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ESTIMATING LABOR SUPPLY RESPONSES USING TAX REFORMS

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The 1980's tax reforms and the changing dispersion of wages offer one of the best opportunities yet to estimate labor supply effects. Nevertheless, changing sample composition, aggregate shocks, the changing composition of the tax paying population, and discontinuities in the tax system create serious identification and estimation problems. We develop grouping estimators that address these issues. Our results reveal positive and moderately sized wage elasticities. We also find negative income effects for women with children.

KEYWORDS: Labor supply, difference-in-differences estimator, incentive effects, tax reform.

1. INTRODUCTION

THE LARGE NUMBER of tax policy reforms in the UK over the 1980's provides an ideal opportunity to evaluate labor supply responses. Indeed, since some working individuals will have been exempt from any direct impact of these reforms due to the progressive nature of the tax system, it may be thought that a control group suitable for evaluating reforms over time could be constructed.

Labor supply effects have been notoriously difficult to estimate in a robust and generally accepted way.[2] The difficulties that researchers typically face relate to the treatment of (nonlinear) tax schedules, the fact that individuals have different tastes over nonmarket time and consumption for reasons that cannot be controlled for using observable information, and the fact that individuals' observed decisions represent intertemporal allocations as well as within period allocations. These issues lead to difficult simultaneity problems with the wage rate and other household income. Thus for example, all else being equal, "hard workers" will be facing higher marginal tax rates and hence lower hourly wages. This biases wage effects downwards. Instrumental variables based on arbitrary exclusion restrictions (such as excluding education) may provide no solution since these variables are probably correlated with tastes for work. However, tax reform can lead to exogenous changes in after-tax wages and

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[2]See, for example, Mroz (1987).

incomes. Thus, the potential for direct evaluation of labor supply effects based on a comparison of responses over time by groups of individuals affected differentially by the reforms is evident.

Our analysis concerns the labor supply responses of married or cohabiting women. In the UK, the weekly hours of work distribution for this group is very dispersed with individuals observed working anything from 1 to over 60 hours a week. Individuals in this group may be expected, more than any other group, to be able to change hours of work in response to changing economic conditions or to changes in household composition. Our idea is to combine a structural approach together with instrumental variables, exploiting the variability induced by the tax reforms and the changing wage structure in order to circumvent the simultaneity problems mentioned above. The structural side of the analysis is crucial; this allows us to distinguish between income and substitution effects which are at the center of the policy debate on incentives and on welfare effects of taxation. Our model will be consistent with life-cycle behavior and the estimation method will allow for the presence of fixed costs of work.

Thus in this paper we derive the conditions on grouping estimators required for the identification and estimation of wage and income elasticities. We relate this to the standard difference of differences approach and consider whether grouping according to tax status itself is likely to provide a reliable guide to labor supply responses. For these purposes the UK tax system has the advantage that it is quite simple, with most people being either basic rate taxpayers or non-taxpayers (because their earnings are below an exogenously given threshold). We argue that composition changes between these two groups, partly induced by the changes in tax policy itself, invalidate grouping according to taxpayer status.

Our identification strategy relies on comparing the labor supply responses over time for different groups defined by cohort and education level. Thus our approach exploits the differential growth of marginal wages between these groups. These differential changes reflect both the differential impact of the reforms on these groups as well as the differential growth in real wages; the latter is due to the well documented increases in the returns to education and to the cohort effects on the wage distribution.

Hours of work is just one aspect of work behavior. Another one is, of course, labor force participation, on which the tax reforms and the change in wage structure are likely to have important effects. Thus, in the presence of fixed costs of work or other factors that differentiate the participation model from labor supply, our behavioral elasticities cannot give the complete picture of the incentive effects of changes in taxes.

The data used in our empirical analysis come from the repeated cross-sections of the UK Family Expenditure Survey over the period 1978 to 1992. The FES data provide detailed information on wages, hours, consumption, and household composition. Although not panel data, they provide consistent and accurate micro level information over a long period of time. These data have also been the subject of considerable empirical application to date and have the distinct advantage of collecting accurate information on hours worked, earned income, and consumption expenditures across all household members, consumption data being particularly important in placing the labor supply decision in a life-cycle context. The data also contain detailed information on the demographic structure of the household.

We begin in Section 2 with a description of the tax reforms of the 1980's. Section 3 discusses the identification of labor supply effects using tax reforms. In this context we develop suitable difference of differences estimators. Section 4 contains the empirical results. It begins with a description of the data. We provide a contrast between the response parameters estimated using our grouping estimator and those using taxpayer status as a grouping instrument. Our estimates provide small but positive uncompensated wage elasticities with income elasticities that are also small. These are shown to differ by household composition, but the general picture remains the same. In contrast, grouping by taxpayer status gives negative uncompensated wage elasticities. This is shown to result from the systematic change in composition in the taxpayer groups over time. Section 5 concludes. Further information on the data and intermediate results are presented in Appendix A. In Appendix B we present the way we estimate the covariance matrix of our estimator.

2. THE UK TAX POLICY REFORMS

In the UK all individuals, irrespective of the total level of household income or consumption, have a tax allowance. Tax is paid only on earnings above this allowance. This aspect of the British tax system implies that about 30% of working married women do not pay tax on earnings. We refer to this group as non-taxpayers. Although this allowance has in the past been different for married men than for (married or single) women, it is totally independent of expenditures; this makes it known to the researcher and the individual very clearly. Beyond this allowance a basic rate of tax is paid. Almost always taxes are collected at source through the Pay as You Earn (PAYE) system. Over this period, reforms announced on budget day and implemented immediately. Any changes to the tax system were widely publicized.

Beyond a certain level of income a higher rate of tax is paid. In addition to income tax, individuals also pay national insurance contributions (NI). These are paid on the entire income by individuals earning above a threshold, the lower earnings limit (LEL). No national insurance contributions are payable over the "upper earnings limit." This system creates a discontinuity in the budget set. To obtain a correct measure of the marginal tax rate for individuals earning more than the tax allowance, we need to add the income tax and NI rates. The budget constraint for British workers over most of our sample period had the form shown in Figure 1, where we have omitted the higher rates, which are faced by

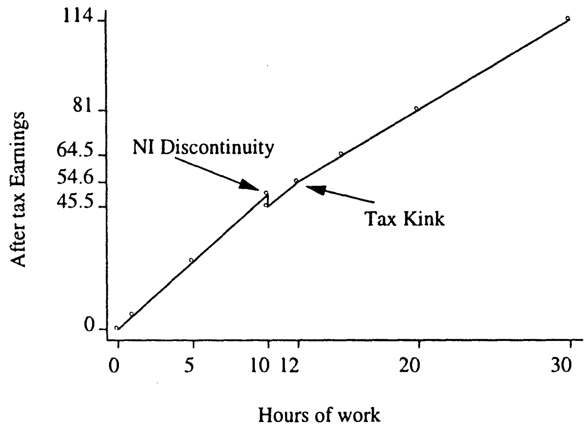

FIGURE 1.—The budget constraint (illustrated for NI rate 9%, tax rate 25%, pre-tax wage £5).

practically no women in our sample. The NI kink and the tax kink are very close to each other in practice.

Finally almost all goods are subject to indirect tax (VAT). Thus, for example, in a partial equilibrium setting and on the assumption that an extra pound of earnings is spent on the consumption good, the marginal tax rate for a taxpayer is (income tax + NI + VAT)/(1 + VAT).

Table I presents the main changes to the tax system that are relevant for the marginal wages of married women: The overwhelming majority of taxpaying women face the "basic rate" (plus the NI rate). Nevertheless, there has been more reform activity, which has been affecting mainly incomes earned by men, who face higher rates more often. These reforms are important for our study since they affect what is other (or unearned) income for the wife. The reforms greatly simplified the tax system. In 1978 there were 12 tax bands (a "lower rate" 25%, the "basic rate" 33%, and the "higher rates" 40–75% in 5% steps and 83%). In 1979–1980, the basic rate was reduced to 30% from 33% and all rates higher than 60% were abolished. In 1980/81 the lower rate of 25% was abolished. In 1988 the basic rate was reduced to 25% and rates higher than 40% were abolished. Nevertheless it must be stressed that the earnings threshold for 40% has been falling in real terms and certainly not keeping up with earnings growth. As a result while only 3% of taxpayers (male and female) in 1978 were facing a rate higher than 33%, the equivalent figure now is 10% (at 40% rate). The other important aspect of the reforms has been the phasing out of the NI kink discontinuity. Since 1989 the drop in income at the NI kink is only 2% as opposed to 6.5% and 9% up until then. Nevertheless the 9% contribution rate for earnings above the LEL remains.

Although the reform agenda can be summarized by saying that there was an overall restructuring with a shift away from direct and towards indirect taxation,

TABLE I

TAX REFORMS FOR THE YEARS 1978 TO 1992

Year	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92
Basic rate	33	30	30	30	30	30	30	30	29	27	25	25	25	25	25
Top rate	83	60	60	60	60	60	60	60	60	60	40	40	40	40	40
Tax All. (%Δ)	0	8	0	−10	0	8	2	2	0	0	2	0	0	−3	−2
NI	6.50	6.50	6.75	7.75	8.75	9.00	9.00	9.00	9.00	9.00	9.00	9.00	9.00	9.00	9.00
VAT	8	15	15	15	15	15	15	15	15	15	15	15	15	17.5	17.5

Note: "Tax All. (%Δ)" denotes real percentage change in the tax allowance. The basic rate, the top rate, NI, and VAT are in percentage terms. The top rate is the maximal applicable income tax marginal rate. From 1988 the top rate was the only rate higher than the basic rate.

the timing of the individual reforms has been such that effective tax rates have been increasing as well as decreasing over this time period. In addition the tax base, as defined by the size of the nontaxable allowances, has both increased and decreased over the sample period (1978–1992). This is evident in Table I, where the allowance changes are given in real terms. Table II presents the average marginal rates faced by our sample of women married to employed men from the UK Family Expenditure Survey broken down by cohort and level of education. The tax rates change differentially across groups. In fact about 33% of the variation in the table is explained by cohort/education/time interactions, the remaining being explained by the primary cohort/education effects and the time effects.

