the slightest effect on the relationship.*

The recent period may be used to amplify evidence presented in Coutts, Godley and Nordhaus (1978) that the price of competing imports does not affect the relationship between costs and prices charged by domestic manufacturers on home sales (see Table 4.1).

It is noteworthy that in 1976, when the price of imported manufactures rose relatively fast as sterling fell, domestic wholesale prices rose less than lagged normal costs. On the other hand in 1979, when import prices rose relatively slowly (only about 3%) because sterling was strong and when import penetration was particularly rapid, prices charged by domestic producers rose about 14% – fully as much as costs or even more. The price movements since 1973 are

*The following OLS regression summarises the relationship between wholesale prices and lagged normal unit costs of production shown in Chart 4.1. The cost series has been constructed using lag distributions and cost compositions given in Godley and Nordhaus (1972) and Coutts, Godley and Nordhaus (1978).

$$\begin{array}{ll} \% PW = -0.672 + 1.055\% PH & \text{No. of observations} = 24 \\ (1.19) \quad (18.33) & \text{S.E.E.} = 1.84 \\ & \text{D.W.} = 1.71 \end{array}$$

where % PW = annual percentage change in wholesale price of home sales.

% PH = annual percentage change in lagged normal unit costs.

Figures in brackets are t ratios.

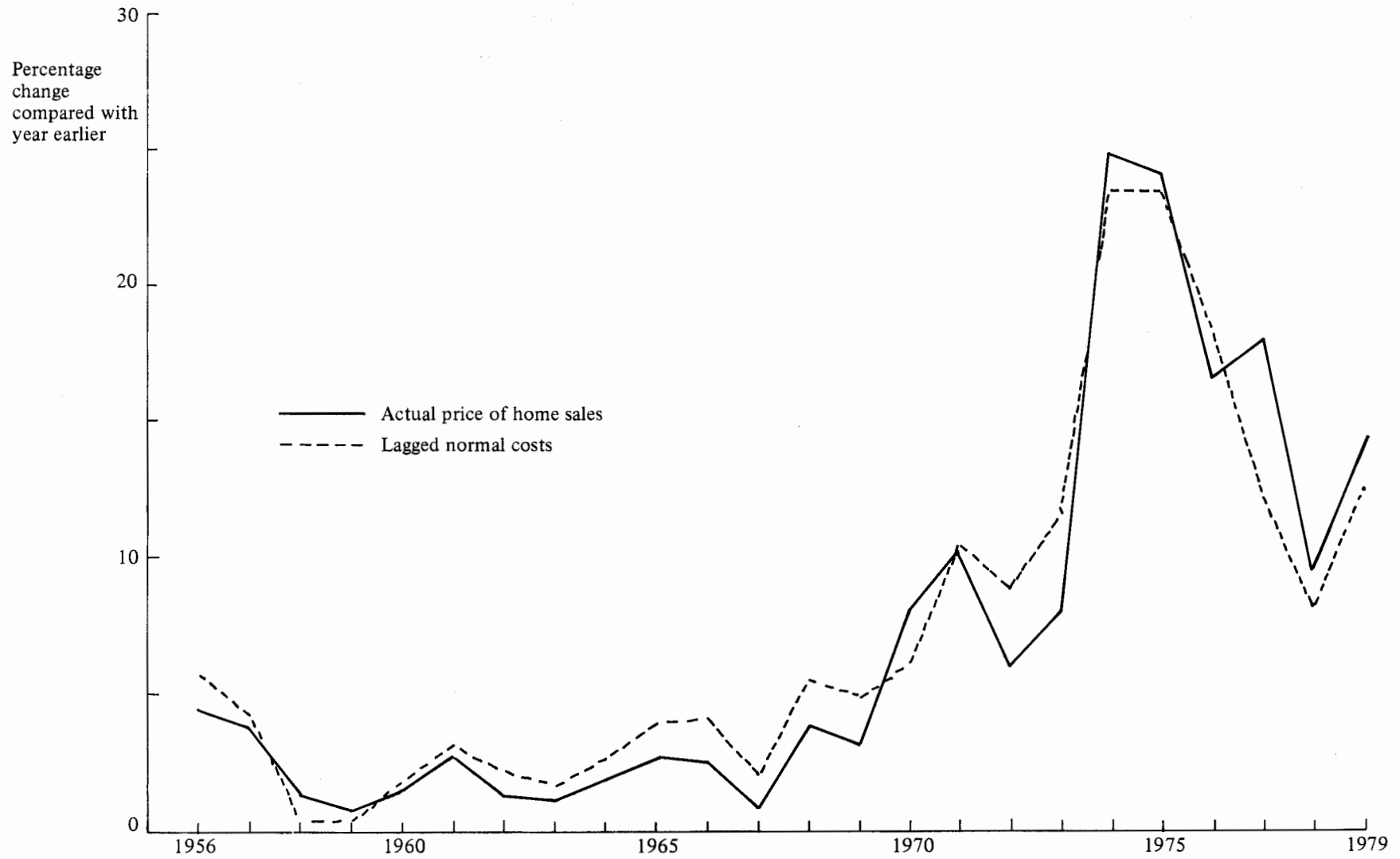
As is evident from Chart 4.1, the cost coefficient is close to unity, satisfying a demand made by some of our previous critics (for example Laidler and Parkin (1975)). Note also that the main departure of prices from lagged normal costs occurred in the first half of 1977 when prices increased relative to costs. This coincides very clearly with the end of the price control policy based on allowable cost increases and reference levels for profit margins.

