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The Effect of Anticipated Tax Changes on Intertemporal Labor Supply and the Realization of Taxable Income

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Abstract

We use anticipated changes in tax rates associated with changes in family composition to estimate intertemporal labor supply elasticities and elasticities of taxable income with respect to the net-of-tax wage rate. A number of provisions of the tax code are tied explicitly to child age and dependent status. Changes in the ages of children can thus affect marginal tax rates through phase-in or phase-out provisions of tax credits or by shifting individuals across tax brackets. We identify the response of labor and income to these tax changes by comparing families who experienced a tax rate change to families who had a similar change in dependents but no resulting tax rate change. A primary advantage of our approach is that the changes are anticipated and therefore should not cause re-evaluations of lifetime income. The estimates of substitution effects should consequently not be confounded by life-cycle income effects. The empirical design also allows us to compare similar families and can be used to estimate elasticities across the income distribution. In particular, we provide estimates for low and middle income families. Using data from the Survey of Income and Program Participation (SIPP), we estimate an intertemporal elasticity of family labor earnings close to one for families earning between \$30,000 and \$75,000. Our estimates for families in the EITC phase-out range are lower but still substantial. Estimates from the IRS-NBER individual tax panel are consistent with the SIPP estimates. Tests using alternate control groups and simulated "placebo" tax schedules support our identifying assumptions. The high-end estimates suggest substantial efficiency costs of taxation.

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1 Introduction

Understanding how individuals shift labor supply and income over time in response to wage and tax rate changes is crucial for numerous economic questions. Estimates of intertemporal labor supply elasticities and taxable income elasticities have important implications for life cycle labor supply, aggregate employment fluctuations and business cycles, efficiency costs of taxation, and the design of optimal tax and transfer systems. There remains considerable uncertainty over the magnitude of these responses. Most estimates of the intertemporal elasticity of labor supply find negligible shifting over time (MaCurdy 1981, Altonji 1986, Blundell and MaCurdy 1999).[1] More recent studies, however, have found more sizable elasticities (Mulligan 1998, Kimball and Shapiro 2003). Estimates of the elasticity of taxable income are also variable, ranging from 0.10 to 3 (Gruber and Saez 2000).

We develop an empirical methodology that allows us to identify elasticities using anticipated tax changes associated with changes in family composition. Parents may claim tax credits and dependent exemptions for their children that are often explicitly tied to children's ages. Changes in the ages of children can thus change parents' marginal tax rates by shifting individuals across tax brackets or through phase-in or phase-out provisions of tax credits. The aging of children provides an exogenous source of variation with which to instrument for actual marginal tax rate changes. We examine changes in family labor supply and income at the time families experience a child-related tax rate change. Only families with incomes close to "kink" points or in phase-in or phase-out ranges experience a change in tax rates as a result of these changes in family composition. We are therefore able to net out changes in tastes that may accompany changes in family composition by comparing our treatment families to similar families who do not experience a tax rate change.

[1] There is an extensive literature estimating static and intertemporal labor supply elasticities. Surveys of this literature include: Heckman and Killingsworth (1986), Pencavel (1986), Card (1994) and Blundell and MaCurdy (1999).

There are several advantages of our methodology for estimating elasticities. A key feature of the strategy is that tax rate changes can be anticipated in advance. This implies that these changes should not precipitate re-evaluations of lifetime income at the time the tax rate change is experienced, allowing us to estimate compensated elasticities that are uncounfounded by life-cycle wealth effects. Most studies estimating the elasticity of taxable income have relied on tax policy changes which are often sudden and may have significant effects on lifetime wealth. Much of the literature is therefore unable to credibly distinguish between income and substitution effects, which is problematic given that the relevant parameter for evaluating the efficiency cost of taxation is the compensated elasticity. The estimation of intertemporal labor supply elasticities has similarly been hindered by a lack of good instruments for anticipated wage changes. By examining the response to tax rate changes that are both exogenous and anticipated, we are able to estimate the compensated elasticity of taxable income and the elasticity of intertemporal labor supply.