Households where the husband is out of work will typically be entitled to means tested benefits, which implies that the wife will face marginal tax rates close to 100% and a highly nonconvex budget set. We decided to simplify the analysis by concentrating on women with employed husbands only. For them the budget set is much simplified. Given that most married men are employed, the potential selection bias is likely to have a minor impact on the results.[3]

TABLE II

MARGINAL TAX RATES BY FINANCIAL YEAR, EDUCATION, AND COHORT

	Compulsory Education				Post-compulsory Education				
	< 1940	1940–49	1950–59	1960 +	< 1940	1940–49	1950–59	1960 +	Total
Financial Year									
1978/79	0.29	0.25	0.31	·	0.37	0.31	0.35	·	0.29
1979/80	0.28	0.24	0.26	·	0.32	0.29	0.32	·	0.27
1980/81	0.29	0.24	0.27	·	0.30	0.26	0.34	·	0.28
1981/82	0.29	0.24	0.28	0.31	0.33	0.28	0.33	·	0.28
1982/83	0.27	0.23	0.25	0.36	·	0.30	0.33	·	0.27
1983/84	0.26	0.23	0.24	0.32	·	0.29	0.29	·	0.26
1984/85	0.28	0.21	0.22	0.31	0.30	0.29	0.31	·	0.26
1985/86	0.29	0.24	0.21	0.32	·	0.26	0.30	0.37	0.27
1986/87	0.27	0.23	0.23	0.31	·	0.27	0.30	0.35	0.27
1987/88	0.24	0.23	0.22	0.28	·	0.30	0.30	0.31	0.26
1988/89	0.23	0.22	0.20	0.24	·	0.25	0.26	0.31	0.24
1989/90	0.23	0.25	0.21	0.23	·	0.29	0.26	0.29	0.25
1990/91	0.24	0.25	0.22	0.24	·	0.27	0.26	0.30	0.25
1991/92	0.24	0.26	0.22	0.24	·	0.29	0.27	0.29	0.25
1992/93	0.25	0.27	0.23	0.25		0.27	0.26	0.28	0.26
Total	0.27	0.24	0.24	0.27	0.33	0.28	0.30	0.30	0.26

Note: Cells with a full stop denote either empty cells or cells that were excluded because the number of observations was less than 50.

[3]As a referee pointed out, the business cycle could generate some composition effects for which we do not account due to the conditioning on households with employed men only.

In summary the large number of policy reforms over this same period have provided shifts in the tax system, sometimes increasing taxes and sometimes decreasing them, that enhance our ability to identify labor supply responses to tax reform. Tax reform is not the unique source of identifying information. There has been a large increase in wage dispersion mainly due to the increase in the returns to education and to increases in wage dispersion across cohorts (see Gosling, Machin, and Meghir (1996) and Schmitt (1993)). Such variation will be a source of identifying information.

3. IDENTIFYING LABOR SUPPLY RESPONSES FROM TAX POLICY REFORMS

3.1. *A Specification for Labor Supply*

We specify the equation for weekly hours of work (h) to be

$$h = \alpha + \beta \ln w + \gamma \mu \tag{1}$$

where w is the post tax hourly wage rate and μ is other income defined by the difference between consumption (c) and wh, i.e. $\mu = c - wh$. This definition of other income is consistent both with intertemporal two-stage budgeting in the absence of liquidity constraints and with the presence of liquidity constraints (MaCurdy (1983), Blundell and Walker (1986), and Arellano and Meghir (1992)). Given that expenditures are collected using diary records, this is a good way of reducing measurement error in the computation of other income in any case. Unobserved taste variation can be introduced by allowing α to vary in the population.

3.2. *A Grouping Estimator*

To see the issues involved in estimating labor supply responses using tax reforms, we simplify the notation and use a labor supply equation with no income effect. Suppose this takes the form

$$h_{it} = a + b \ln w_{it} + u_{it} \tag{2}$$

where h_{it} are hours of work and w_{it} is the post-tax hourly wage rate for individual i in financial year t. The error term u_{it} in general will be serially correlated, correlated with the observables, and may be dependent across individuals, reflecting common (macroeconomic) shocks.

The presence of common shocks have a number of implications. First, tax reforms may no longer be exogenous for labor supply: Governments may time their reforms based on their predictions on how the aggregate labor supply is likely to shift over time. Second, even if this were not the case, the mere fact that the number of time periods we have is fixed will imply that the aggregate shocks do not average to zero and hence are a potential source of bias. With a large number of time periods, if tax reforms were predictable by instrumental variables exogenous to labor supply, we could use time series methods on the aggregated data to estimate the wage effects. In general this is not the case; data will consist of a number of repeated cross sections over a relatively small number of time periods. In order to control for the presence of common shocks in this context we need to use some cross section variation. Estimation will have to rely on comparing otherwise similar groups of individuals who have been affected in different ways by the reform, for reasons that are exogenous to labor supply.

The problems in estimating the wage effect b in (2) are the following. We need to control for (i) the common shocks, (ii) for the correlation of w_{it} with u_{it}, and (iii) for self selection into employment.

Suppose individuals can be categorized in one of, say, two groups, $g=\{u,d\}$, each sampled for at least two time periods.[4] For any variable x_{it}, define

$$D_x^{gt}=E(x_{it}|P_{it},g,t)-E(x_{it}|P_{it},g)-E(x_{it}|P_{it},t), \tag{3}$$

where P_{it} indicates that the individual is observed working. We start by making the following assumptions:

ASSUMPTION A1.1: $E(u_{it}|P_{it},g,t)=a_g+m_t$.

ASSUMPTION A2.1: $E[D_w^{gt}]^2\neq 0$.

Assumption A1.1 summarizes the exclusion restrictions for identification; it states that the unobserved differences in average labor supply across groups can be summarized by a permanent group effect (a_g) and an additive time effect (m_t). In other words differences in average labor supply across groups, given the observables, remain unchanged over time. It also says that any self selection into employment (the conditioning on P_{it}) can be controlled by group effects and time effects additively. Assumption A2.1 is equivalent to the rank condition for identification; it states that wages grow differentially across groups; this is because the assumption requires that after we have taken away time and group effects there is still some variance of wages left. If there is a tax reform between two periods, affecting the post-tax wages of the two groups in different ways, and assuming that tax incidence does not fully counteract the effects of the reforms, identification of the wage elasticity will be guaranteed.

With these assumptions we can implement a generalized Wald estimator (see Heckman and Robb (1985) for an extensive discussion of grouping estimators). Defining the sample counterpart of D_x^{gt} as $\tilde{x}_{gt}=\bar{x}_{gt}-\bar{x}_g-\bar{x}_t$, i.e. the time-group cell mean minus the overall mean for group g over time and minus the mean at time t over all groups (all defined over workers only), we can write the estimator as

$$\hat{b}=\frac{\Sigma_g\Sigma_t\left[\tilde{h}_{gt}\right]\left[\widetilde{\ln w}_{gt}\right]/n_{gt}}{\Sigma_g\Sigma_t\left(\widetilde{\ln w}_{gt}\right)^2/n_{gt}} \tag{4}$$

[4] The key to the approach is the choice of groups. We discuss this later in detail.

where n_{gt} is the number of observations in cell (g, t). The implementation of this estimator is simple; group the data for workers by g and by time and regress by weighted least squares the group average of hours of work on the group average of the log wage, including a set of time dummies and group dummies. An alternative that gives numerically identical results is as follows: regress using OLS the log after-tax wage rate on time dummies interacted with the group dummies, over the sample of workers only and compute the residual from this regression. Then use the original data to regress hours of work on the individual wage, a set of time dummies and group dummies, and the wage residual. The t value on the coefficient of the latter is a test of exogeneity, once the standard errors have been corrected for generated regressor bias (see Pagan (1986)) and intra-group dependence. This is the approach that we follow. Finally note that for two time periods and two groups equation (4b) is the difference of differences estimator.

A potential problem with the approach above is that it assumes that the composition effects from changes in participation can be fully accounted for by the additive time and group effects, $a_g + m_t$. Firstly changes in m_t will cause individuals to enter and leave the labor market. Second, with nonconvexities, tax reforms beyond the nontaxable allowance may lead to changes in participation. This will be particularly true if fixed costs are large relative to the nontaxable allowance.[5] The presence of composition effects is equivalent to saying that $E(u_{it}|P_{it}, g, t)$ is some general function of time and group and does not have the additive structure assumed in A1.1.

To control for the possibility that $E(u_{it}|P_{it}, g, t)$ may vary over time, we require structural restrictions. A parsimonious specification which we will use is to make the assumption of linear conditional expectation. We now extend A1.1 and A2.1 by assuming the following.

Assumption A1.2: $E(u_{it}|P_{it}, g, t) = a_g + m_t + \delta\lambda_{gt}$.

Assumption A2.2: $[D_w^{gt} - \delta_w \lambda_{gt}]^2 \neq 0$.

Here, λ_{gt} is the inverse Mill's ratio evaluated at $\Phi^{-1}(L_{gt})$, Φ^{-1} being the inverse function of the normal distribution and L_{gt} being the proportion of group g working in period t.[6] δ is a fixed but unknown parameter and δ_w is the (population) partial regression coefficient defined by $\delta_w = E[D_w^{gt} D_\lambda^{gt}]/E[D_\lambda^{gt}]^2$. Since we now have an extra parameter to estimate, we need an extra reform. Assumption A1.2 models the way composition changes affect differences in the

[5] Whether this is the case is hard to evaluate and depends heavily on where the woman lives and how good the public childcare provision is in the area.

[6] See Heckman (1974), Gronau (1974), and Heckman (1979).

observed labor supplies across groups. It implies that

$$(5) \qquad E(h_{it}|P_{it}, g, t) = bE(\ln w_{it}|P_{it}, g, t) + a_g + m_t + \delta\lambda_{gt}$$

where all expectations are over workers only. Assumption A2.2 states that wages must vary differentially across groups over time over and above any observed variation induced by changes in sample composition. We have also implicitly assumed that $E[D_\lambda^{gt}]^2 \neq 0$. If this is not the case, there is no selection bias on the coefficients of interest (here the wage effect) and we can simply use (4). Otherwise we can now estimate the wage effect using a generalization of (4), i.e.

$$(6) \qquad \hat{b} = \frac{\Sigma_g \Sigma_t \left[\tilde{h}_{gt} - \hat{\delta}_h \hat{\lambda}_{gt}\right]\left[\widetilde{\ln w}_{gt} - \hat{\delta}_w \hat{\lambda}_{gt}\right]/n_{gt}}{\Sigma_g \Sigma_t \left(\widetilde{\ln w}_{gt} - \hat{\delta}_w \hat{\lambda}_{gt}\right)^2/n_{gt}}$$

where $\hat{\lambda}_{gt}$ is an estimate of λ_{gt} and where the partial regression coefficients $\hat{\delta}_x(x = h, w)$ are defined by $\hat{\delta}_x = (\Sigma_g \Sigma_t x_{gt} \hat{\lambda}_{gt}/n_{gt})/(\Sigma_g \Sigma_t \hat{\lambda}^2_{gt}/n_{gt})$ and where n_{gt} is the number of observations in cell (g, t). As before this estimator can be implemented using a residual addition technique. We can add an estimate of λ_{gt} as well as the residual of the wage equation estimated on the sample of workers (with no correction for sample selection bias as implied by (5)) to an OLS regression of individual hours on individual wages, time dummies, and group dummies.

To determine whether (6) or (4) should best be used, we can test the null hypothesis that $E[D_\lambda^{gt}]^2 = 0$, which implies that the group effects (a_g) and the time effects (m_t) adequately control for any composition changes (given our choice of groups). If we do not reject this we can use (4).

The assumption in A1.2 is worth some discussion. First note that where all regressors are discrete and a full set of interactions are included in the selection equation, use of the normal distribution to compute $\hat{\lambda}_{gt}$ imposes no restrictions. However, the linear conditional expectation assumption implies that a term linear in $\hat{\lambda}_{gt}$ is sufficient to control for selection effects and is potentially restrictive. Using the results in Lee (1984) in general we have that

$$(7) \qquad E(u_{it}|P_{it}, g, t) = a_g + m_t + \sum_{k=1}^{K} \delta_k \lambda_{gt}^{(k)}$$

where $\lambda_{gt}^{(k)}$ are generalized residuals of order k. The linearity reduces the number of parameters to be estimated and hence the number of periods over which we require exogenous variability in wages. If it is found that $E[D_\lambda^{gt}]^2 \neq 0$, then one can experiment by including higher order generalized residuals after checking that they display sufficient independent variability.