Table 4.1 Import prices and domestic profit margins

(%)

	Increase in wholesale prices of home sales by domestic manufacturers less increase in lagged normal costs	Increase in unit value of imported finished manufactures less increase in wholesale prices of home sales by domestic manufacturers
1973	- 3.5	+ 7.3
1974	+ 1.3	- 2.3
1975	+ 0.6	- 2.0
1976	- 1.8	+ 8.5
1977	+ 5.6	- 3.5
1978	+ 0.7	- 1.0
1979	+ 1.5	- 10.7

Chart 4.1 Normal cost pricing: manufacturing industry excluding food, drink and tobacco, 1956-79



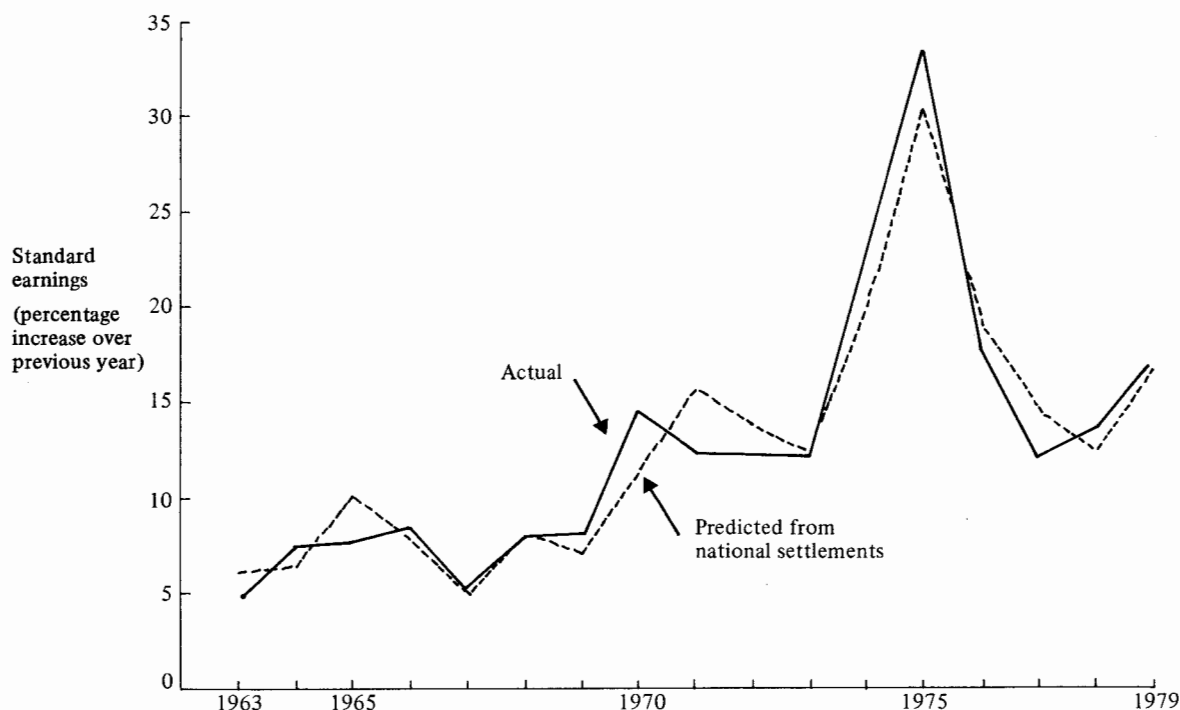
completely inconsistent with the idea that profit margins on home sales by domestic manufacturers are in aggregate influenced by the prices of competing imports.