Another advantage is that the tax rate changes we study affect individuals throughout the income distribution, which yields two benefits. First, this allows us to compare families with very similar income and demographic characteristics. Many previous estimates of the effects of taxes on labor supply rely on comparisons of different income groups. These comparisons can be problematic if incomes of the different groups grow differentially over time, as during the growth in inequality over the 1980s. We also include rich controls for base year income, alleviating problems of mean reversion and changes in the income distribution.

Second, evidence suggests that the magnitude of the behavioral response to taxation varies across income levels. In this paper, we are able to provide new elasticity estimates for middle and lower-middle income families that comprise approximately 60 percent of the US income distribution. Much of the existing literature has focused on estimates for the extremes of the distribution.[2]

[2] An exception is Gruber and Saez (2000) who estimate elasticities across the income distribution using a variety of tax policy changes. They find an overall elasticity of taxable income of 0.4 which is primarily driven by responses

These estimates are relevant for certain policy questions: elasticities for high income groups may be particularly relevant for calculating revenue effects of tax changes and elasticities for very low income families have important implications for the design of welfare programs. Elasticities for our income ranges, however, are critical for determining the magnitude of aggregate employment fluctuations and for evaluating the efficiency costs of taxation.

We implement our methodology by using panel data from the 1990-1996 SIPP panels and the NBER tax panel (1987-1990). The SIPP is a nationally representative panel survey of households. The SIPP data contain detailed demographic and income information on all family members, allowing us to measure labor supply responses and decompose those estimates into a variety of margins of response. The tax panel contains precise data on AGI and taxable income and therefore allows us to estimate a broader measure of the response to tax rate changes.

We focus primarily on tax rate changes arising from the loss of a dependent exemption. Using the SIPP, we estimate a significant elasticity of family labor income close to one for families with base year earnings between \$30,000 and \$75,000.[3] Elasticity estimates for alternate treatment groups (families gaining a dependent and families in the EITC phaseout range) are lower but still large and significant. We find broadly consistent estimates for similar families using the tax panel data.

These estimates are higher than those found in previous work. This may be because studies examining unanticipated changes tend to confound substitution and income effects, which would result in downward biased estimates. Our high-end estimates are consistent with elasticities implied by calibrating real business cycle models to data on macro fluctuations and imply substantial

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of high income taxpayers. Studies estimating the elasticity of taxable income of the rich include Feldstein (1995) and Auten and Carroll (1999). Eissa (1995) estimates the elasticity of taxable income for high-income married women. Studies focusing on labor supply responses of low income families include Eissa and Leibman (1996), Eissa and Hoynes (1998), Meyer and Rosenbaum (2001), and Meyer (2002).

[3]Throughout the paper, all dollar figures are adjusted to year 2000 using the CPI-U.

deadweight loss from taxation.

The remainder of the paper proceeds as follows: Section 2 provides an overview of intertemporal substitution in the life-cycle model and explains the importance of anticipation in separating price and income effects. Section 3 describes the relationship between changes in family composition and changes in marginal tax rates. Section 4 provides a description of the SIPP and NBER tax panel data and Section 5 describes the empirical methodology. Section 6 presents results and tests of the identification strategy. Section 7 discusses implications of the results and directions for future research.

2 Intertemporal Substitution in the Life-Cycle Model

The traditional life-cycle labor supply model assumes that individuals maximize an intertemporally separable utility function:

$$U = \Sigma_{t=0}^{T} \beta^t U\left(c_{it}, l_{it}, a_{it}\right)$$

where β^t is a discount factor, c_{it} is period t consumption, l_{it} is labor and a_{it} is the taste of the individual in period t. Individuals maximize utility subject to the intertemporal and lifetime budget constraints:

$$A_{it+1} = \left(1 + r_t\right)\left[A_{it} + w_{it} l_{it} - c_{it}\right]$$

$$A_T \geq 0$$

where A_{it} is assets, r_t is the interest rate. Finding the first order conditions and solving for l_{it} gives the following Frisch labor supply equation:

$$l_{it} = l_{it}\left(w_{it}, \lambda_{it}, a_{it}\right)$$

where λ_{it} is the Lagrange multiplier or the marginal utility of income.

