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Raising Wages in the Controlled Sector: a Comment on Elliott

by Frances Stewart and John Weeks

A well known conclusion of neo-classical economics is that a rise in the wage rate will reduce employment: this conclusion has informed wages policies in many LDCs. In a paper in this journal (January 1975) we argued that the conclusion was derived from a one-sector model; in a two sector model, a 'controlled' high-wage sector and an 'uncontrolled' low wage sector, a rise in wages in the controlled sector alone might actually increase total employment, since a switch in output from the controlled to the uncontrolled sector would raise employment in the more labour-intensive uncontrolled sector, and this could more than offset a reduction in employment in the controlled and relatively capital-intensive sector. We concluded that "the two sector approach must be pursued if intelligent wages and employment policies are to be formulated."

In commenting on our paper, James Elliott develops a general equilibrium model to examine the effects of a wage increase in the context of a two sector model [Elliott, 1979]. His main conclusion supports our findings that the effects of a wage increase in total employment in a two sector model are ambiguous: a wage increase may increase or decrease total employment depending on the elasticities involved. 'Raising w_1 could increase total employment but there is no compelling reason to suppose that this is likely to happen' [Elliott, p. 82].

We are all, thus, in agreement that it is necessary to examine the effects of a wage rise in the context of a two sector model, rather than the conventional one-sector approach; we are also agreed that in any actual situation the effects will depend on the empirical values of the relationships involved. Where there is some disagreement is in the precise form of the relationships and in the balance of judgement about the likely empirical values of the relevant elasticities. While we did not (as Elliott implies) argue that a wage rise 'is likely to increase overall employment' (rather that it *may* do so) our paper did suggest that under plausible values for the variables it might do so, whereas it is Elliott's judgement that 'a rise in the controlled sector wage rate does not seem likely to raise total employment but is likely to have the opposite effect' [Elliott, p. 83].

THE ELLIOTT MODEL

Apart from its much more complex formulation, there are certain features of the Elliott model with which we disagree, and others which we find confusing.

We disagree with (and also find confusing) Elliott's treatment of capital. He assumes that a reallocation of output between the two sectors can only occur with a reallocation of capital, and, moreover, that the required reallocation of capital towards the uncontrolled sector will be the greater the less the capital-labour substitutability in the controlled sector. In our model, we assumed that

in the short run, additional output could be obtained from the uncontrolled sector by using the existing capital stock more intensively — this assumption is in line with putty/putty neo-classical assumptions, and also appears plausible in the light of what we know about the sort of technology in use in the uncontrolled sector. In the long run, we assumed that a reallocation of investment resources would be required, but we saw no particular problem in such a reallocation since the profitability of investment in the uncontrolled sector will have risen (as demand shifts to the output of that sector), while the profitability of investment in the controlled sector will have fallen.

Under neo-classical assumptions, the short run employment increase in the uncontrolled sector, unaccompanied by any extra capital, will decrease K/L and exert downward pressure on wages in that sector. However, to the extent that capital is idle in the uncontrolled sector (e.g. rickshaws unused much of the time) this need not occur. Moreover, as we pointed out in the original article, many of those already working in the uncontrolled sector are under-employed (working short hours for example), and some of the extra output may be obtained by employing existing workers more fully rather than taking on new ones: where this happens earnings per worker in the uncontrolled sector will rise as output shifts to that sector. In this case, obviously, the total rise in numbers employed (as against hours worked) in the uncontrolled sector would be correspondingly smaller.

Capital-labour substitutability in the controlled sector is one of the determinants of the reduction in employment in the controlled sector following a wage rise; thus the greater the substitutability the greater the extra employment that must be generated in the uncontrolled sector to offset the employment decline in the uncontrolled sector. We find it very difficult to interpret Elliott's statements on this matter. 'If capital and labour are highly substitutable in sector 1 production, this required adjustment in output composition can take place very largely without any movement of capital out of sector 1 . . . If on the other hand, capital labour substitutability in controlled sector production is very low, the required change in output composition can take place only if there is some considerable reallocation of capital towards the uncontrolled sector' [Elliott, p. 76]. As stated above, we assume that in the short run both sectors exhibit sufficient flexibility in a putty-putty sense for changes in K/L ratios to occur without any movement of capital: however, in so far as that is *not* the case, we would come to the opposite conclusion from that of Elliott — *viz.* the greater the capital-labour substitutability in sector 1 (the uncontrolled sector) the greater the need for extra capital in sector 2.