3.2.1. *Allowing for Income Effects*

In general, income effects are important for labor supply and we need to take them into account for at least two reasons. First, the wage elasticity cannot in general be interpreted as an uncompensated wage elasticity, unless we control for other income. Second, income effects are important if we wish to compute compensated wage elasticities for the purpose of evaluating the welfare effects of tax reforms. It is straightforward to extend the estimator in (6) to allow for extra regressors, such as other income. This involves regressing $\tilde{h}_{gt} - \hat{\delta}_h \tilde{\lambda}_{gt}$ on $\widetilde{\ln w}_{gt} - \hat{\delta}_w \tilde{\lambda}_{gt}$ and $\tilde{\mu}_{gt} - \hat{\delta}_\mu \tilde{\lambda}_{gt}$ where μ is household other income. The rank condition for identification is now more stringent: It requires that the covariance matrix $V = Ez_{gt}z'_{gt}$ has full rank, where $z_{gt} = [D_w^{gt} - \delta_w \lambda_{gt}, D_\mu^{gt} - \delta_\mu \lambda_{gt}]'$. This is equivalent to requiring that the matrix of coefficients on the excluded exogenous variables in the reduced forms of log wage and other income, after taking into account composition effects, has rank 2. A necessary but not sufficient condition for this to be true is that these coefficients be nonzero in each of the reduced forms—i.e., that $E(D_w^{gt} - \delta_w \lambda_{gt})^2$ and $E(D_\mu^{gt} - \delta_\mu \lambda_{gt})^2$ be nonzero. As before if we accept the hypothesis that $E(D_\lambda^{gt})^2 = 0$ we need to consider whether the rank of $V^* = Ez^*_{gt}z^{*\prime}_{gt}$ is two, where $z^*_{gt} = [D_w^{gt}, D_\mu^{gt}]'$. In this case we estimate the model using the sample counterparts of z^*_{gt} as regressors.

3.3. *Discontinuities in the Budget Set*

The budget set in the UK up to 1989 had a major discontinuity at the level of earnings where individuals must start paying national insurance contributions. The contributions are payable on all earnings, leading to a drop in income at that point. In addition there is a kink in the budget set at the level of earnings beyond which individuals must start paying tax. Other kinks are unimportant for our sample. Both the tax kink and the NI discontinuity are close to each other in terms of earnings. The basic structure of the tax system is depicted in Figure 1. In the data there is evidence of bunching at the discontinuity.

There is a close link between the statistical coherency of labor supply models with nonlinear taxes and the integrability conditions (see Heckman (1978), Gourieroux, Laffont, and Monfort (1980), and MaCurdy, Green, and Paarsch (1990)). Nevertheless, imposing the integrability condition at the kinks within the context of a model with a limited number of parameters risks distorting the effects elsewhere in the budget constraint. Ignoring the issue is also a problem since the results may be uninterpretable from a preference point of view. Moreover, the wage effects would probably be biased downwards since for people on the kink we would attribute their inertia to preferences rather than to the structure of the budget constraint. To overcome the problem we need to increase the flexibility of the model (by adding extra parameters); the easiest way to achieve this is to condition out observations close (in a range of 5 hours) to the kink.[7] To correct for this potentially endogenous selection we include an additional selectivity term. This is the first order generalized residual from an ordered probit with three groups: the working non-taxpayers, those close to the kink, and those above the kink. Taking the λ_{gt} terms in (6) to be a vector associated with two stepwise regression coefficients, nothing else changes. We discuss the identification issues that arise and implementation of the extended estimator below.

3.4. *The Identifying Assumptions*

To identify our model we need to define the groups whose post-tax wages and other income have changed differentially over time. One might be tempted to split the sample up into taxpayers and non-taxpayers. However, this separation is probably invalid because under very general conditions the composition of the two groups will change over time in a nonrandom way, in response to tax reforms. In an interesting paper, Eissa (1994) applies the difference of differences estimator and compares the behavior of wives married to high earning husbands to that of wives of lower earning husbands. The two groups were affected differentially by the 1986 tax reform she was analyzing. Her approach requires that the composition of the two groups vis a vis preferences for work not change as a result of the reform. This imposes implicitly restrictions on behavior, since the household's position in the income distribution is to an extent endogenous.

We group the data based on the year of birth and the age the person left full time education, both interacted with the tax year. To make sure that the number of observations is large enough in each group/year cell we take four cohorts, each born in a ten year interval and only two education groups: Those who left education at the minimum legal age and those who continued beyond the minimum. The four cohorts consist of individuals born in 1930–1939, 1940–1949, 1950–1959, and 1960–1969 respectively. This defines eight groups. Our data extends over 15 financial years. Hence there are substantially more groups than parameters to estimate; this will allow us to construct a test of overidentifying restrictions.

The identifying assumption we make is that the average differences in labor supply (given the wage, other income, and demographics) between the groups we defined above be constant over time, as implied by Assumptions A1.1 or A1.2.[8] Hence, the identifying assumption does not require that the education choice be unrelated to preferences for hours of work or unrelated to the economic environment facing a cohort. It requires (i) the relationship of the unobservables

[7] This is exactly analogous to conditioning out nonworkers, which avoids modelling the participation decision. As discussed in Blundell, Duncan, and Meghir (1992), this may also account for the presence of "optimization" errors whose distribution has bounded and limited support.

[8] In practice this implies the exclusion of time-group interactions from the labor supply model. A full set of time effects and group effects are included.

and education can be described by a fixed group effect depending on education and cohort only and a time effect which is the same across groups; this is the meaning of equation (5); (ii) once 20 years old (when we start including individuals in the sample) individuals with just the statutory level of education cannot switch groups by returning to full time education. The proportion of workers who left school after the minimum age has increased over time. This is a cohort effect and partly reflects the increase in the statutory years of education. Within a cohort our education measure remains constant apart from sampling variability. To illustrate this we take a ten year cohort of individuals born from 1945 to 1954, which is observed over the entire sample time period and we regress the proportion of those in the cohort who had post-compulsory education (post 16 years of age) on a linear trend increasing by one each year. The coefficient is -0.0016 with a standard error of 0.0034. Hence our measure of the education level used for grouping did not change over the 15 year period for this cohort. This does not mean that workers do not join any training courses, only that these do not imply a change in group vis a vis our education classification; these courses are part time or, when full time, they are attended by individuals with post-compulsory education. Evidence from the 1958 NCDS cohort confirms this (see Blundell, Dearden, and Meghir (1996)).

The reason we expect the groups by which we classify individuals to be affected differentially by the tax reforms is because the cohort/experience effects on wages and other income (essentially husbands' earnings) and the returns to education ensure that the wage and other income distribution will be different across groups. Moreover, the substantial increase in the education returns over these years provides another important source of identifying information.

Finally it is possible that the government was targeting taxation so as to exploit the increased returns to education. If this did happen, it could reduce the explanatory power of the instruments since the two effects would counteract each other. This does not seem to be the case, since taxation at the top of the income distribution fell relatively to the bottom, rather than increased.

In any case all these arguments call for a careful evaluation of the relevance and validity of our instruments. We discuss the way we do this below.

At this point, however, it is worth considering what distinguishes equation (1), a within period marginal rate of substitution equation, from an intertemporal Euler condition once we include a full set of time dummies, given the latter will be colinear with a common real interest rate. In this case identification requires that interest rates differ across individuals as well as time and that this variation is correlated with the education/cohort indicator. This will be true both because the relevant after tax interest rates are different and because of liquidity constraints. Thus the exclusion restriction distinguishing equation (1) from an intertemporal Euler condition can be thought of as the average group interest rate; this is assumed to vary over time and across groups. Such variation in the interest rate can be induced by the tax system as well as by liquidity constraints.

3.5. *Household Composition and Labor Supply*

In the labor supply model we include household composition variables. These are dummies that point to the age band of the youngest child in the family. The age bands for the children are 0–2 ($DK02$), 3–4 ($DK34$), 5–10 ($DK510$), and 11 + ($DK11+$).

Potentially, demographics could be used like the other grouping instruments; the discrete demographic variables could be interacted with the other group indicators to form cells of data. The resulting cells would be too small; i.e., we would have an excessively large number of instruments relative to sample size, which would lead to overfitting in the reduced forms. Thus we restrict the reduced forms to include the set of demographic characteristics linearly. This is equivalent to imposing cross cell restrictions in computing the grouped averages. Finally, we assume that the relationship of demographics and labor supply is constant over time.

3.6. *The Relevance and Validity of the Instruments*

A number of recent papers have discussed the adverse effects of using weak instruments (see Staiger and Stock (1997) and Bound et al. (1995)) as well as invalid exclusion restrictions. Thus, we need to evaluate whether indeed the post tax wages and other incomes of the various groups (defined by cohort and education) do change differentially over time. In practice this amounts to evaluating the rank of the matrix of reduced form coefficients on the excluded cohort/education/time interactions. This is after accounting for the time effects, group effects, and demographics that are included in the labor supply equation. We also need to evaluate the validity of these overidentifying restrictions.

To evaluate the rank of the coefficient matrix on the excluded instruments in the reduced form, we use the extension of Anderson's (1951) eigenvalue-based test provided by Robin and Smith (1994). That is, let $\hat{\boldsymbol{\Pi}}$ be a consistent and asymptotically normal estimator of a $p \times k$ $(p \geq k)$ reduced form parameter matrix $\boldsymbol{\Pi}$ on the excluded instruments (i.e. there are k endogenous variables and p excluded instruments). Let $\boldsymbol{\Omega}$ be the covariance matrix of $\sqrt{N}\,\mathrm{vec}(\hat{\boldsymbol{\Pi}})$ where N is the sample size. Assume that $\boldsymbol{\Omega}$ is full rank. Define $\hat{\tau}_1 \geq \cdots \geq \hat{\tau}_k$ to be the eigenvalues of $\hat{\boldsymbol{\Pi}}'\hat{\boldsymbol{\Pi}}$. Under the null hypothesis that the rank of the matrix $\boldsymbol{\Pi}$ is r, the smallest $k-r$ eigenvalues should be zero. Robin and Smith show that under this null, $N\sum_{i=r+1}^{k}\hat{\tau}_i$ has for limiting distribution a mixture of $(p-r)(k-r)$ one-degree-of-freedom chi-square distributions. The weights can be computed as the nonzero ordered characteristic roots of the matrix $(\mathbf{D}'_{k-r} \otimes \mathbf{C}'_{p-r})\boldsymbol{\Omega}(\mathbf{D}_{k-r} \otimes \mathbf{C}_{p-r})$, where $\mathbf{D}_{k-r}$ (respectively $\mathbf{C}_{p-r}$) is a $k \times (k-r)$ matrix (respectively $p \times (p-r)$) formed by the eigenvectors corresponding to the k lowest eigenvalues of $\hat{\boldsymbol{\Pi}}'\hat{\boldsymbol{\Pi}}$ (respectively $\hat{\boldsymbol{\Pi}}\hat{\boldsymbol{\Pi}}'$).