After taking a log-linear approximation, changes in labor supply over time can be decomposed as follows:

$$
\begin{aligned}
\Delta \ln(l_{it}) &= \Delta\alpha_{it} + \gamma\Delta \ln(w_{it}) - \delta(r_{it} - \rho) + \delta[\ln(\lambda_{it}) - E_{t-1}(\ln(\lambda_{it}))] + \delta\varepsilon_{it} \\
\Delta \ln(l_{it}) &= \Delta\alpha_{it} + \gamma\Delta \ln(w_{it}) - \delta(r_{it} - \rho) + \delta\Phi_{it} + \delta\varepsilon_{it}
\end{aligned}
$$

where $\Delta \ln(l_{it})$ is the change in log labor supply (or taxable income), $\Delta\alpha_{it}$ is changes in tastes, $\delta(r_{it} - \rho)$ denotes differences in the rate of time preference and the interest rate, Φ_{it} is the difference between expected and actual marginal utility of wealth, and ε_{it} is a disturbance term. Many studies assume $r_{it} = \rho$, control for various covariates to proxy for changes in tastes, assume perfect capital markets and perfect foresight, and allocate $\delta\Phi_{it}$ to the error term.

These assumptions are not innocuous. If individuals cannot smooth consumption over time by borrowing or saving, or if wage changes are unanticipated and lead to permanent changes in lifetime wealth it is unlikely that $E[\ln(\lambda_{it}) - E_{t-1}(\ln(\lambda_{it}))] = 0$, i.e. that the marginal utility of wealth is not affected by the wage change. This is problematic when using tax reforms or other wage changes to estimate intertemporal labor supply because these changes also affect permanent income.[4]

Researchers have tried to overcome these issues using instrumental variables strategies and natural experiments. However, it is difficult to find exogenous wage changes that do not change expected lifetime wealth. The past literature has used age and education related variables as instruments for life cycle wage changes but these are unlikely to be exogenous to changes in tastes

[4]The derivation of the labor supply equation is not as straightforward when a non-linear budget set is introduced. However, the same intuition applies. See Blomquist (1985) and Ziliak and Kniesner (1999) for further discussion. We discuss the implications for interpreting our estimates if families are myopic or face credit constraints in the Results section.

and therefore do not satisfy the exclusion restriction. Furthermore, these estimates are often sensitive to the choice of instruments (Mroz 1987). Mulligan (1998) notes these difficulties and examines the labor supply response of AFDC recipients to a fully anticipated change in wages: the termination of AFDC benefits when the recipient's youngest child turns 18. Mulligan's estimates imply large elasticities (between 0.38 and 1.66). We revisit the Mulligan experiment in Section 6.6.

The same issue is relevant in the literature on elasticities of taxable income. When tax changes are unanticipated, they affect families' income and lifetime wealth. Resulting elasticity estimates are therefore a mix of income and substitution effects. If leisure is a normal good, mixing income and substitution effects will downward bias elasticity estimates. Few existing studies in the literature have attempted to separate the two. TRA86 was designed to be revenue neutral within income categories and therefore arguably did not induce wealth effects. However, the reform was at best revenue neutral at the income class level, not the individual level. Saez (1999) estimates elasticities using "bracket creep," an experiment in which income effects are likely to be negligible. Gruber and Saez (2000) attempt to mitigate income effects by explicitly including a term for the change in after-tax income in their estimating equation. However, the one year change in after tax income is likely to misstate the net change in wealth over the life-cycle.

3 Family Composition and Marginal Tax Rates

Changes in the age structure of children may affect marginal tax rates for a number of reasons. Figure I illustrates the differences in federal marginal tax rates faced by married couples with differing numbers of dependents. At the lower end of the income distribution the EITC phase-in and phase-out ranges have the greatest effect on marginal tax rates. The child tax credit phase-out increases marginal tax rates by 5 percentage points for families starting at $80,000 for families with children age 16 or younger. These phase-in and phase-out rates add to statutory bracket rates. In addition, the dependent and child care credit, the Hope and Lifetime learning credits, the student loan interest deduction, and the exemption phase-out are all related to a child's age and/or post-secondary school attendance.

The figure also illustrates changes in marginal tax rates that occur because of the dependent exemption. Increases in the number of dependents reduce taxable income (by \$3,000 in 2002) thereby pushing bracket "kink" points to higher levels of AGI. This is apparent in Figure 1 for those families with AGI between \$45,000 and \$55,000. These bracket shifts move families between the 15 percent bracket and the 28 percent bracket. Therefore when a child no longer qualifies as a dependent (either by turning 24 or by turning 19 and not pursuing full time post secondary schooling), the marginal tax rate of families in this range roughly doubles. We focus primarily on this group in the following analyses and then consider alternative treatment groups.