A critical element in determining the effects of a wage change on employment is the effect on the level of output — as we made clear in our initial article. Clearly, if total output falls then the employment effects of such a fall may outweigh other effects following the wage change. In a strictly neo-classical model, the effects of a wage change on the level of output depend on the production function in sector 1 (and consequently the elasticity of output with respect to a change in the wage rate) and the substitutability of the outputs of the two sectors. However, as Elliott's models shows, 'the sectoral outputs can be considerably less than nearly [perfectly?]-substitutable in consumption for the Stewart-Weeks result to hold' [Elliott, p. 77].

In the main part of our analysis, we assumed no change in the overall level of output following a wage rise in the controlled sector. This assumption was not based on the assumption that the outputs of the two sectors were nearly perfectly substitutable in consumption; we spent some considerable time in discussing in what circumstances the outputs were and in what not likely to be substitutable. We assumed some substitutability – indeed a certain amount is necessary for the conclusion that employment might increase following a wage rise in one sector. However, the output assumption was to a large extent based on a non-neo-classical view of the determinants of the aggregate level of output. As Keynes emphasized, an increase in wages not only represents an increase in costs but also an increase in purchasing power. If one is considering the effects of a wage rise which is confined to a single firm, then it is legitimate to ignore the effects of extra purchasing power since only a very small part of the extra wages are likely to be spent on the products of the firm. In contrast, if a wage rise occurs across the whole economy, the effects of the wage rise – in generating extra purchasing power – may offset the effects in raising costs, so firms can raise prices without reducing their markets. A wage rise which is confined to one sector – the controlled sector – is in an intermediate position. Assuming a closed economy (as we did in our initial analysis, and as Elliott does), and assuming workers spend all additional wages, the extra wages, ΔW_1 , following a wage rise may be spent on the products of the uncontrolled sector, P_1 , or the products of the uncontrolled sector, P_2 : $\Delta W_1 = \Delta P_1 + \Delta P_2$ in money terms. The extra expenditure on P_1 , ΔP_1 , will partly offset the extra costs in that sector arising from a wage rise, thus moderating negative effects on output. To the extent that the extra income is spent on P_2 it will have positive effects on output and employment in that sector. The argument here is oversimplified. Taking the view that (within limits set by productive capacity) it is aggregate purchasing power that determines the level of output means that the effects on the level of output cannot be assessed independently of the effects on the total wage bill, which depend on the employment effect of the change in the two sectors, and the wage rates in the two sectors.

The situation is more complex in an open economy. In the first place there may be some leakage from the extra wage payments into imports. Secondly, if costs rise in the export sector, then export markets may be curtailed. But these changes would involve an adverse movement in the overall balance of trade. Consequently, they would need to be offset by changes in the exchange rate which would tend to restore the initial level of demand and output. A further complication is introduced by government policy. Government budgetary policy may offset or accentuate any change in output. In view of the complex situation, a reasonable first approximation would seem to be to assume no change in output.

The experience [see e.g. *Smith 1969* and *Turner and Jackson 1970*] of innumerable LDCs where continuous rises in money wages in the ‘controlled’ sector have been accompanied by continuous rises in output – not by the fall in output predicted by neo-classical theory – indicates that in this respect at least a Keynesian type analysis may have more to offer than neo-classical views of output elasticity.

EMPIRICAL ESTIMATES

To a large extent, the significance of the empirical estimates Elliott puts forward rests on the validity of the model: because of the major qualifications about some of his assumptions – most relevantly with respect to the determinants of output change just discussed – his conclusions derived from empirical estimates are also suspect. Nonetheless we believe Elliott has done a valuable job in collecting together the empirical work which has been done on the various parameters and showing their implications when fed into his model. However, we have serious doubts about the meaning and validity of some of the empirical estimates – in particular those relating to the substitutability of capital and labour in the controlled sector, derived from CES production functions. O’Herlihy [1972] and Morawetz [1976] have admirably summarized the main defects of such estimates: it is worth repeating some of their arguments here, since they are so often (as in this case) ignored.