Our views about *wage inflation* are contrary to those of most macro-economists. They hold that wages are more-or-less closely determined by market forces. We are confident that in the UK the general movement of money wages is governed by institutional bargaining which is virtually independent of market

forces. The most forceful explicit criticism of our view is by Scott (1980). Two objections are also posed by Allsop and Joshi (1980). Since the overwhelming nature of the evidence in favour of our view has not been widely appreciated, we briefly recapitulate it here.

The first point is that, as Chart 4.2 shows, money earnings can be accurately predicted as the outcome of nationally-negotiated settlements provided allowance is made for a steady process of 'drift'.

Chart 4.2 Prediction of earnings from national wage settlements



Notes: *Standard earnings:* average money earnings per full-time male employee in the business sector, adjusted for cyclical overtime.

Predicted: derived from the level and timing of national settlements for the business sector with an allowance for the trend drift between earnings and wage rates. (See *Technical Manual on the CEPG Model of the UK* for details of the prediction formula.)

There are no grounds for changing the view we have held for many years that these national settlements are heavily influenced by real wage aspirations as well as by political forces including incomes policies and perceptions of comparability.

Since it is plausible to suppose that the bargaining in question is about real take-home pay, we construct a 'real wage settlement' index which measures the real take-home value of negotiated settlements at the moment they were actually obtained.

It is particularly important to distinguish the level of real wage settlements measured by this index from the actual level of real take-home pay measured *ex post* which is determined by macroeconomic condi-

tions — broadly, the level of real national income and the share of this which comes to employees. The difference between the value of pay rates at the moment of settlement and their value when actually paid out will be perceived in terms of the extent to which the settlement wage is eroded by subsequent price or tax increases.*

The view that the level of real wage settlements is so largely determined by what may broadly be called political forces can be tested in terms of its ability to explain their movement with reference to political history.

* For a formal statement of this model see Cripps and Godley (1976), also Tarling and Wilkinson (1977).

The real wage settlement index first published by Coutts, Tarling and Wilkinson (1976) has been extended back through the 1950s and also brought up to date. This index, now covering the whole period from 1949 to early 1980, is shown in Chart A7 of the Appendix to Chapter 2.

As the chart shows, incomes policies have temporarily depressed the real settlement index below its long-run trend. The intention of the 1948 incomes policy was to hold wage rates steady but the policy broke down in 1950 following the devaluation in the previous year. The next major attempt to establish an incomes policy was during the 1960s, beginning with an abortive attempt by the Conservative government in 1961. The Labour government's policies began in 1965 with a 'statement of intent'; from 1966 to 1969 a zero norm was applied but various exceptions were allowed. This, the 1967 devaluation and rising effective tax rates substantially depressed the real settlement index, but the policy collapsed progressively from the end of 1969. The Conservative government's freeze at the end of 1972 delayed settlements by about 6 months, bunching them into the second quarter of 1973, but overall the various stages of this incomes policy between 1972 and 1974 had little impact on national wage rates at settlement.* The Social Contract of the Labour government succeeded in restraining wage settlements progressively below trend between 1975 and 1977 but tax concessions and relaxed guidelines for the 1977-78 wage round permitted some recovery. In 1978-79, the incomes policy was largely ignored and wage bargains struck last year have just about restored the real settlement index, yet again, to its long-run trend. The fact that, for three decades when labour market conditions have been so different, the real settlement index has persistently returned to the same slowly-rising trend ceiling whenever incomes policies were abandoned or broke down makes it impossible to accept that any form of Phillips curve has been operative in postwar Britain. The fact that incomes policies did always break down when they depressed the real settlement index makes it impossible to believe that any incomes policy similar to those tried hitherto could be used to make large-scale devaluation effective.