We first evaluate whether the effects of changes in participation and selection away from the NI kink can be explained individually and jointly by the group and time effects. To preempt, we find that this is the case for participation, while for the selection away from the kink the test is borderline. We then evaluate whether the matrix of reduced form coefficients on the excluded interactions in the log wage and other income equations is rank two. We then add the term relating to the selection away from the kink and consider the case for rank three, in the reduced form coefficient matrix for the log after-tax wage, other income, and the ordered probit.

This rank test procedure can also be used to construct a test of overidentifying restrictions: Suppose there are $k+1$ endogenous variables, including the left-hand side. If we add to the set of endogenous variables the "left-hand-side one" (labor supply here), then the rank of the reduced form coefficients on the excluded instruments must be no more than k. A test of the null hypothesis that the rank is in fact k against the hypothesis that it is $k+1$ is the test of overidentifying restrictions that we present. We present the most stringent version of the test where we test for rank two against rank three in the reduced form labor supply, other income, and log wage equations.

3.7. *Implementation of the Estimator*

First the four reduced forms are estimated on the individual data. In the reduced forms the right-hand-side variables include a complete set of group and time interactions as well as linearly the demographic variables DK_{it}. The estimation sample for the log wage and other income equations excludes nonworkers and those working within five hours from the NI or "basic rate of tax" kink. The participation probit is estimated on the entire sample. Finally an ordered probit is used to correct for selection away from the tax and NI kink. This is estimated on the entire sample of workers. For this reduced form, workers are classified as those below the kink, those in close range to the kink, and those above. The reduced forms are used to evaluate the relevance of the instruments.

The labor supply equation is then estimated using OLS on

$$\begin{aligned} h_{it} &= a_g + m_t + \theta' DK_{it} + \beta \ln w_{it} + \gamma \mu_{it} \\ &\quad + \delta^w \hat{\nu}_{it}^w + \delta^\mu \hat{\nu}_{it}^\mu + \delta^P \hat{\nu}_{it}^P + \delta^T \hat{\nu}_{it}^T + e_{it}, \end{aligned} \tag{8}$$

where a_g are group dummies, m_t are time dummies, DK_{it} are the demographic variables, and $\ln w_{it}$ and μ_{it} are the individual level of log after-tax wages and other income ($consumption_{it} - w_{it} h_{it}$). The $\hat{\nu}$ are the residuals from reduced forms to control for the endogeneity of wages ($\hat{\nu}_{it}^w$), other income ($\hat{\nu}_{it}^\mu$), participation ($\hat{\nu}_{it}^P$, an inverse Mill's ratio), and selection away from the tax and NI kink ($\hat{\nu}_{it}^T$, a generalized residual from an ordered probit). This computational approach gives identical results to grouping but provides directly tests of exogeneity; these are the t statistics on the δ parameters (see Smith and Blundell (1986)).

We estimate a version of the model that allows β and γ to vary with demographic composition. This is estimated simply by adding to equation (8) the interactions of log w and μ with the demographic characteristics described earlier.

The average group cell size is 142 observations and we exclude nine cells with less than 50 observations that occur at the higher ages. The estimator of the asymptotic covariance matrix that we use accounts for the generated regressors (the residuals) and for heteroskedasticity. Moreover, even after time and group effects are controlled for, it is still possible that there remains some limited intragroup correlation in the unobservables. Our estimate of the asymptotic covariance matrix takes this into account. The details of the computation of the covariance matrix are provided in Appendix B.

4. LABOR SUPPLY RESPONSES

4.1. *The Data*

The data are drawn from the repeated cross sections of the UK Family Expenditure Survey (FES) for the years 1978–1992 and consist of married or cohabiting women in the age range 20–50, whose husbands/partners are employed.[9] The survey is continuous and individuals are uniformly distributed across all months of the year. There are 24626 women of which 16781 work. Of these 2970 are within five hours of the NI or tax kink. A brief summary of the data is presented in Table XV of Appendix A. Hours of work are "usual weekly hours, including usual overtime" and the pre-tax wage is constructed by dividing "usual weekly earnings, including usual overtime pay" by "usual weekly hours, including usual overtime." Note that expenditures are not deductible for tax purposes, which makes the calculation of marginal tax rates much more straightforward in the UK, since the earnings allowance is known explicitly. Finally for consumption we use total weekly nondurable household consumption.

In Figure 2 we show a histogram of hours of work by taxpayer status as well as overall. This shows that there is indeed a great deal of variability to be explained. A possible implication of this is that there is ample opportunity for women who wish to change their hours of work to do so. In Figure 3 we show the evolution of average hours for the workers which shows the aggregate number of hours per week does vary substantially.

More to the point though, in Figure 4 we plot the difference of hours worked by the taxpayers to those worked by the non-taxpayers. This shows a marked

[9] In principle married and cohabiting couples face the same budget constraint. The FES distinguishes between the two types of households only after 1988.

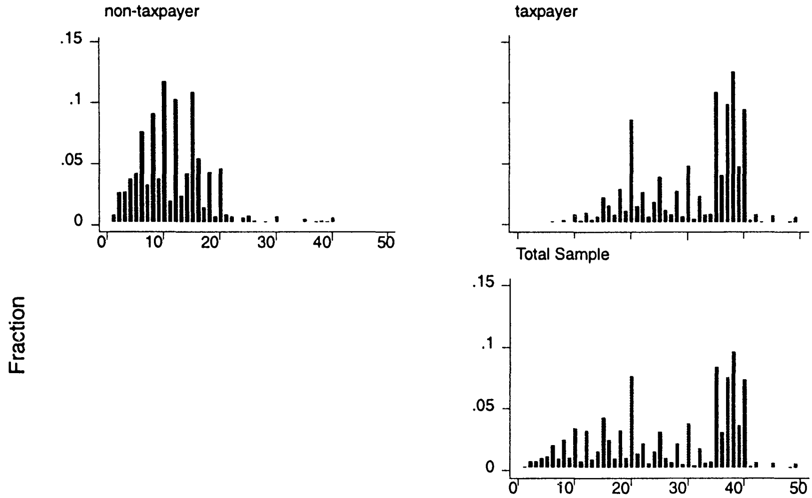

FIGURE 2.—Hours of work by taxpayer status.

decline. We can compare this to the time series pattern of the difference in the after-tax log wage between taxpayers and non-taxpayers. Figure 5 shows that taxpayers' after-tax relative wages have increased quite impressively as we would expect given the increase in wage dispersion. The implied wage effect on hours worked is equivalent to an estimate obtained from a simple difference of

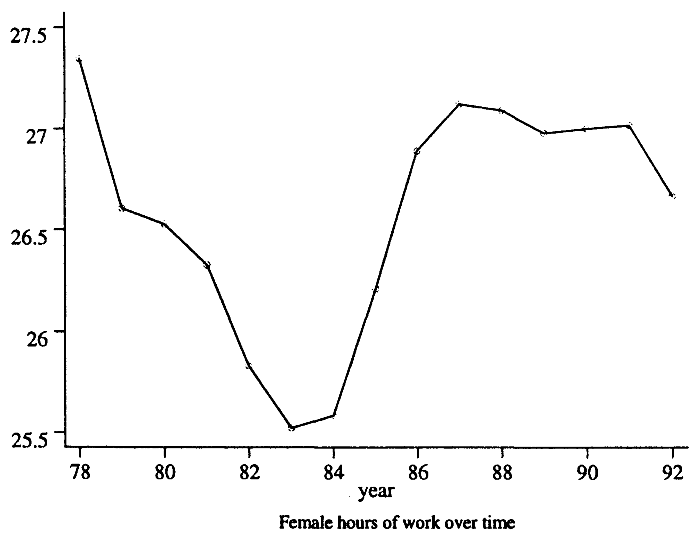

FIGURE 3

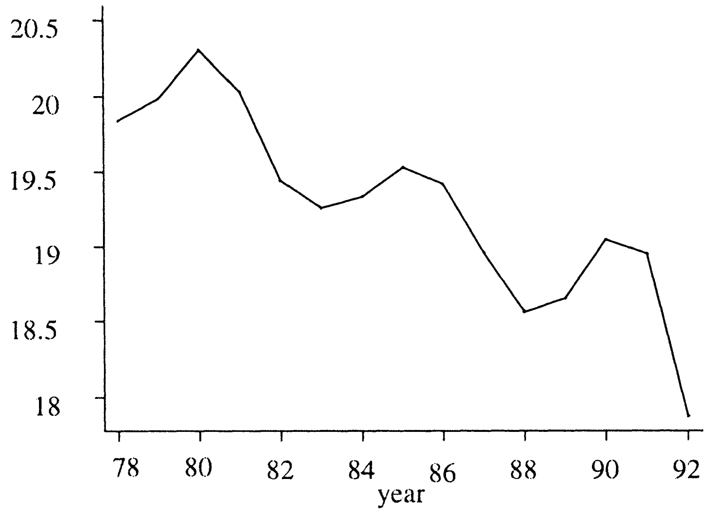

FIGURE 4.—Differences of female hours of work between taxpayers and non-taxpayers over time.

differences estimator where the groups being compared are the taxpayers and the non-taxpayers. This wage effect on labor supply is negative (reported in detail later). Nevertheless such an inference is only justified if we can assume that the composition of the two groups, vis a vis tastes for work has remained constant over time. We return to this below.

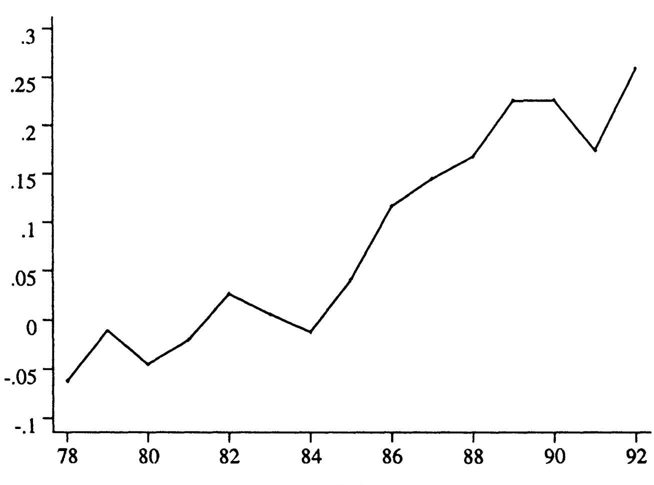

FIGURE 5.—Differences in female log wages between taxpayers and non-taxpayers over time.

4.2. *The Reduced Forms and the Validity of Instruments*

Before we present any further results we report on the relevant rank tests. The reduced forms are presented in Appendix A. We first test the null hypothesis that each of the endogenous variables has not been changing differentially over time across education and cohort groups. The p-values for the four individual reduced forms are given in Table III in percentage terms. The reduced forms are presented in the Appendix.