Figure 2 illustrates the change in the budget set for families losing a dependent in the median income range. For a given level of AGI, after tax income shifts down as a result of the loss of the dependent exemption. For some families, this loss does not result in a change in marginal tax rate. For families with initial AGI in the region immediately below the kink point, the loss of the dependent exemption shifts them into a higher tax bracket. For the same level of AGI, they now face a higher marginal tax rate. As the figure shows, there are no dominated kink points in our experiment; families may rationally chose to locate over the entire budget set.

Our identification strategy compares changes in labor supply of families for whom the loss of a dependent causes a change in marginal tax rates (the "treatment" group illustrated on Figure 2) to changes for families for whom the loss of a dependent has no marginal tax consequences (the "control" groups). Note that the loss of a dependent may have a number of direct effects on labor supply. However, as long as these direct effects are the same for treatment and control families, our estimates will not be affected. Under the assumption that treatment and control groups experience similar changes in tastes and shocks to income between periods, the labor supply elasticity is identified by changes in the tax rate of the treatment group.

4 Data

We implement our empirical strategy using data from the 1990-1996 panels of the Survey of Income and Program Participation (SIPP) and from the NBER tax panel from 1987-1990.[5] By exploiting both of these data sources, we are able to provide a comprehensive picture of families' responses to tax incentives. The SIPP data contains detailed information on all family members, allowing us to identify husbands' and wives' responses and also to determine whether responses are occurring on participation, hours or other margins. The SIPP also provides a relatively large sample size and the detailed information on children required to predict their status as dependents. The tax panel is smaller and contains only the most rudimentary demographic information, but has the advantage of extremely precise income measures and allows us to capture other dimensions of the behavioral response to taxation.

The SIPP is a nationally representative panel survey of households. Each panel includes between 20,000 and 45,000 households. Within panels each household is interviewed at four month intervals for between 28 and 48 months. The 1990-1996 panels cover the time period from 1990 to 1999. We focus our attention on married couples who live with their own dependent children (children under age 19 or under 24 and full time students) at some point during the panel.[6]

[5] The tax panel data are also referred to as the University of Michigan Tax Panel and the Continuous Work History File. Note that the short panels of the CPS are not suitable for our analysis because of the difficulty identifying dependents once they leave the household and poor matching rates. It would be worthwhile to validate our estimates using data such as the Treasury department tax panel data, which contain large samples of taxpayer returns over a long time period and have information on demographic variables unavailable in the public use data. Our strategy could be used with those data to provide estimates for other income ranges not considered here due to sample size constraints.

[6] We limit our sample to married couples because non-married parents often have a choice over which individual

Our empirical strategy involves looking at year to year changes in income and taxes so we first-difference the annual observations and use these changes as the unit of observation. We retain only those families with first-period income between $30,000 and $75,000 per year. We also drop families that experience a change in marital status or a change in their marginal tax rate greater than 50 percentage points between year 1 and year 2 (0.2 percent of the sample).

Table 1 provides summary statistics for this sub-sample of interest. Column 1 includes all observations. Families averaged $51,203 in earnings (defined as labor income earned by the mother or father). Women in the sample are, on average, 38 years old (husbands are about 2 years older), high school graduates, and have 1.8 dependents living in their household. 11 percent are non-white.

Columns 2 and 3 compare those with no change in the number of dependent children to those that have one fewer because of a change in the age of their child.[7] In general, these families are very similar. Mothers losing a dependent are seven years older than those with no change, have slightly lower mean income and schooling, and mechanically have one less child.

We augment these data with the NBER tax panel for the years 1987-1990. The tax panel, assembled by the Statistics of Income Division of the IRS, is a longitudinal sample of federal tax returns sampled randomly on the basis of the last four digits of filers' social security numbers. These data span the time period from 1979 to 1990. However, we use only the 1987-1990 years to avoid complications associated with the 1981 and 1986 tax reforms and because the tax schedule during this period closely resembles the schedule we examine in the SIPP data. These data provide excellent information on tax-related variables; almost every element of a typical filer's return is recorded in the data. However, the only demographic information contained in the data is marital status (through filing status) and a proxy for children (through number of dependents).

claims a child as a dependent. Such claiming decisions may be endogenous to tax incentives.

