

## Employers' Social Insurance Contributions and Employment: Reply

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### I HOLMLUND'S MODEL

Fagan and Murphy (1986) put forward a number of criticisms of my estimates of the incidence of the employer social insurance contribution in Ireland and the effect on employment of a reduction in this contribution. They assert that the labour demand and supply equations in the model which I used, i.e., Holmlund's (1983) model, are misspecified due to the omission of variables which, they say, have been shown to be relevant in "widely accepted labour market models which have been estimated and reported in the literature" (p. 46).<sup>\*</sup> Holmlund's paper was completed in 1982 while mine was completed in 1984. Since all but one of the ten papers referred to by Fagan and Murphy in footnotes 4 and 5 are dated 1983 or later, neither of us could have taken account of the results in the papers referred to even if the view of the labour market which is generally taken in these papers could be accepted without reservations. In addition, a number of variables to which they refer, such as the replacement ratio, were included in the original formulation of Holmlund's model by Parkin *et al.* (1976, p. 209) and they found that "inclusion of the relation between unemployment benefit and earnings, either as an independent determinant of excess demand or as a factor modifying the response to excess supply, made no contribution to the results". It is also worth noting that Newell and Symons (1985, p. 15), to whose paper they refer to support their misspecification argument, tested every combination of demand variables they could think of and "found no uniformity in the international data which suggests variables additional to the wage as proximate determinants of employment".

<sup>\*</sup>Page references given at the end of quotes refer to Fagan and Murphy's paper in this issue.

Fagan and Murphy's insistence that the set of variables to which they refer "represent the set of relevant variables to be included in one's most general specification" (p. 46) is rather extreme in its implication that only the variables listed by them are relevant to the operation of the labour market and in its direction to researchers to proceed from the general to the particular in their work. The data generating process in the labour market is complex and there is unlikely to be agreement on the large number of variables which may have to be included in a general model or on the criterion which should be applied in eliminating variables, as Atkinson (1985, p. 148) has noted. Some researchers, including myself, may prefer to start with a specific model which can be modified if it fails to pass misspecification tests.

The disequilibrium reaction function used by Holmlund to allow agents in the labour market to adjust towards equilibrium is thought to be unusual by Fagan and Murphy because "it is normal to assume that price adjusts to remove an imbalance in quantity, or vice versa, not that quantity adjusts to quantity" (p. 47). There is nothing unusual, however, about Holmlund's reaction function as it is just a standard partial adjustment equation which, as is normal in such equations, says that the actual change in some quantity is only a fraction of the desired change in that quantity.

## II DATA USED TO ESTIMATE THE WAGE EQUATION

Having asserted that Holmlund's model is misspecified, Fagan and Murphy move on to consider the data which I used to estimate the model for Ireland. They say that there are some problems with the data because (1) the employee tax variable which I used was calculated on the basis of a single person's tax allowances rather than being a weighted average of single and married persons' tax rates which they feel may be a better proxy, and (2) the unemployment rate in the last two years is overstated as the denominator used to derive the unemployment rate for manufacturing industry is the insured population in that sector at the end of 1978. No evidence is presented to show what difference it would make to the coefficients of the wage equation if the tax variable which Fagan and Murphy favour had been used instead of the tax variable which I used. Their argument appears to be that the change in the tax burden of married couples over the period 1953(4)-1980(4) was so different to that for single persons that use of a weighted average direct tax rate would have made a significant difference to the measurement of the tax burden for employees. What then would the change in the tax burden have been over this period if a weighted average of the tax rates for single and married persons had been used as they suggest?

The *Census of Population 1979*, Vol. II, Table 1A, shows that the proportion of the population aged 15 and over in 1951 who were married was 0.4335 while the

proportion single or widowed was 0.5665. A single person in receipt of average industrial earnings of £5.63 per week in the last quarter of 1953 would have paid £0.33 in income tax plus social insurance contribution per week while a married person would have paid a social insurance contribution of £0.10 per week but no income tax. Weighting these figures by the proportions married and single gives a weighted average direct tax rate of 4.09 per cent. In 1979 the proportion married was 0.5331 while the proportion single or widowed was 0.4668. A single person receiving average industrial earnings of £104.64 per week in the last quarter of 1980 would have paid £31.90 tax per week while a married person would have paid £22.48. Weighting by the proportions married and single in 1979 gives a weighted average tax rate of 25.68 per cent. Hence the change in the weighted average tax rate between the beginning and the end of the data period was 21.59 percentage points. The change in the direct tax rate for single persons over the same period was 24.62 percentage points (see Hughes, 1985, Table A.1). It makes very little difference therefore to the size of the change in the tax burden between the beginning and end of the data period whether a weighted average or a single person's tax rate is used. In view of this, I feel it is very unlikely that use of the weighted average tax variable suggested by Fagan and Murphy would make any significant difference to my estimates of the coefficients of the wage equation.