This null hypothesis is clearly rejected for the wage and other income variables, indicating that the instruments are indeed highly significant for these two variables. The p-values for the other two equations (participation and ordered probit) are somewhat high. Nevertheless, in the participation equation, a number of interaction effects for the older low educated cohorts in particular are significant. The hypothesis that the rank of the reduced form coefficient matrix (on the excluded instruments) for these two latter reduced forms (participation and ordered probit) is zero has a p-value as high as 36.8% (based on the Robin-Smith test). In contrast, the hypothesis that the rank of the reduced form coefficient matrix for other income and the log wage is one is 0.63%, implying rank two as required for identification. Finally, the rank test for rank two against three in the reduced form coefficient matrix obtained from the log wage, other income, and the ordered probit (where the individual p-value is relatively low) is 8.0%.

These results can be interpreted as follows: The composition effects due to changes in participation are explainable by the included time and group effects (i.e. the cohort education indicators and the demographic variables). The result for the composition effects due to selection away from the kink are borderline. On the other hand, the excluded instruments have very strong explanatory power jointly for the after-tax wage and other income variable. These effects are clearly very well identified by our instruments. From our theoretical analysis the implication is that we can ignore the corrections for selection into work and (possibly) around the kink, in identifying the wage and income effects. There is however a question mark as to whether we can really drop the correction for selection away from the kink. The p-value for the excluded instruments in that reduced form is 1.4% and the rank test is not far from rejecting the rank two hypothesis in favor of rank three (p-value 8%). Thus we also present estimates which include corrections for participation and exclusion of individuals on the tax/NI kink.

TABLE III

p-VALUES FOR THE SIGNIFICANCE OF THE EXCLUDED INSTRUMENTS IN THE REDUCED FORMS

Log Wage	Other Income	Participation	Ordered Probit
0%	0.038%	10.4%	1.4%

Finally we also carried out the test of the overidentifying restrictions as described in Section 3.6. The overidentifying restrictions arise from the fact that we have a larger number of groups multiplied by time periods than parameters to estimate. It tests for the absence of time group interactions from labor supply over and above the number of exclusions needed for exact identification. The test has a p-value of 0.9% which is quite acceptable.

4.3. *Labor Supply Elasticities and Parameter Estimates*

We organize the presentation of the remaining results as follows. First we present a table of elasticities relating to the model including all endogeneity corrections which also allows the coefficients to vary by demographic group. We then show parameter estimates with and without interaction effects for demographics. Here we perform a sensitivity analysis where we assume that participation and the selection around the kink are exogenous, we compare the results to what happens when we include individuals around the kink and finally we compare the results to ones obtained by OLS. All standard errors have been corrected for generated regressor bias and for heteroskedasticity induced by including the generalized residuals, as well as for intra-group dependence as described in Appendix B.

4.3.1. *The Elasticities*

To start off, in Table IV we present the elasticities implied by the estimates in Table V presented later. All wage elasticities are positive and highest for women with children at pre-school age, as we would expect. The income elasticities are all negative, except for those women with no children, where it is zero. As a result the compensated wage effects, which matter for welfare, are all positive

TABLE IV

ELASTICITIES: GROUPING INSTRUMENTS: COHORT AND EDUCATION

	Wage	Compensated Wage	Other Income	Group Means: Hours	Group Means: Wage	Group Means: Income
No Children	0.140	0.140	0.000	32	2.97	88.63
	(0.075)	(0.088)	(0.041)			
Youngest Child 0–2	0.205	0.301	−0.185	20	3.36	129.69
	(0.128)	(0.144)	(0.104)			
Youngest Child 3–4	0.371	0.439	−0.173	18	3.10	143.64
	(0.150)	(0.159)	(0.139)			
Youngest Child 5–10	0.132	0.173	−0.102	21	2.86	151.13
	(0.117)	(0.127)	(0.109)			
Youngest Child 11 +	0.130	0.160	−0.063	25	2.83	147.31
	(0.107)	(0.117)	(0.084)			

Note: Asymptotic standard errors in parentheses.

and the model is consistent with standard theory everywhere in the data. As we report below these elasticities are lower than some recent U.S. estimates, although the latter relate to annual hours of work. In any case our substitution effects imply that taxation does have efficiency costs since taxation will cause substitution leading to reductions in hours of work. Moreover this is only part of the story: Taxation may have important participation effects and corresponding welfare effects. Looking at hours of work is not sufficient to evaluate this, because of fixed costs of work. We now evaluate the robustness of these results.

4.3.2. *The Parameter Estimates*

In Table V we present the parameter estimates from six different specifications for the model including interactions with demographic variables. In Table VI we present the same sets of models but with demographics included only in the intercept. All specifications include a full set of time dummies and group dummies (cohort/education indicators fully interacted). Indicatively we present a set of cohort education and time effects in the Appendix, following the reduced form tables. These correspond to column (i) of Table VI. Age effects are accounted for by the combination of time and cohort effects. It is straightforward to compute the corresponding elasticities to compare with the results in Table IV by using the group means for hours, the wage, and other income reported in that table.[10] The coefficients are presented in three groups: the intercept coefficients, followed by the wage effects for each demographic group, followed by the other income effects.

In the first column we correct for the endogeneity of the wage rate, other income, participation, and selection away from the kink. In the next column we drop the correction for participation, since the rank test suggested that the changes in participation can be controlled by the included group and time effects. As expected, the results between these two columns are virtually identical. In the third column we also drop the correction for selection away from the NI/tax kink. The wage elasticities do become somewhat smaller but the effects are not dramatic. In contrast, when we use OLS in the fourth column the results are completely different; the implied wage elasticities become negative and the income elasticities larger in absolute value. This reflects the large and negative coefficient on the wage residual in the first three columns; we return to this below.

As already noted the standard errors are corrected for heteroskedasticity, generated regressors, and intra-group dependence, over and above that accounted for by the group effects. It turns out that the latter correction has large effects. For example, the standard error for the wage effect for women without children in column (i) of Table V is increased from 2.086 to 2.390 as a result of

[10] The wage elasticity is the coefficient on the log wage divided by hours of work and the other income elasticity is the coefficient on other income divided by hours of work and multiplied by other income.

TABLE V

PARAMETER ESTIMATES—GROUPS DEFINED BY COHORT AND EDUCATION

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Constant	33.147	33.339	32.261	40.947	29.635	29.558
	3.439	*3.362*	*3.022*	*0.693*	*3.843*	*3.280*
DK02	−11.797	−11.684	−12.055	−10.138	−11.394	−11.424
	1.971	*1.939*	*1.916*	*1.706*	*1.754*	*1.645*
DK34	−15.960	−16.012	−16.597	−15.048	−15.412	−15.402
	2.217	*2.214*	*2.168*	*1.941*	*1.812*	*1.701*
DK510	−8.466	−8.531	−9.196	−8.132	−9.242	−9.231
	1.381	*1.364*	*1.240*	*0.897*	*1.410*	*1.300*
DK11O	−3.164	−3.183	−3.889	−3.198	−3.808	−3.810
	1.187	*1.180*	*1.086*	*0.991*	*1.165*	*1.074*
Wage Effects						
No Children	4.493	4.579	2.795	−2.377	4.196	4.155
	2.390	*2.364*	*2.082*	*0.400*	*2.745*	*2.336*
DK02	4.105	4.110	2.976	−2.148	1.766	1.749
	2.558	*2.531*	*2.267*	*1.134*	*2.809*	*2.419*
DK34	6.686	6.739	5.467	1.314	4.185	4.158
	2.707	*2.683*	*2.405*	*1.109*	*2.912*	*2.533*
DK510	2.777	2.841	1.520	−3.661	1.338	1.309
	2.448	*2.426*	*2.178*	*0.606*	*2.781*	*2.383*
DK11 +	3.260	3.337	1.992	−3.230	2.308	2.275
	2.685	*2.664*	*2.430*	*0.655*	*3.001*	*2.629*
Other Income						
No Children	0.000	0.000	0.013	−0.008	0.018	0.018
	0.015	*0.015*	*0.013*	*0.001*	*0.015*	*0.013*
DK02	−0.028	−0.028	−0.016	−0.037	−0.004	−0.004
	0.016	*0.016*	*0.014*	*0.005*	*0.016*	*0.014*
DK34	−0.022	−0.021	−0.008	−0.030	0.002	0.002
	0.017	*0.017*	*0.016*	*0.009*	*0.016*	*0.015*
DK510	−0.014	−0.014	−0.001	−0.023	0.010	0.011
	0.015	*0.015*	*0.013*	*0.003*	*0.015*	*0.013*
DK11 +	−0.011	−0.010	0.002	−0.019	0.009	0.009
	0.014	*0.014*	*0.012*	*0.003*	*0.014*	*0.012*
Residuals						
Wage	−6.699	−6.758	−5.246		−7.435	−7.405
	2.482	*2.455*	*2.204*		*2.820*	*2.426*
Other Income	−0.008	−0.009	−0.021		−0.029	−0.029
	0.015	*0.015*	*0.013*		*0.015*	*0.013*
Tax Kink	0.336	0.321				
	0.082	*0.083*				
Participation	0.258				−0.071	
	0.450				*0.347*	

Note: Asymptotic standard errors in italics. Complete set of cohort/education and time dummies included.

the correction for intra-group dependence; the standard error for the wage effect for women with the youngest child aged 3-4 increases from 2.173 to 2.707.

We argued earlier that leaving individuals on or close to the kink in the data would reduce the elasticities, since those who are on the kink are less likely to react to policy changes according to the labor supply model. The effects of including the entire sample and ignoring this nonlinearity can be seen by comparing columns one and five (or two and six which do not include corrections for participation) of Table V. When we use the entire sample the coefficients on the wage rate are always lower, but the effects are more marked for women with young children, who tend to work a low number of hours and hence are more likely to be close to the kink. Finally we can repeat the comparison using column (iii), where we exclude the observations close to the kink, but we do not correct for this selection. There we find marginally larger effects for women with children when we exclude the observations close to the kink vis a vis the comparable column (vi). For women without children the elasticity is larger when we include all the sample points. Even if we take this comparison to be credible, we should note that childless women rarely work so few hours as to be affected by the NI discontinuity.

In Table VI we report the same set of experiments but excluding the interaction effects with demographics. The intercept of the model contains, as before, a full set of time and group dummies as well as the demographic characteristics. The same broad conclusions follow. In particular, the OLS results imply negative wage elasticities and larger other income elasticities. When we use the entire sample the wage elasticity is much smaller when we

TABLE VI

ESTIMATES WITH NO DEMOGRAPHIC INTERACTIONS

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Constant	34.551	34.630	33.213	41.661	31.687	31.800
	3.386	*3.324*	*2.947*	*0.689*	*4.299*	*3.182*
DK02	−15.221	−15.211	−14.953	−13.079	−16.499	−16.492
	1.200	*1.200*	*1.137*	*0.509*	*1.397*	*1.074*
DK34	−16.033	−16.061	−16.112	−14.622	−16.945	−16.977
	1.214	*1.185*	*1.099*	*0.490*	*1.381*	*1.046*
DK510	−11.746	−11.774	−11.997	−11.025	−12.776	−12.805
	1.091	*1.067*	*0.971*	*0.325*	*1.233*	*0.945*
DK11O	−5.433	−5.443	−5.706	−5.118	−6.624	−6.632
	0.883	*0.875*	*0.794*	*0.347*	*1.039*	*0.810*
Log Wage	4.254	4.273	2.635	−2.446	2.851	2.894
	2.349	*2.341*	*2.054*	*0.346*	*3.062*	*2.265*
Other Income	−0.010	−0.010	0.004	−0.016	0.009	0.009
	0.015	*0.015*	*0.013*	*0.001*	*0.017*	*0.013*
Residuals						
Wage	−6.779	−6.795	−5.153		−7.371	−7.410
	2.405	*2.396*	*2.135*		*3.113*	*2.334*
Other Income	−0.006	−0.006	−0.020		−0.026	−0.026
	0.015	*0.015*	*0.013*		*0.017*	*0.013*
Tax Kink	0.337	0.332				
	0.076	*0.075*				
Participation	0.083				0.092	
	0.436				*0.356*	

Note: Asymptotic standard errors in italics. Complete set of cohort/education and time dummies included.

correct for the selection (columns (i) and (ii) compared to (v) and (vi) respectively) but virtually the same when compared to the case when we exclude the observations without correcting for the selection (compare columns (iii) with (vi)).