[7] The remaining families included in column 1 are almost entirely families who experienced a gain of one dependent; families experiencing a gain or loss of more than one dependent represent less than 1% of the sample.

The sample originally contained more than 46,000 returns, but after 1981 the IRS choose to follow only a fraction of the original sample. Combined with attrition due to factors like marriage and non-filing, approximately 20,000 returns remain each year between 1987-1990.

Our sample restrictions are similar to those used for the SIPP. We again restrict the sample to married couples and drop returns filed by dependents or irregular filers, those who change filing status, and returns for families experiencing a change in marginal tax rate greater than 50 percentage points between year 1 and 2.

Table 2, column 1, shows that individuals in the selected income range have average wage income of \$50,168, quite similar to SIPP sample. Average AGI is \$56,365 and taxable income is \$36,689. Filers claim an average of 1.3 dependents. Columns 2 and 3 again compare those losing a dependent to those with no change in the number of dependents. These groups appear quite similar.

5 Methods

The outline of our empirical strategy is as follows: In order to locate families correctly on the tax schedule, we predict income using lagged income and other controls. We then predict the change in marginal tax rates faced by families and instrument for their actual change in the net-of-tax wage with our predicted change in net-of-tax wage. Regressing changes in labor supply and income variables on this instrumented change in tax price produces estimates of the relevant elasticities.

We predict income in the SIPP panel by running a log linear prediction of this year's income on last year's income and year dummies to capture mean wage growth that year for the families in the \$30-\$75K income range in the base year. $\widehat{Income}_{it}$ is given by $\ln\left(\widehat{Income}_{it}\right) = \beta \ln\left(Income_{it-1}\right) + \rho(age_{it}) + \delta \sum_t Year_t$.[8] In the tax panel we take a more simplified approach and assume that AGI

[8] We use this measure of income because it appears to be more accurate than a log variance adjusted measure of

remains constant in real terms between year 1 and 2.

We estimate the change in marginal tax rates using NBER's TAXSIM program.[9] TAXSIM calculates state and federal tax liabilities and tax rates from survey data using information on income, marital status, and number and age of children. For each family, we calculate the anticipated change in marginal tax rates from year $t-1$ to t as the difference in marginal tax rates between the actual marginal tax rate last year and predicted marginal tax rate this year, where the prediction is formed using predicted income and actual number of dependents. Formally this corresponds to the following:

$$\Delta mtr_{it} = Tax_{it-1}\left(\widehat{Income}_{it}, dependents_{it}\right) - Tax_{it-1}\left(Income_{it-1}, dependents_{it-1}\right)$$

where Δmtr_{it} is the change in marginal tax rates faced by individual i between periods $t-1$ and t, and $Tax_{it-1}()$ is the tax schedule faced by individual i in period $t-1$ as a function of income and number of dependent children. Note that we use the $t-1$ tax schedule in both years so that we do not confound changes in tax rates arising from changes in dependents with those arising from changes in tax policy. When using the SIPP data, we define a dependent to be any individual who is eligible to be claimed as a dependent: individuals under 19 living in the household and individuals under 24 who are full-time students.[10] In the publicly available tax panel data, we do not have precise information on the ages of children. Therefore, we use changes in dependents claimed as reported on the tax return.

Table 3 provides summary statistics for these tax parameters. In the SIPP sample, families

expected level income.

[9]See Feenberg and Coutts (1993) for a detailed explanation of the TAXSIM calculators.