Fagan and Murphy's point about the unemployment rate in the last two years being somewhat overstated is correct but once again they do not say how much difference use of revised figures would make to my estimates of the wage equation. The overstatement of the unemployment rate arose following the changeover to a system of fully pay-related social insurance (PRSI) in 1979 as the CSO was unable to obtain estimates of the currently insured population classified by industrial group and it continued to use the estimates available at the end of 1978 until it, unfortunately, ceased publication of information on the percentage unemployed by industrial group in 1985. Revised unemployment rates for industry are not therefore available for 1978 and 1979 so it is not possible to assess what difference their use would make to my estimates of the wage equation. However, I would not expect the revised figures to make any significant difference as the number of insured persons in industry does not change a lot from year to year, so the revised figures, if they were available, are unlikely to differ very much from the unrevised figures.

### III DIAGNOSTIC TESTS

The range of diagnostic tests given with my estimates of the wage equation is criticised as being too limited. However, the diagnostic tests which I gave were those commonly presented in the literature such as the F-test for the joint significance of all the explanatory variables, the Durbin-Watson test which "is

arguably the most important test of misspecification in linear regression” (Harvey, 1981, p. 155), and a test of multicollinearity among the explanatory variables which is based on the condition number,  $COND(X)$ , of the data matrix. In addition, a test of the stability of the coefficients of the wage equation was carried out. This test, which is referred to on page 45 of my paper, did not indicate any instability in the coefficients of the wage equation for the period before and after the introduction of the pay-related component of social insurance contributions in the second quarter of 1974. Fagan and Murphy imply in their comments on diagnostic tests that it is standard practice to present batteries of tests such as those which they use in their paper. Desirable as it might be to present diagnostic tests additional to those which are standard in the literature, a recent survey on diagnostic checking in practice by Kramer *et al.* (1985) shows that the use of such tests is the exception rather than the rule.

Having presented their argument that my estimate of the coefficient of the employer tax variable is inconsistent due to misspecification of the wage equation and having proposed a battery of tests of misspecification, in addition to those already used in my paper, Fagan and Murphy cite as evidence for their argument the estimates of the structural equations which they derived using a different instrument set to that used in my paper. Neither Holmlund nor I estimated any of the three equations presented in Fagan and Murphy’s Table 1, as they acknowledge in their comments, as the purpose of the labour market model which we both used was to derive a wage equation which would be suitable for estimating the extent of tax shifting in the labour market. If this equation is misspecified one would expect to see some evidence to that effect when the tests which I used, or those which Fagan and Murphy propose, are applied to the wage equation. Fagan and Murphy, however, are unable to find any evidence of misspecification of the wage equation although they use a different instrument set and a different sub-period, 1954(2)-1976(4) instead of 1954(2)-1974(1), to replicate my estimates of the restricted and unrestricted versions of the wage equation. Indeed, far from finding any evidence of misspecification of this equation. They acknowledge that “on purely statistical grounds the equations are not unsatisfactory” (p. 51). However, they are not willing to accept the evidence of their own tests when these are applied to the wage equation as they explain away its satisfactory statistical performance by arguing that “this result is not altogether surprising as *any set* of trending time series data on wages prices and tax rates would probably yield results that are equally satisfactory” (p. 51, my italics). This is a remarkably uninformed statement to make as it completely ignores the difficulties which have been encountered in econometric studies of payroll tax incidence in Ireland and other countries in establishing results which are satisfactory on theoretical and econometric grounds. Bradley and Cassidy (1979, Sections 5.1-5.5), for example, employed annual data on wages, prices and tax rates for the period

1959-76 to study wage determination in Ireland by sector using the labour market labour model made operational by Parkin *et al.* (1976), i.e., the same model as Holmlund and I used, and they found that none of the employer payroll tax variables were significant and the coefficients of the employee direct tax variables had signs which were contrary to *a priori* expectations. In an earlier paper, Holmlund (1981, pp. 23-24) noted that "various efforts to estimate the relationships between wage inflation and payroll tax changes in the US have been undertaken — with results that are ambiguous to an embarrassing degree" while Beach and Balfour (1983, p. 35) commented that eight studies of payroll tax incidence, to which they refer in their paper; "employed different data bases and empirical procedures, and obtained widely differing results". The evidence from these and other surveys of the literature on payroll tax incidence would not therefore appear to support Fagan and Murphy's contention that any data set on wages, prices, and taxes which trends over time would yield results which are as satisfactory as those given in my paper.