New Cambridge

The mainspring of New Cambridge was the realisation as to how unsatisfactory was the treatment of financial stocks and flows in the 'vulgar' Keynesian position generally taught in the 1950s and 1960s and built into the first generation of macroeconomic forecasting models. The standard exposition of income determination took government expenditure, tax rates and exports as exogenous, investment as determined by the accelerator, imports by a propensity, and consumption

* The real settlement index shown on the chart falls between 1973 and 1974 because threshold payments which were actually received during 1974 have been introduced into the index only when they were consolidated into base rates.

by personal disposable income. One characteristic consequence of this crude system as embodied in early econometric models was the virtual omission of any representation of company finances making it impossible, for instance, to say what would happen if corporation tax were changed. Nor was there any coherent representation of the current balance of payments as a money flow interdependent with domestic money flows. The external balance was just a relatively small gap between two large and substantially independent magnitudes (exports and imports) and 'particularly difficult to forecast' (as one used to say) for that reason.

The New Cambridge theorem, that private expenditure as a whole (personal and company) is related to private disposable income as a whole, was put forward as a useful approximation which ensured at least that all financial flows, albeit at a very high degree of aggregation, were incorporated into the system and that nothing absurd about the relationship of stocks to flows of financial assets was being assumed by default.

By far the best exposition so far of the New Cambridge position is contained in a paper by McCallum and Vines (1980) who correctly see that the essential criticism of 'vulgar' Keynesianism made by New Cambridge is the same as that made by the so-called 'global monetarists' as set out for instance in Johnson (1976). As McCallum and Vines show in a scholarly and elegant way, the central theoretical propositions, the assumptions about dynamic processes and even the simplifications made by New Cambridge are generally indistinguishable logically from those of global monetarists. The crucial difference between the two schools lies in their empirical postulates. Thus global monetarists assume continuous full employment and completely flexible market-clearing wages and prices. New Cambridge assume wages and prices to be unresponsive to changes in demand, and domestic prices, even in the 'traded' goods sector, to be unresponsive to foreign prices of competing products. So far from *assuming* full employment, New Cambridge see employment and domestic activity as being determined by the inter-relationship between net export demand and domestic fiscal and monetary policy; indeed, with regard to income, output and employment determination the New Cambridge position is firmly in the Keynesian tradition.

McCallum and Vines deliberately stop short of adjudication between the empirical propositions which separate the two schools. But we have no hesitation in claiming that the evidence strongly supports the New Cambridge side. To *assume* continuous full employment is somewhat worse than ridiculous when Britain is threatened with endemic and growing mass unemployment because of adverse trends in foreign trade. It seems equally foolish to build anything on the assumption, so clearly contrary to the evidence, that prices and money wages are perfectly flexible, downwards as well as upwards.

Of the papers criticising New Cambridge which have so far been published, that by Alan Blinder (1978) is pre-eminent. His long critique of Fetherston

and Godley (1978)* contains a complete re-exposition of the New Cambridge position using a much more comprehensible notation.