We now return to what feature of the data leads to the OLS results being so different from the IV ones. To understand whether the difference from the OLS results originates primarily from the endogeneity of the pre-tax wage or from differential changes in the composition of the taxpaying group we re-estimate (1) including as a grouping instrument taxpayer status. The model includes a full set of time effects and group effects, where the groups here are defined by education, cohort, and taxpaying status. The estimator is a difference of differences estimator with control group the non-taxpayers and treatment group the taxpayers.[11] In order to keep the cell sizes comparable to the previous results we aggregate the four date-of-birth cohorts we use to two larger cohorts. These estimates include no corrections for participation or for selection on the kink. We also do not include any demographic interactions; the model is most comparable to the results presented in Table VI. The results are presented in Table VII.

The estimates are very similar to those obtained by OLS which implies that the main source of endogeneity is in fact the changing composition of the taxpaying group. The estimates are a reflection of Figures 4 and 5.

To interpret the results, note that in Table VII although we control for the endogeneity of individual pre-tax wages by grouping, we assumed that taste differences between taxpayers and non-taxpayers can be modelled as a group fixed effect and time effects; the basic difference between the results in Tables VII and VI (columns (i) and (ii) and (iii)) is that in the latter we allow for changes in taste composition between the two groups over time. Why might this be important?

Figure 6 shows how female participation rates have changed over time. After 1982 there is a rapid increase in the proportion of women working. At the same time the proportion of women paying tax has varied substantially. In Figure 7 we

TABLE VII

USING NON-TAXPAYERS AS A CONTROL GROUP

Wage		Other Income	
Coeff	Elasticity[1]	Coeff	Elasticity[1]
−2.877	−0.115	−0.0147	−0.0764
1.122	*0.0449*	*0.0069*	*0.0359*

[1] Elasticities evaluated at 25 hours and £130.00 other income.

[11] As a referee pointed out we would ideally have liked to compare the labor market trends of these two groups during a period of a stable wage structure and the absence of reforms. The UK does not offer us such a chance since during the 70's a number of income policies, designed to compress wages, were implemented, as well as other tax reforms.

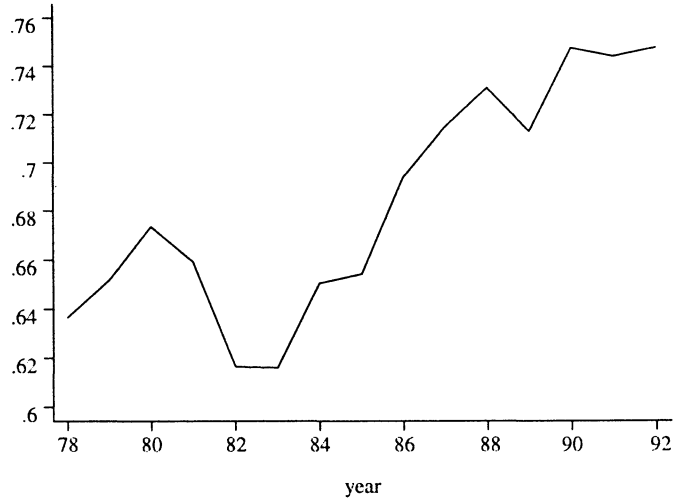

FIGURE 6.—Female participation over time.

show that this proportion fell quite dramatically up to 1984 but rose fast thereafter. This is partly a reflection of the effect of the reforms. Thus for example in 1983/84 there was a large increase in the nontaxable allowance. If, in addition, women entering the labor force in the 1980's are relatively well paid part-timers, as is considered to be the case, the average unobserved taste for work will be falling among the taxpaying group. This would be consistent with the decline of relative hours for the taxpaying group as shown in Figure 4 and

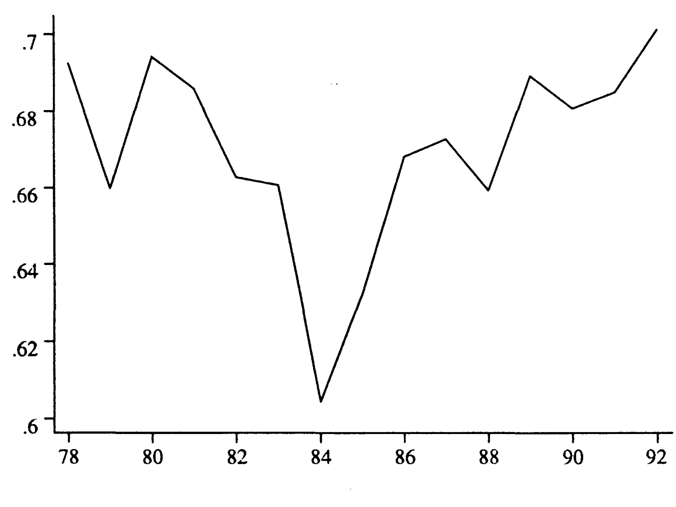

FIGURE 7.—Proportion of taxpayers over time.

leads to the negative wage elasticity in Table VII as well as for OLS. Regrouping the data by groups whose composition cannot change (date of birth and whether statutory education was received) reverses the results and reveals moderate but positive substitution elasticities as well as negative income effects for women with children.

4.3.3. *Sensitivity to the Number of Instruments and to Restrictions of Time Effects*

We conclude our analysis by carrying out some further sensitivity analysis vis a vis the number of instruments. For brevity we present results based on the model with no interactions of demographics. All the following experiments include all four residuals.

In the first experiment we reduce the number of instruments by using the prevailing value of five selected tax parameters interacted with the cohort/education indicators; this is instead of using time dummies in these interactions. We still include a full set of time dummies and cohort/education indicators additively in the reduced forms and the labor supply function. The tax parameters we use are: the basic tax rate, the higher tax rate, the VAT rate (value added tax, i.e. the indirect tax rate), the NI rate, and the nontaxable earnings allowance. The effect of using these instruments is not only to increase the number of observations per cell but also to increase the weight given to the tax reforms relative to the changing wage structure in identifying the labor supply effects. The results are very similar to what was obtained before: The wage and other income elasticity evaluated at the means is given in the first row of Table VIII. The wage elasticity is still quite high but the income effect is effectively zero.

In the next experiment we restrict the time effects both in the reduced forms and in the labor supply equation to be a cubic time trend. As with our earlier results we include in the reduced form a full set of time effects interacted with cohort/education indicators. This effectively improves the precision of the

TABLE VIII

ELASTICITIES WITH ALTERNATIVE INSTRUMENT SETS AND EXCLUSION RESTRICTIONS

	Wage		Other Income	
	Coeff	Elasticity[2]	Coeff	Elasticity[2]
Tax parameters	4.540	0.182	0.00036	0.002
as instruments	*2.426*	*0.094*	*0.014*	*0.082*
Cubic Trends	5.732	0.229	0.008	0.042
	2.252	*0.090*	*0.013*	*0.078*
No Time Effects[1]	3.163	0.127	−0.016	−0.083
	1.537	*0.061*	*0.014*	*0.085*
No Time Effects &	8.680	0.347	−0.063	−0.330
no cohort/educ[1]	*1.137*	*0.045*	*0.012*	*0.072*

[1] Includes age, age^2, and a dummy for age > 40.
[2] Elasticities evaluated at 25 hours and £130.00 other income.

explanatory power of the excluded instruments. The wage coefficient is larger and the income effect essentially zero.

In the last two experiments we assess the effect of excluding the time effects. When we do this we do have to control for age, since there are important life-cycle effects of age on hours worked. In the first of these two experiments we still include the full set of cohort education interactions. This implicitly means that the time effects are constrained and not completely suppressed. The wage elasticity becomes somewhat smaller and the income effect remains very small, but this time negative as in most of our earlier results. In the last row we exclude the cohort/education effects. This makes the approach similar to traditional cross section studies, such as those reviewed by Mroz (1987) except that the data contain a large number of time periods. Both the other income and wage elasticities now become much larger. The result is very similar to those reported in Arellano and Meghir (1992) where education is used as an identifying instrument.

Two papers known to us use broadly comparable methods although they are different in a number of respects. One is Angrist (1991); he groups PSID data on annual hours worked for a number of years but does not distinguish cross-sectional groups. He interprets his elasticities as intertemporal ones which are always at least as large as the within period ones which we report. He finds an elasticity of 0.634. When using OLS he finds −0.063. These results are consistent with ours. The other paper, by Eissa (1994), evaluates the effects of the 1986 tax reform on female labor supply. Her reported wage elasticities are at least 0.6 and some higher. She also considers the participation effects to derive a total elasticity. Thus our elasticities for weekly hours in the UK are lower than some of those estimated recently in the U.S. It is important however to emphasize that our paper differs from these studies in important methodological respects as well as in the hours measure we use. Tracing the precise reason for the differences in the estimates is an interesting project.

5. CONCLUSIONS

The aim of this paper is to investigate the responsiveness of labor supply to exogenous changes in wage rates and nonlabor income. To estimate the model we use for our basis the numerous tax reforms of the 1980's whose effect at different times was both to raise and to reduce marginal tax rates. Moreover at the same point in time taxes went up (or down) for some individuals but remained unaffected for others. In addition there have been important changes in the dispersion of pre-tax wages leading to further variation over time in after-tax wages. These changes seem to form an ideal setting for identifying labor supply responses and appear to avoid the need for hard-to-justify exclusion or exogeneity restrictions.

We develop extensions to the difference of differences estimator that account for the effects of changes in labor force composition and for the effects of the discontinuity in the British tax system. Our estimates are based on comparing the evolution of post-tax wages, other income, and hours, of different date-of-birth cohort and education groups. These groups will have been affected differently by the reforms because they occupy different points in the income distribution. Moreover, the increase in wage dispersion favored some groups more than others. The reforms and the change in dispersion affect both after-tax wages and other income since the latter is comprised to a great extent of husband's earnings. We illustrate the explanatory power and validity of the grouping instruments using rank tests.

Using our approach we show that wage elasticities are positive and moderately sized. Other income elasticities are quite small and for women without children these are zero. The OLS results are very different from IV, implying negative wage elasticities. We trace the cause of this discrepancy to changes in the composition of the taxpayer group over time. On the other hand, we find that changes in labor force participation can be explained by common time effects across all groups. Once these are included in the model no further correction is necessary. Our results are very robust to a number of restrictions on the instrument set which effectively increases the number of observations per cell. In particular when we use the values of five key parameters of the tax system as instruments, interacting these with the cohort/education dummies, we find virtually the same results as when we use time dummies in these interactions. Our conclusion is that major tax reform should take into account behavioral effects since our compensated substitution elasticities suggest that the welfare effects are not negligible.