Fagan and Murphy note in commenting on my estimates of the employer payroll tax variable that the coefficient is about twice as large in the unrestricted as in the restricted version of the wage equations. However, there is nothing unusual in this as tax incidence theory allows one to place *a priori* restrictions on the coefficients of the relevant tax variables which can then be tested to see if they are rejected by the data. The restrictions which I placed on the coefficients of variables in the wage equation were not rejected by the data and it is for this reason that I prefer the restricted to the unrestricted version of the wage equation. As Beach and Balfour (1983, p. 40) note "in order to get estimates of the tax-shift factor, it is useful to impose a certain amount of structure on the problem so that one can begin to get meaningful and conclusive results". Fagan and Murphy also note that their tests of the validity of the set of instruments which they used in replicating my estimates of the wage equation are not significant but they comment that this is "possibly because the set of instruments is a limited set" (p. 52). It is difficult to understand why they bothered to test for validity of their instrument set when they appear so reluctant to accept the outcome.

#### IV ESTIMATE OF THE EMPLOYMENT EFFECT OF A PAYROLL TAX CUT

In their comments on my estimate of the effect on employment in manufacturing of a reduction in the employer social insurance contribution, Fagan and Murphy criticise my estimate on the grounds that an unconditional labour demand elasticity should have been used, that the labour demand elasticity which I used,  $-0.2$ , comes from the low end of a wide range, and that

recent studies have shown that the elasticity of demand for labour in Ireland lies in the range  $-0.75$  to  $-1.25$ .

Their first point is incorrect. The purpose for which my estimate of the employment effect was derived was to compare it with an estimate given by the Confederation of Irish Industry (CII) in its *Newsletter* of 17 January 1984. The CII used a conditional elasticity of demand in its calculations because it was advancing a proposal for job-saving having regard to the 1980–83 recession and I did the same in order to compare like with like. It should be clear from the context that my conclusion (Hughes, 1985, p. 48) that “the employment elasticity of a payroll tax cut in Ireland, . . . , appears to be quite low” refers to the short run, but if it is not, I accept that this qualification should be added to it. An important point underlying the comparison of the employment effect, which Fagan and Murphy appear to miss, is that the CII failed to allow for any shifting of the employer social insurance contribution in its calculations. If it had done so, its estimates of the employment effect would have been reduced even though it used a larger estimate of the conditional elasticity of demand for labour than was warranted by the research results which were available on the real wage-employment relation in Ireland at the time that its calculations were made.

The second point made by Fagan and Murphy is also incorrect. There were no reliable estimates available in 1984, when I was writing my paper, which indicated that the conditional elasticity of demand for labour in manufacturing industry in Ireland covered a wide range. The estimates which were available indicated that this elasticity tended to be small and similar to estimates which had been made for other countries (cf. Hazeldine, 1981, p. 155). Fagan and Murphy’s argument appears to rest on the fact that the CII used an elasticity figure of  $-0.5$  in arriving at its estimate of the employment effect of a reduction in the employer social insurance contribution. This figure was taken from a report by the NES (1983) in which, *inter alia*, previous research results on the relation between the real wage and employment in transportable goods industries in Ireland were reviewed. Two sets of results were referred to in this report. The first set was contained in the OECD (1979) annual review of Ireland for 1979 and it suggested, according to the CII’s reading of the NES Report, that for the period 1955(3)–1974(4) “employment in transportable goods industries declined by 0.25 per cent for every increase of 1 per cent in the real wage” (CII *Newsletter*, 17 January 1984, p. 3). The second set of results was given in a paper by FitzGerald and Keegan (1981–82) for the period 1961–77 and their “research puts the figure between 0.2 per cent and 0.5 per cent” according to the CII *Newsletter’s* interpretation of the NES review of this research. However, this interpretation of the FitzGerald and Keegan results misrepresents their findings because what the NES (1983, p. 51) said in its review was that “a recent estimate is that the short-run elasticity of employment in transportable goods industry with respect to the real wage is *either*  $-0.2$  or  $-0.5$ ” (my italics). The