In an important paper O’Herlihy showed that CES production functions assume:

- (i) the elasticity of substitution is constant over all outputs and time periods;
- (ii) economies of scale do not exist; and
- (iii) technological change is neutral between labour and capital and progresses evenly over time. [p. 279]

Micro-studies of choice of techniques have shown all these assumptions are incorrect [see *Jenkins 1975* and *Stewart 1977*]. Moreover, O’Herlihy shows that close examination of four major studies of the estimation of CES production functions in LDCs [*Reynolds and Gregory 1965*, *Erikson 1969*, *Harris and Todaro 1969* and *Katz 1969*] reveal major biases and flaws. Two of the studies assumed each industry had the same production function parameters; a third contained an upward bias because of the use of undeflated data. The fourth study – by Katz – gave much lower estimates for time series than for cross section studies. O’Herlihy found data biases and unwarranted assumptions in the cross section study. For each of the studies, other than the Katz time series study, he found that the biases and flaws tended to lead to a substantial over-estimation of elasticity of substitution between capital and labor.

Morawetz shows that ranking of industries in terms of CES estimates of elasticity of substitution shows no consistency across industry in the various studies that have been done. He concludes that this shows either (i) that the values of CES elasticities vary at different times and places; or (ii) that the industrial level of disaggregation adopted (2-digit level) is insufficient to produce stable conclusions; or (iii) ‘finally theoretical estimation and data problems may make estimates of the elasticity of substitution at the industry level unreliable’ (p. 12). Any one of these conclusions would invalidate the use of this data for establishing capital-labour substitutability in the way that Elliott uses it. Reason (i) would mean that the data is non-generalizable because of changes over time and place; reason (ii) would mean that it is non-generalizable because of differences in industry composition; and (iii) that it is not valid because of theoretical and estimation problems.

In summary, there is considerable reason to think that CES estimations of capital-labour substitutability are in fact useless, and even misleading. One

need not enter into the 'Cambridge capital controversy' to reach such a conclusion. First, it seems impossible in principle empirically to discriminate between returns to scale and technological change. As a consequence, the inclusion or exclusion of a returns-to-scale variable in the labour demand function which influences the value of the elasticity of substitution becomes totally arbitrary – at the discretion of the researcher. This, however, is only the most obvious problem of estimation. Most econometric exercises (including all those cited by Elliott) do not estimate the CES production function directly, but employ the assumption of cost minimization in order to make output (or productivity per worker) a function of the real wage. This is a convenient approach, since it *appears* to eliminate any need to include either the capital stock, or the 'price' of capital as explanatory variables. In fact, this is illusory. The cost-minimizing exercise, in which the production function is constrained with respect to the price of factors, gives rise to *two* factor demand equations – the demand for labour and the demand for capital (in the two-factor case). These equations clearly are not independent of each other, since the elasticity of substitution of capital for labour must be the exact reflection of the elasticity of substitution of labour for capital. Further, if the researcher decides to include a returns to scale variable, its parameter must be the same in each equation. This type of relationship requires simultaneous estimation for the parameters generated by either equation independently, or the other will be biased. It is not, in general, possible to predict the direction of the bias.

It is not our purpose to try to provide a definitive answer to whether the substitutability of factors in the neo-classical sense can be empirically estimated. We only point out that the evidence Elliott presents is at best ambiguous, and cannot be taken as indicative of actual behavioural relations. Indeed, we do not think it is very useful to approach the problem of the inter-sectoral distribution of employment primarily from a production function framework. A production function approach lumps all sort of change under a single label – 'the elasticity of substitution of capital for labour' – and attributes them all to a single cause – a change in the relative price of capital and labour. But observed changes in capital labour ratios arise from diverse causes – for example, economies of scale, technical change, the entry and exit of different firms using different technologies. Breaking down observed change in factor proportions into constituent parts and providing empirical estimation in concrete situations would provide far more illumination on likely behaviour in response to particular changes. In particular, we believe that any attempt empirically to verify our hypothesis would require concrete investigation of the conditions of entry and exit by producers in the uncontrolled sector. If operators can easily enter into production when conditions become more favourable (as a result of relative wage changes, for example) then the type of positive employment effect that we have suggested is likely. But if entry is difficult the case we tended to stress is less likely.

Doubts concerning CES production function estimates of capital-labour estimates mean that one of the main elements underlying Elliott's empirical estimates is undetermined. Moreover, it is likely that the estimates exaggerate the real extent of capital-labour substitutability. Using these CES derived estimates, Elliott finds a plausible range of cases where a wage rise in the controlled

sector may increase total employment. With lower estimates of capital-labour substitutability, the likelihood of this result would be increased. Thus we find nothing in Elliott's new formulation to challenge our original argument and conclusions.

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