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APPENDIX A: THE REDUCED FORMS AND DESCRIPTIVE STATISTICS

In Tables IX, X, XI, and XII we present the reduced forms used in estimation. In each table the first row is the cohort education/effect and the first column is the time effect. To obtain the predicted wage (say) the group effect is added to the time effect and to the number in the cell which represents the interaction effect. Thus the predicted log wage for a low education individual born 1950–1959 in 1981 is $1.316 - .351 - 0.061 = 0.904$ (Table IX). For the participation probit and the ordered probit these calculations provide index values which need to be converted to probabilities using the normal distribution. For the ordered probit the thresholds are given at the bottom of the table. Finally the linear demographic effects are presented at the bottom of each reduced form.

TABLE IX

REDUCED FORM FOR THE LOG WAGE

	Time Effects	Compulsory Schooling Year of Birth < 1940	1940–49	1950–59	1960 +	Post Compulsory Year of Birth < 1940	1940–49	1950–59	1960 +
Group Effect:	Effects	1.279 (0.059)	1.145 (0.033)	1.316 (0.024)	1.130 (0.022)	1.749 (0.075)	1.608 (0.057)	1.544 (0.022)	1.343 (0.024)
Year = 78	−0.501 (0.029)	0.035 (0.069)	0.275 (0.050)	E	·	0.133 (0.092)	0.142 (0.074)	E	·
Year = 79	−0.444 (0.028)	0.043 (0.068)	0.239 (0.049)	E	·	−0.032 (0.096)	0.136 (0.074)	E	·
Year = 80	−0.387 (0.029)	0.033 (0.069)	0.210 (0.049)	E	·	0.011 (0.093)	0.161 (0.074)	E	·
Year = 81	−0.351 (0.033)	−0.055 (0.072)	0.153 (0.053)	−0.061 (0.041)	E	·	−0.056 (0.074)	E	·
Year = 82	−0.306 (0.033)	−0.121 (0.072)	0.148 (0.053)	−0.038 (0.042)	E	·	0.057 (0.076)	E	·
Year = 83	−0.243 (0.033)	−0.086 (0.073)	0.137 (0.054)	−0.091 (0.043)	E	·	−0.028 (0.077)	E	·
Year = 84	−0.264 (0.032)	−0.042 (0.072)	0.198 (0.054)	−0.041 (0.041)	E	−0.175 (0.101)	0.057 (0.078)	E	·
Year = 85	−0.286 (0.038)	−0.047 (0.076)	0.224 (0.058)	0.026 (0.049)	E	·	0.040 (0.081)	0.096 (0.049)	E
Year = 86	−0.209 (0.033)	−0.017 (0.074)	0.101 (0.056)	−0.003 (0.045)	E	·	0.165 (0.078)	0.142 (0.045)	E
Year = 87	−0.139 (0.032)	−0.043 (0.075)	0.074 (0.056)	−0.066 (0.044)	E	·	0.121 (0.078)	0.094 (0.044)	E
Year = 88	−0.080 (0.031)	−0.103 (0.075)	0.164 (0.055)	−0.050 (0.043)	E	·	0.073 (0.076)	0.090 (0.045)	E
Year = 89	−0.027 (0.030)	−0.106 (0.075)	0.105 (0.053)	−0.082 (0.043)	E	·	−0.045 (0.079)	−0.016 (0.044)	E
Year = 90	−0.032 (0.030)	−0.172 (0.077)	0.092 (0.054)	−0.094 (0.044)	E	·	−0.021 (0.074)	−0.052 (0.045)	E
Year = 91	0.069 (0.029)	−0.231 (0.077)	−0.006 (0.053)	−0.177 (0.043)	E	·	−0.099 (0.077)	−0.018 (0.044)	E
Child Aged:		0–2	3–4	5–10	11 +				
		0.046 (0.011)	−0.020 (0.013)	−0.084 (0.009)	−0.096 (0.010)				

Note: Asymptotic standard errors in parentheses.

Whenever a cell had to be dropped because of exact multicollinearity this is denoted by "E". The interaction effect then is zero. The base year is 1992. Finally cells with a full stop denote either empty cells or cells that were excluded because the number of observations were less than 50.

Following the reduced forms we present the cohort education effects and time effects for the model in column (i) of Table VI in Tables XIII and XIV. The base year is 1992 and the base cohort consists of those born in the 1960's with post compulsory education.

In Table XV describing the data, child is a dummy for the age of the youngest child, education is the age at which the individual left full time education, wages and other income are in 1992 prices, and year denotes the financial year that starts in April.

TABLE X

REDUCED FORM FOR OTHER INCOME

	Time Effects	Compulsory Schooling Year of Birth				Post Compulsory Schooling Year of Birth			
		< 1940	1940–49	1950–59	1960 +	< 1940	1940–49	1950–59	1960 +
Group Effect:		140.13	123.85	109.39	95.22	220.72	160.62	110.60	90.88
		(14.51)	(9.36)	(6.52)	(5.74)	(18.56)	(15.89)	(5.95)	(6.21)
Year = 78	−40.16	6.34	1.85	E	·	−70.75	−34.82	E	·
	(7.49)	(17.12)	(13.23)			(22.88)	(19.67)		
Year = 79	−24.61	−5.31	0.95	E	·	−36.30	−19.02	E	·
	(7.31)	(17.16)	(13.08)			(24.00)	(19.95)		
Year = 80	−33.30	0.32	0.06	E	·	−16.97	−12.65	E	·
	(7.42)	(17.25)	(13.26)			(23.51)	(19.97)		
Year = 81	−41.34	13.64	10.29	3.58	E	·	−16.53	E	·
	(8.61)	(17.92)	(14.05)	(10.37)			(20.17)		
Year = 82	−33.71	4.00	20.92	4.57	E	·	−5.26	E	·
	(8.46)	(18.02)	(14.00)	(10.05)			(20.26)		
Year = 83	−34.86	17.14	14.95	9.15	E	·	−6.44	E	·
	(8.26)	(18.12)	(14.23)	(10.26)			(20.42)		
Year = 84	−20.63	2.92	6.96	−2.30	E	−38.64	−16.74	E	·
	(8.10)	(18.10)	(14.32)	(10.13)		(25.52)	(20.96)		
Year = 85	−23.32	2.39	8.06	0.53	E	·	3.83	12.78	E
	(9.64)	(19.11)	(15.46)	(12.21)			(21.62)	(12.33)	
Year = 86	−10.73	−11.55	−2.19	3.71	E	·	−6.22	7.69	E
	(8.84)	(18.78)	(15.26)	(11.62)			(21.47)	(11.83)	
Year = 87	−20.84	5.39	17.97	1.48	E	·	21.55	16.38	E
	(8.31)	(18.86)	(15.17)	(11.27)			(21.44)	(11.36)	
Year = 88	−12.06	3.96	15.61	5.01	E	·	−10.16	37.56	E
	(8.02)	(19.05)	(14.81)	(11.21)			(20.93)	(11.48)	
Year = 89	−16.16	0.89	15.76	3.75	E	·	−18.15	23.10	E
	(7.69)	(18.86)	(14.56)	(11.13)			(21.75)	(11.33)	
Year = 90	−24.68	22.29	6.22	8.69	E	·	32.16	51.34	E
	(7.81)	(19.31)	(14.82)	(11.42)			(20.71)	(11.60)	
Year = 91	−17.39	9.25	22.32	12.45	E	·	14.24	33.93	E
	(7.54)	(19.73)	(14.62)	(11.30)			(21.65)	(11.51)	
Child Aged:		0–2	3–4	5–10	11 +				
		78.50	75.16	64.74	49.37				
		(2.36)	(3.03)	(2.44)	(2.88)				
R^2	0.086								

Note: Asymptotic standard errors in parentheses.

APPENDIX B: THE COMPUTATION OF THE STANDARD ERRORS

The model we estimate has the form

$$y_i = \beta' x_i + \delta' \hat{z}_i + v_i$$

where i denotes individuals, x_i contains all the regressors including the time effects and the group effects. The $\hat{z}_i$ are the estimated residuals. Let the kth estimated residual be defined by $\hat{z}_i^k = s(m_i' \hat{\gamma}_k)$ where s (.) could represent a generalized residual or just a residual from a linear reduced form and

TABLE XI

REDUCED FORM PARTICIPATION PROBIT

	Time Effects	Compulsory Schooling Year of Birth				Post Compulsory Schooling Year of Birth			
		< 1940	1940–49	1950–59	1960 +	< 1940	1940–49	1950–59	1960 +
Group Effect:		0.328	1.043	1.590	1.582	0.754	1.316	1.751	1.846
		(0.153)	(0.114)	(0.082)	(0.071)	(0.205)	(0.199)	(0.075)	(0.079)
Year = 78	−0.348	0.718	0.496	E	·	0.413	0.284	E	·
	(0.092)	(0.187)	(0.160)			(0.253)	(0.241)		
Year = 79	−0.154	0.511	0.380	E	·	0.099	0.168	E	·
	(0.091)	(0.188)	(0.159)			(0.265)	(0.245)		
Year = 80	−0.248	0.568	0.495	E	·	0.270	0.342	E	·
	(0.092)	(0.189)	(0.161)			(0.262)	(0.245)		
Year = 81	−0.293	0.459	0.348	0.075	E	·	0.316	E	·
	(0.107)	(0.197)	(0.171)	(0.128)			(0.248)		
Year = 82	−0.404	0.567	0.404	−0.077	E	·	0.243	E	·
	(0.104)	(0.198)	(0.169)	(0.122)			(0.247)		
Year = 83	−0.318	0.511	0.339	−0.117	E	·	0.119	E	·
	(0.101)	(0.198)	(0.171)	(0.123)			(0.248)		
Year = 84	−0.414	0.759	0.527	0.147	E	0.288	0.277	E	·
	(0.098)	(0.198)	(0.172)	(0.121)		(0.281)	(0.254)		
Year = 85	−0.351	0.540	0.373	−0.032	E	·	0.106	0.130	E
	(0.120)	(0.212)	(0.188)	(0.149)			(0.263)	(0.152)	
Year = 86	−0.071	0.252	0.231	−0.077	E	·	0.133	−0.080	E
	(0.115)	(0.210)	(0.189)	(0.146)			(0.269)	(0.150)	
Year = 87	−0.036	0.363	0.158	−0.131	E	·	−0.088	−0.043	E
	(0.105)	(0.209)	(0.186)	(0.139)			(0.264)	(0.139)	
Year = 88	−0.012	0.431	−0.010	0.016	E	·	0.038	−0.244	E
	(0.100)	(0.211)	(0.179)	(0.139)			(0.261)	(0.139)	
Year = 89	−0.030	0.287	0.195	−0.031	E	·	−0.084	−0.144	E
	(0.094)	(0.206)	(0.179)	(0.136)			(0.268)	(0.136)	
Year = 90	−0.015	0.182	0.191	−0.080	E	·	0.086	−0.177	E
	(0.096)	(0.210)	(0.185)	(0.139)			(0.261)	(0.140)	
Year = 91	0.106	0.321	−0.189	−0.213	E	·	−0.090	−0.220	E
	(0.093)	(0.219)	(0.177)	(0.139)			(0.273)	(0.139)	
Child Aged:		0–2	3–4	5–10	11 +				
		−1.831	−1.284	−0.650	−0.149				
		(0.030)	(0.035)	(0.030)	(0.035)				