NESC Report did not say that the short-run elasticity lies in the range  $-0.2$  to  $-0.5$  because FitzGerald and Keegan had found that their estimates of the coefficient of the real wage variable in the equation for employment in transportable goods industries fell from  $-0.5$  to  $-0.2$  when one more year was added to the estimation period and data were drawn from the 1978, rather than the 1977, issue of *National Income and Expenditure*. The NESC Report noted that there was a good deal of uncertainty about FitzGerald and Keegan's elasticity estimates and FitzGerald and Keegan (1981-82, p. 67) themselves noted that "these results indicate the need for caution when considering the behaviour of the model, in particular when the employment effects of changes in wages are being estimated". Despite these warnings the CII chose to use the higher estimate,  $-0.5$ , of the short-run elasticity of demand for labour because it believed that the size of the elasticity would tend towards the higher estimate in the labour-intensive sector of industry with which it was concerned. The figure which the CII used is, therefore, based on its own assumption about the size of the relevant elasticity rather than on any evidence which indicated that it covered a wide range at the time that the CII *Newsletter* was issued in 1984.

The figure for the short-run elasticity,  $-0.2$ , which I used in deriving an estimate of the effect on employment of a reduction in the employer social insurance contribution to compare with the CII's estimate of the employment effect was derived, as I indicated in my paper, from direct estimates of this elasticity by Walsh (1978), using quarterly data for the period 1958-76, and myself (Hughes, 1982), using quarterly data for the period 1953(1)-1980(4). I also took account of the indirect estimates of the labour demand elasticity implicit in Geary, Walsh and Copeland's (1975) estimates of the elasticity of substitution, which suggested, as I noted in my paper, that the price elasticity of demand for labour could lie in the range  $-0.1$  to  $-0.5$ . Their data period is 1953-69 and the higher estimate in this range is an overestimate, as they acknowledge themselves (Geary, Walsh and Copeland, 1975, p. 306) when they note that the specification of the cost of capital from which it is derived ignores capital grants, among other factors, and they comment that "it seems that ignoring capital allowances and capital gains leads to an overestimation of the elasticity of substitution". I chose, therefore, to use a figure of  $-0.2$  for the short-run labour demand elasticity rather than the figure of  $-0.5$ , which the CII had used, because all of the reliable evidence which was available at the time of writing indicated that the short-run elasticity of demand for labour in transportable goods industries was approximately  $-0.2$ . It should also be noted that none of the four recent studies using Irish data, to which Fagan and Murphy refer, shows that the short-run or conditional elasticity is greater than  $-0.21$ . Geary and Murphy's (1986, Table 2) estimate is 0.235 but this has the wrong sign and the coefficient is not significant. Bradley and FitzGerald's (1986, p. 20) estimates lie in the range  $-0.058$  to  $-0.073$  while Newell and Symons (1985,

p. 49) estimate is  $-0.21$ . Bradley, Fanning, Prendergast, and Wynne (1985, p. 177) do not give an explicit estimate of the conditional elasticity of demand for labour but such an elasticity is, of course, implicit in their factor demand equations and the low elasticity of substitution between capital and labour,  $0.2926$ , is suggestive of a correspondingly low conditional elasticity of labour demand.

The third point which Fagan and Murphy make is correct but it is misplaced because it is the conditional, and not the unconditional, elasticity which is relevant to the comparison of my estimate of the employment effect with the CII's estimate. However, it is worth noting in connection with their third point that the evidence concerning the size of the long-run elasticity of demand for labour according to the four unpublished studies which they cite, all of which have appeared since my paper was written, is not as strong as they suggest. Geary and Murphy's (1986) estimates, for example, cannot be relied upon because neither the long- or short-run elasticities are significantly different from zero as they acknowledge in footnote 6 in their paper where they state that "clearly the long-run wage elasticities are large and not well determined. When the demand for labour equation conditional on output is estimated, the wage elasticities are appreciably smaller but even less well determined". Bradley and FitzGerald's (1986, p. 20) estimates appear to be significant but the figures have been revised downwards from about  $-0.87$  to around  $-0.75$  since their first draft appeared and the paper by Bradley, Fanning, Prendergast and Wynne (1985, pp. 176-178) does not contain an explicit estimate of the unconditional elasticity although John Bradley has informed me that such an elasticity is implicit in the reduced form of their equations determining industrial capacity output, capacity utilisation and labour demand.