[10]To test our assignment of dependents, we merge data from SIPP Tax Modules to our sample. The predicted number of dependents matches the actual number recorded by the SIPP 92.4 percent of the time for people with a copy of their tax form (N=4,678) and 87.3 percent of the time more generally (N=15,385). Even for children older than 18, our predictions match actual dependents correctly more than 75% of the time. In addition to measurement error, part of the mismatch appears to be due to the fact that 4 percent of married couples file separately.

who lose a dependent and those who do not appear very similar in year 1: the year 1 marginal tax rate and the average taxes paid in year 1 are 29.5 percent and $12,687 for families with no change and 29.4 percent and $12,656 for families who lose a dependent. These figures include federal, state and FICA taxes. The predicted tax parameters for year 2 are different in the directions we would expect. Families without a change in dependents are predicted to face a small decline in marginal tax rates (to 29.2 percent) and tax liability (to $12,308). In contrast, families losing a dependent are predicted to experience an increase in marginal tax rates (to 29.8 percent) and a smaller decline in tax liability (to $12,565). Columns 3 and 4 present these statistics for the tax panel sample. The year 1 marginal tax rates and average taxes paid are comparable across groups and are similar to those from the SIPP sample. The year 2 predicted marginal tax rate and tax liability are higher for families losing a dependent than for families with no change.

As a prelude to the estimation strategy described below, we separate the sample of individuals who lose a dependent into those predicted to experience a tax change and those for whom no change is predicted. Table 4 provides statistics for the SIPP and tax panel samples. The net of tax rate falls for families experiencing a tax change (by 4 percent on average in the SIPP sample and by 13 percent in the tax panel) as parents are bumped into higher tax brackets as a result of losing exemptions. Those who experienced this change in the net of tax rate also experienced a drop in income relative to those with no change in the tax rate. In the SIPP sample, earnings of families with a change in tax rate dropped $1,730 on average between year 1 and year 2 compared to an increase of $833 for families with no change. The same pattern is evident in the tax panel sample: average earnings of families with a tax rate change dropped $3,365 compared to a drop of $649 for families with no change. Our empirical strategy aims to formalize this relationship.

We instrument for the actual change in marginal tax rates with our predicted change and estimate equations similar to those described in section 2:

$$\Delta \ln (l_{it}) = \gamma \Delta \ln (1 - \tau_{it}) + \alpha_1 X_{it} + \varepsilon_{it}$$

where $\Delta \ln (l_{it})$ is the change in log labor supply (or taxable income), $\Delta \ln (1 - \tau_{it})$ is the instrumented change in the log of the tax price, which measures the change in the net-of-tax wage, and X_{it} are covariates. γ is the estimate of the elasticity of intertemporal substitution.

6 Results

6.1 Elasticity of Family Labor Income

We focus first on families shifting from the 15 percent bracket to the 28 percent bracket as the result of losing a dependent exemption. This is likely to be the cleanest experiment since families are likely to anticipate the loss of a dependent well in advance and to understand the tax consequences. Table 5 presents elasticity estimates from the SIPP sample. The sample in columns 1-3 consists of families with income between $30,000 and $75,000 who lost a dependent between years 1 and 2. Column 1 presents the base regression of change in log family earnings on change in log tax price controlling for the log of family earnings in year 1 and year fixed effects. The elasticity estimate is 0.97 with a standard error of 0.31. This implies that a one percent change in a family's net-of-tax wage rate results in an almost one percent change in family labor earnings.

Adding controls for the number of dependents in year 2 (column 2) and mother's age (column 3) leave the elasticity estimate virtually unchanged. The estimates are significant at the 5 percent level in all cases. The coefficient on log year 1 earnings is negative (around -0.08) as would be expected if families experience mean reversion in earnings. Controlling for changes in the log of predicted after tax income does not affect the elasticity estimate. These results are also robust to richer controls for base period income and perturbations of the income range.

There are several reasons why our estimates may in fact be lower bounds on the true behavioral response. First, some families may not be aware of the potential change in marginal tax rates arising from changes in dependents. Second, families may not have full control over their incomes. As Saez (2003) points out, this may cause jumps in marginal tax rates to be partially smoothed out, implying that families' perceived change in marginal tax rates may be lower than actual changes. These factors should all create a downward bias in our estimates. Finally, if some families are myopic and do not predict the change in dependents or if families face credit constraints, they may be experiencing income effects. This would also cause us to underestimate the true substitution effect. In the extreme case where families are completely unable to borrow or save, the intertemporal link is broken and our estimates can be interpreted as comparable to the uncompensated elasticity of labor supply that would arise from a static model.