Fetherston and Godley (exactly like the global monetarists) postulated an equilibrium relationship between private disposable income YD and the private sector's stock of financial assets SFA,

$$SFA = (1 - \alpha) YD \quad (1)$$

and a unit period over which stocks adjust to flows. Since private expenditure P is given by

$$P \equiv YD - \Delta SFA$$

it follows that

$$P_t = \alpha YD_t + (1 - \alpha) YD_{t-1}. \quad (2)$$

Blinder draws the inference from (1) and (2) that

$$P_t = \alpha YD_t + SFA_{t-1} \quad (3)$$

Blinder then incorrectly interprets a unit coefficient of spending out of last period's stock of financial assets held by the private sector as being equivalent to the propensity to consume out of the net real worth of the *personal* sector. He then wrongly concludes that the wealth effect implied by the private expenditure equation is many times greater than that found in consumer budget studies. The stock of assets of the private sector to which the hypothesis refers is financial assets only, not total real wealth. Secondly, the financial assets refer to the net assets of companies and persons, i.e. refer only to net claims on the government and overseas sectors.

Even if we ignored Blinder's slip in mistaking total personal wealth with private net financial claims on other sectors, it is most misleading to assert that 'as a behavioural relation [the private expenditure function] says that private agents begin each period by spending their entire accumulated wealth, and then save a fraction $(1 - \alpha)$ of their current income stream'.

The period needed for the stock of financial assets to reach their equilibrium relationship to income must be such that the stock is lower than the income flow in that period. In other words, as pointed out by Fetherston and Godley, the value of the coefficient α must be positive and fractional. If α were negative, the private sector would spend more in acquiring assets than the increase in its disposable

income during the period, implying that expenditure initially *falls* with an increase in disposable income. If α exceeded unity, equilibrium stocks would be negative (i.e. net liabilities). The fact that the stock of financial assets must be lower than the flow of income in the period makes it possible to infer that the private sector first spends an *amount equal* to its total net stocks of financial assets at the beginning of the period. But this is now a trivial proposition quite different from the rather startling statement that people start each period by 'spending their entire accumulated wealth'.

Russell and Wakeman (1978), commenting on the 'New Cambridge' equation, provide a critique of the empirical results by rewriting the estimating equation for the relationship between total private expenditure and current and lagged private disposable income as

$$\Delta SFA = (1 - \alpha) YD \quad (4)$$

They then set out to test the hypothesis that α is constant using evidence on the UK economy.

It should be pointed out that Russell and Wakeman have made a major data error by comparing changes in stocks of financial assets held by the *private* sector with changes in disposable income of the *personal* sector. Apart from this, their testing procedure appears amateurish in the extreme -- 'back of an envelope' calculations instead of a defined statistical test with known levels of significance.

The comments on New Cambridge in a recent textbook by K. A. Chrystal (1979) hardly merit a reply. The general quality of the work may be judged by the fact that he quotes (pp. 98 and 99), as though it were authoritative, Blinder's concluding remark about Fetherston and Godley (dealt with in the footnote above):

In reviewing the differences between the Fetherston-Godley model and Keynesian economics as it is now practiced in the U.S., I am struck by how many aspects have already been jettisoned here, often after a great controversy: the completely passive supply side, the fixed interest rate, the interest inelastic investment demand, the trivialization of monetary policy, and so on. Revising any of these hypotheses would seem to be taking a step backward. . . .

but omits to mention that Blinder continues:

To the credit of the New Cambridge group, however, the one feature of the model that Fetherston clearly labels as absolutely essential to New Cambridge is also the one feature that should elicit the greatest interest on this side of the Atlantic: the unusual specification of aggregate private expenditure. I rather doubt that the sum of consumption and investment spending can be explained very well by the sum of disposable income plus retained earnings, and its lagged value, in the U.S. But, if it can be, American Keynesians will have to reexamine the prevailing empirical models of consumer and investor behaviour. An empirical study of this question in the U.S. would be most welcome, and would really decide whether there is anything in New Cambridge that we in America should import.

* Blinder's concluding remarks (p. 83) include a list of a number of aspects of Fetherston and Godley which he considers retrograde, such as the assumption that there is a fixed interest rate and a completely passive supply side, as though these are intrinsic to our thinking. In fact these were clearly stated to be simplifying assumptions made in order to facilitate a reasonably simple exposition of certain selected features of our model. None of these assumptions are made in our main model.

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