Note: Asymptotic standard errors in parentheses.

where $\hat{\gamma}_k$ is the $q_k \times 1$ vector of coefficients in the kth reduced form. The $q_k \times 1$ vector of variables included in the reduced form for observation i is denoted m_i. Let z_i represent the residuals evaluated at the true parameter estimates. Finally $v_i = u_i + \delta'(z_i - \hat{z}_i)$. In computing the standard errors we need to account for the effect of using estimated rather than actual values for γ_k. Dependence within groups and time is mainly accounted for by the presence of the group and time effects (see, for example, Moulton (1986)). However there may still be some limited dependence between the errors within a group even after removing these main effects. We use a White (1982) approach to allow for this problem. There are N_{gt} individuals within each group in period t. Let the

TABLE XII

REDUCED FORM ORDERED PROBIT FOR SELECTION AWAY FROM THE KINK

	Time Effects	Compulsory Schooling Year of Birth < 1940	1940–49	1950–59	1960 +	Post Compulsory Schooling Year of Birth < 1940	1940–49	1950–59	1960 +
Group Effect:		−0.494	−0.319	0.312	0.448	.	0.381	0.711	0.787
		(0.327)	(0.270)	(0.252)	(0.247)	.	(0.312)	(0.241)	(0.253)
Year = 78	0.109	−0.471	0.045	E	·	0.428	0.680	E	·
	(0.116)	(0.393)	(0.250)		·	(0.174)	(0.312)		
Year = 79	−0.102	0.209	0.198	E	·	0.689	0.386	E	·
	(0.108)	(0.265)	(0.247)		·	(0.166)	(0.319)		
Year = 80	−0.020	0.337	0.212	E	·	0.510	0.227	E	·
	(0.106)	(0.261)	(0.247)		·	(0.166)	(0.325)		
Year = 81	0.044	0.185	−0.036	0.286	E	·	−0.089	E	·
	(0.144)	(0.252)	(0.267)	(0.193)		·	(0.172)		
Year = 82	0.028	−0.072	−0.096	0.199	E	·	−0.205	E	·
	(0.143)	(0.264)	(0.268)	(0.192)		·	(0.169)		
Year = 83	−0.029	0.209	−0.179	0.296	E	·	−0.230	E	·
	(0.131)	(0.267)	(0.263)	(0.188)		·	(0.164)		
Year = 84	−0.347	0.346	0.270	0.398	E	0.043	0.404	E	·
	(0.123)	(0.274)	(0.260)	(0.181)		(0.150)	(0.340)		
Year = 85	−0.015	0.258	−0.003	0.124	E	·	−0.260	−0.198	E
	(0.154)	(0.268)	(0.280)	(0.208)		·	(0.189)	(0.279)	
Year = 86	−0.108	−0.195	−0.037	0.215	E	·	−0.031	0.275	E
	(0.134)	(0.193)	(0.270)	(0.198)		·	(0.170)	(0.276)	
Year = 87	−0.136	0.122	−0.110	0.421	E	·	−0.001	0.282	E
	(0.131)	(0.190)	(0.270)	(0.196)		·	(0.168)	(0.278)	
Year = 88	−0.079	0.215	0.010	0.214	E	·	0.019	0.236	E
	(0.121)	(0.173)	(0.271)	(0.189)		·	(0.160)	(0.264)	
Year = 89	−0.062	0.135	−0.093	0.332	E	·	0.023	0.259	E
	(0.116)	(0.176)	(0.268)	(0.185)		·	(0.157)	(0.279)	
Year = 90	0.048	0.132	−0.142	0.216	E	·	−0.204	−0.041	E
	(0.118)	(0.169)	(0.276)	(0.188)		·	(0.161)	(0.259)	
Year = 91	0.070	−0.134	−0.283	0.076	E	·	−0.224	0.245	E
	(0.112)	(0.167)	(0.273)	(0.185)		·	(0.158)	(0.276)	
Child Aged:		0–2	3–4	5–10	11 +				
		−1.561	−1.643	−1.335	−0.718				
		(0.040)	(0.044)	(0.033)	(0.035)				
Thresholds		no tax/NI	NI/tax						
		−1.748	−1.075						
		(0.248)	(0.248)						

Note: Asymptotic standard errors in parentheses.

$N_{gt} \times N_{gt}$ covariance matrix of the errors within a group g in time period t be denoted by Ω_{gt}. The off-diagonal elements represent intragroup covariances. Let X_{gt} and Z_{gt} represent the matrix of N_{gt} observations of the variables in x and z respectively for group g in period t. Define the $N_{gt} \times p$ matrix $Q_{gt} = [X_{gt} \quad Z_{gt}]$, p being the total number of regressors including the residuals z_i. Let Q represent the entire matrix of observations over the whole sample for the x and z variables. We

TABLE XIII

COHORT / EDUCATION EFFECTS

	Cohort Education Effects on Labor Supply (from Table VI, column (i))						
	Low Education				High Education		
Cohort:	< 1940	1940–49	1950–59	1960 +	< 1940	1940–49	1950–59
Coef.	−9.434	−6.335	−3.942	−1.107	−7.978	−5.336	−2.694
Stand. Err.	1.229	0.966	0.818	0.836	1.970	1.280	0.959

assume that

$$\text{(9)} \qquad \operatorname{plim}_{N_{gt}\to\infty} \frac{Q'_{gt}\Omega_{gt}Q_{gt}}{N_{gt}} = P_{gt}$$

where P_{gt} is a $p \times p$ positive definite matrix. This assumption effectively limits the amount of intra-group dependence and implies that the model can be consistently estimated with a fixed number of time periods and the number of individuals going to infinity, which is our framework. Denote the $N_{gt} \times 1$ vector of estimated residuals within a group g in period t by $\hat{v}_{gt}$. Denote $\zeta = (\beta' \ \delta')'$. There are G groups over T time periods and K generated regressors. Denote by Γ_{gt}^{k} the $N_{gt} \times q_k$ matrix whose ith row is given by the derivative of $s(m_i'\hat{\gamma}_k)$ with respect to γ_k. Finally denote by $V(\hat{\gamma}_k)$ the covariance matrix of $\hat{\gamma}_k$. We assume that the number of time periods and the number of groups is fixed but that the number of individuals within each group is large and goes to infinity. Given the above assumptions we can estimate consistently the asymptotic covariance of the estimated parameters $\hat{\zeta}$ by

$$V(\hat{\zeta}) = (Q'Q)^{-1}\left\{\sum_{g=1}^{G}\sum_{t=1}^{T}\left[Q'_{gt}\hat{v}_{gt}\hat{v}'_{gt}Q_{gt} + \sum_{k=1}^{K}\hat{\delta}_k^2 Q'_{gt}\Gamma_{gt}^{k}V(\hat{\gamma}_k)\Gamma_{gt}^{k\prime}Q_{gt}\right]\right\}(Q'Q)^{-1}.$$

This covariance matrix allows for the effects of estimated residuals, for heteroskedasticity, and for dependence within groups consistent with assumption (9). The formula we use ignores, for computational simplicity, the covariance of the coefficients $\hat{\gamma}_k$ across the $k = 1, \ldots, K$ reduced forms. However, note that in our case the correction for generated regressors (the second term in the square brackets) accounts only for a small component of the above covariance matrix.

TABLE XIV

TIME EFFECTS

	Time Effects on Labor Supply. Base Financial Year 1992 (from Table VI, Column (i))													
Fin. Year	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991
Coef.	3.79	2.74	2.46	1.26	0.95	0.91	0.02	0.55	0.77	0.90	1.38	0.85	1.06	1.27
Stand. Err	1.16	1.04	0.89	1.08	0.90	0.87	1.03	0.79	0.61	0.81	0.60	0.61	0.78	0.59

TABLE XV

DESCRIPTIVE STATISTICS FOR THE SAMPLE OF WORKERS

Year	78	79	80	81	82	83	84	85
Hours	27.34	26.62	26.53	25.89	25.81	26.04	25.17	26.41
	11.75	*12.00*	*11.95*	*12.12*	*11.97*	*12.07*	*11.99*	*11.72*
Log Wage	0.90	0.94	0.99	0.96	0.99	1.04	1.06	1.29
	0.39	*0.39*	*0.40*	*0.40*	*0.41*	*0.44*	*0.40*	*0.45*
Other Inc.	109.55	121.88	115.29	110.41	116.08	115.15	121.82	127.71
	87.51	*105.22*	*104.81*	*97.44*	*102.83*	*97.65*	*106.84*	*112.30*
Child 0–2	0.08	0.09	0.08	0.09	0.09	0.10	0.08	0.14
Child 3–4	0.08	0.07	0.07	0.05	0.06	0.06	0.08	0.09
Child 5–10	0.23	0.25	0.25	0.27	0.22	0.22	0.23	0.20
Child 11 +	0.12	0.12	0.14	0.12	0.14	0.15	0.15	0.15
Age	38.25	37.97	38.47	38.10	38.04	37.99	38.50	38.60
	7.62	*7.29*	*6.71*	*7.09*	*6.89*	*6.92*	*6.92*	*6.04*
Education	15.96	15.98	16.05	16.23	16.26	16.24	16.24	16.86
	2.21	*2.06*	*2.19*	*2.25*	*2.26*	*2.09*	*2.10*	*2.14*
Educ > 16	0.23	0.23	0.23	0.27	0.24	0.27	0.26	0.39

Year	86	87	88	89	90	91	92
Hours	26.51	26.93	27.22	27.14	27.03	27.03	26.97
	12.14	*11.89*	*12.00*	*12.40*	*11.74*	*12.06*	*12.42*
Log Wage	1.08	1.14	1.18	1.24	1.24	1.23	1.29
	0.40	*0.44*	*0.46*	*0.46*	*0.43*	*0.45*	*0.47*
Other Inc.	114.45	124.38	120.47	131.23	118.22	117.14	121.59
	109.31	*153.74*	*125.51*	*170.55*	*126.58*	*126.00*	*116.23*
Child 0–2	0.10	0.10	0.14	0.13	0.15	0.12	0.14
Child 3–4	0.07	0.08	0.08	0.08	0.07	0.09	0.09
Child 5–10	0.22	0.20	0.20	0.22	0.21	0.19	0.19
Child 11 +	0.12	0.12	0.10	0.12	0.10	0.16	0.15
Age	37.50	37.44	37.27	37.62	37.80	38.11	38.27
	6.36	*6.58*	*6.80*	*6.43*	*6.50*	*6.30*	*6.36*
Education	16.51	16.68	16.67	16.71	16.64	16.91	16.90
	2.17	*2.28*	*2.18*	*2.17*	*2.07*	*2.35*	*2.37*
Educ > 16	0.32	0.34	0.35	0.36	0.36	0.39	0.37

Note: Wages and Other Income in April, 1992 prices.

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