Despite the downward biases mentioned above, these elasticities are significantly higher than those estimated in early studies of intertemporal labor supply and lie at the high end of the most recent estimates of the elasticity of taxable income. If tax changes are anticipated, estimates are not downward biased by income effects. This bias may be quite substantial. In a survey module of the Health and Retirement Study, Kimball and Shapiro (2003) ask respondents to predict their labor supply response to a hypothetical lottery win and use these data to estimate income effects. In their framework, which assumes that income and substitution effects perfectly offset, these estimates imply a substitution elasticity of one, consistent with our estimates.[11] Anticipation also means that families have the ability to respond both *ex ante* and *ex post*, leading to potentially larger measured responses.[12]

[11]The assumption of cancelling income and substitution effects is motivated by estimates of uncompensated elasticities close to zero (Pencavel 1986). Imbens, Rubin and Sacerdote (2001) find smaller but still substantial income effects using data from a survey of actual lottery participants.

[12]It is also possible that families are especially flexible at the time a child leaves the household or completes school,

It should be noted that we are comparing only year-to-year changes and therefore capture only short-run responses to taxation. There are two major reasons we might in general expect short-run responses to tax changes to differ from long-run responses. First, families may shift income over time in response to the change, creating an upward bias in the "true" response. While this has been raised as a concern in studies of the rich (Slemrod 1995), it is less likely to be a concern for families in our income ranges as they have little capital income. Second, families may face commitments that make it difficult to adjust immediately in response to a tax change. In this case, short-run effects might understate long-run impacts. Again, this is unlikely to be problematic in our case. Because families can anticipate these tax changes in advance, they have time to make necessary adjustments. In this sense, our estimates should be capturing what would traditionally be thought of as the long-run response in a world with commitments. There may, however, be other reasons why short-run and long-run responses differ. Because the SIPP panels are 2 1/2 to 4 years and we are able to use only 3 years of the tax panel, we are limited in our ability to explore the dynamics of the effect over a longer time horizon.

6.2 Estimates for Alternate Treatment Groups

We now examine income responses for other groups whose marginal tax rates are affected by anticipated changes in family composition: families adding a dependent and families in other ranges of the income distribution. We first focus on families in the same income range as above, restricting the sample to families who added a dependent through the birth of a child. This experiment is potentially less clean than the case where a dependent is lost, since it is not clear how far in advance the birth of a child may have been anticipated for all families. Nevertheless, we find significant

making them particularly responsive to tax incentives. It would be worthwhile to explore this possibility further by applying the identification strategy to other child-related tax changes. The child tax credit, for example, is available to certain families until the child turns 17. The marginal tax rate implications of a variety of child-related tax provisions are described in detail by Looney and Singhal (2004).

elasticity estimates for this group (Table 6). In column 1, we regress the change in log family income on the instrumented change in log tax price, year 1 log family income and year dummies using the SIPP data. Our elasticity estimate is 0.23 with a standard error of 0.08. The result is robust to adding a variety of controls including mother's age and the number of year 2 dependents.

These labor income elasticity estimates are quite large, though substantially smaller than our estimates for families losing a dependent. There are several possible reasons for this difference. If families did not anticipate the additional dependent prior to the base year, we may be confounding wealth effects with price effects, downward biasing the estimate. In addition, these families are being shifted into a lower tax bracket and would therefore be expected to increase their labor supply. However, the birth of a child might constrain response margins, leading to a smaller effect than for families losing a dependent.[13] Note that the potential direct negative effect of a child born on labor supply is not problematic for our identification strategy as long as families away from and close to the kink point are affected similarly.

We now examine the response to dependent related tax changes for families in other parts of the income distribution. We focus in particular on families in the phaseout region of the EITC, restricting the sample to families with base year income between $15,000 and $40,000 (Table 6). We estimate an elasticity of family labor income for families losing a dependent of 0.09 (not significant) and an elasticity for families gaining a dependent of 0.12 (standard error 0.04).

The downward bias caused by families not understanding the marginal tax rate implications may be particularly severe for this group, particularly since we measure the first year response. As shown in Figure 1, these families are shifted from one complex tax schedule to another as a result of dependent changes. It may also be the case, however, that the difference is a result of true heterogeneity in elasticities over the income distribution.

[13] We might expect to see a larger response for these families later, when the child enters school.