If the recent estimates of the elasticity of demand for labour in the long run are used to derive the total employment effect of a reduction in the employer social insurance contribution using my estimate of the short-run incidence of this contribution, it will clearly be considerably larger, as Fagan and Murphy point out, than the estimates which either the CII or I gave using different estimates of the short-run elasticity of demand for labour. However, the example which they give in which the employment effect is 3.75 times greater than my estimate depends on the assumption that the short- and long-run incidence of the payroll tax are the same. This may not be the case as there are grounds for believing (see Feldstein, 1974) that the entire burden of the tax falls upon labour in the long run. Apart from the possibility that the incidence of the payroll tax may differ in the short and long run, the fact that the long-run elasticity of demand for labour is significantly greater than the short-run elasticity does not indicate that a policy of creating jobs by reducing employers' PRSI contributions, as argued by the CII, should be adopted. The importance of the recent estimates of the long-run elasticities depends on a number of things apart from their size. For



policy purposes the length of time which it takes for the full output and substitution effects of a change in labour costs to feed through to employment is clearly very important. None of the recent studies gives an explicit estimate of how long the long run is but Newell and Symons' labour demand equation for Ireland suggests that the long-run real wage elasticity has a mean lag of around two and a half years so it could take a considerably longer period for most of the change in employment to come through. The relationship between the elasticity of the money demand and the real demand for labour is also very important for policy purposes because, as Solow (1980, p. 6) notes, "for practical purposes, one would want to know the elasticity of demand with respect to the nominal wage, taking account of the likelihood that prices will follow wages down, at least partially". When the aggregate quantity of money income accruing per unit of time is constant Pigou (1968, Chapter X) shows that the reciprocal of the money demand elasticity is equal to the reciprocal of the real demand elasticity minus labour's share of output. Bradley, Fanning, Prendergast and Wynne (1985, p. 35) show that labour's share of added value in industry averaged 72 per cent over the period 1962-80, according to their second measure which is the appropriate one for our purpose. Hence, the range of the money demand elasticity implied by the range of  $-0.75$  to  $-1.49$  for the long-run real demand elasticity is  $-0.49$  to  $-0.73$ . For practical purposes therefore the responsiveness of the demand for labour in the long run to reductions in labour costs may be significantly less than indicated by the recent estimates of the long-run real wage elasticity.

I discussed briefly in my paper the cost of increasing employment by a reduction in the employer social insurance contribution relative to the cost of job creation by grant aid to Irish industry. Fagan and Murphy do not agree with the way in which this issue is dealt in my paper and they argue that a proper treatment of the issue should be based on a model in which the relevant variables are jointly determined. Such an approach is desirable but until it is undertaken I believe that the estimates which I gave of the relative costs of the two job creation policies are useful in showing that a reduction in the employer social insurance contribution would appear to be a far more costly option than the policy of grant aid. It is interesting to note that in a recent assessment of the cost of different job creation policies in the United Kingdom Davies and Metcalf (1985, p. 1) used a number of macroeconomic models, including the Treasury model, and they concluded that "macroeconomic measures to reduce taxation (income tax, VAT or employers' NICs) are by far the most expensive way of cutting unemployment, at least over a two year period" whereas "special employment measures are by far the most cost effective way of cutting unemployment — the next cheapest method is five times as expensive".

## V CONCLUSION

Fagan and Murphy have argued that the labour market model on which my estimate of the incidence of the employer social insurance contribution is based is misspecified but they have failed to find any evidence of misspecification in the wage equation. They do not supply any evidence to support their case that there are some problems with the data I used and the calculations I have done in connection with their suggestion that a weighted average tax rate should have been used suggests that their argument is spurious. Their criticisms of my estimate of the employment effect of a reduction in the employer social insurance contribution rests on their contention that the labour demand elasticity which I used came from the low end of a wide range and that the unconditional elasticity of demand should have been used instead of the conditional elasticity. I have presented the evidence which was available on the conditional elasticity at the time I was writing and the evidence which has become available subsequently. Neither set of evidence shows that the conditional elasticity of demand for labour is significantly greater than the figure which I used. The purpose for which my estimate of the employment effect was derived was to consider the implications for employment of a reduction in the employer social insurance contribution when part of this contribution could be shifted on to labour because the CII had not allowed for payroll tax shifting in its calculations. The CII used a conditional elasticity of demand for labour in its calculations and I did the same in order to compare like with like. I do not, therefore, accept Fagan and Murphy's argument that my estimate of the employment effect is misleading. My estimate refers to the short run and it is, therefore, perfectly valid for the period to which it relates. The estimates of the long-run elasticity of demand for labour with respect to the real wage which have been produced recently are interesting as they suggest that the labour market in Ireland is more responsive to changes in the cost of labour than appeared to be the case in the past. However, the implications of these estimates for labour market policy need to be clarified for two reasons. The first is that it is the money demand elasticity which matters for practical purposes and the second is that we do not know how long it could take for the full effect of labour cost reductions to come through to employment.

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