Due to sample size constraints, we are unfortunately not able to estimate stable responses for higher income groups.

6.3 Placebo Tests

A potential concern with these estimates is that variation in the instrumented tax price may also arise from families being shifted across tax code provisions as a result of noise in the income estimating equation. As a test of our identification strategy we therefore run two types of placebo tests. First, we run the regressions on families in the same income range who did not experience a change in the number of dependents. Second, we estimate the effects using simulated tax schedules in which the tax rate discontinuity is shifted to alternate levels of income. If our estimates are driven by discontinuous changes in taxes arising from changes in dependents (rather than changes induced by our income estimating equation or other spurious effects), we should find no response for these groups.

In column 4 in Table 5, we run the same regression as in column 3 for families with base year income between \$30,000 and \$75,000 who had no change in the number of dependents between year 1 and year 2. We find support for our identification strategy: the coefficient on the change in tax price for this group is essentially zero (-0.004) and insignificant. As additional checks, we restrict the sample of families with no change in dependents to be as similar as possible to the treatment group. The sample in column 5 consists of families in the relevant income range who did not experience a change in the number of dependents and whose oldest child is between the ages of 16 and 18 in year 2. The coefficient is again small and insignificant. Finally, we restrict the sample to families with no changes in dependents whose oldest child is between 19 and 24 in year 2 (column 6), and again find no response. Placebo tests with similar age restrictions for families gaining a dependent also show no response. For families in the EITC phaseout range experiencing no change in dependents, the elasticity estimate is essentially zero (0.0002) and insignificant (Table 6, column 4). Richer placebo tests similar to those discussed above confirm this finding.

We then simulate alternative tax schedules and use our methodology to calculate elasticities under the assumption that the treatment groups faced the simulated schedules. In particular, we shift the tax schedule to the left by $10,000 and to the right by $10,000. Table 7 presents the results for treatment groups dropping and adding dependents, in both the middle-income and EITC ranges. None of the elasticity estimates for the two simulated tax schedules are significant, and many are wrong signed.

6.4 Decomposition of the Effects

The tax rate changes we consider affect marginal tax rates at the family level; the elasticity of family labor income is therefore the measure that best captures the total labor supply response to the change in the net-of-tax wage rate. In this section, we make use of the detailed labor supply data in the SIPP to gain a better understanding of the various components that comprise the total family response. In particular, we examine whether husbands or wives appear to be responding, and whether the response is on participation, hours, or other margins. We run unconditional regressions of change in labor income on change in tax price for husbands and wives separately and similar regressions for changes in participation and hours.

These coefficients should not be interpreted as individual elasticities because the tax rate changes affect both members of the household. Without making strong assumptions about the interaction of preferences within the household (for example, assuming that the husband's labor supply is fixed), we cannot recover the underlying elasticities for each individual. These estimates provide information about the composition of the aggregate response but should not be compared to existing estimates of participation and hours elasticities.

Table 8 presents the results for our four treatment groups: families in the \$30-75K range losing a dependent (Panel A), families in this range gaining a dependent (Panel B), families in the EITC phaseout losing a dependent (Panel C), and those gaining a dependent (Panel D). Overall, the responses are mixed: in most cases, there appears to be some response by both husbands and wives and along both participation and hours margins. Somewhat surprisingly, the effects for families in the \$30-75K range losing a dependent appear to be driven entirely by husbands' responses.

Note that there may be variation in the change in labor earnings that is not captured by participation and hours. Changes in family labor earnings may also reflect changes in effort or shifts between wage and non-wage compensation.

6.5 AGI and Taxable Income

Using tax panel data, we can estimate elasticities of AGI and taxable income. The elasticity of taxable income captures the full behavioral response to the tax rate change and, as emphasized by Feldstein (1999), is the relevant parameter for evaluating the efficiency costs of taxation. We focus on the \$30-75K income range because sample sizes in the EITC phaseout range and in higher income ranges are too small to estimate stable elasticities. Note again that this is entirely due to data constraints; our methodology could be used to provide estimates for these income ranges with richer data. We construct the sample as described in Section 3 and assign returns taxable income of \$1 if they report no taxable income and AGI is positive.

Table 9 presents these results. The elasticity estimate for wage income is 0.65, somewhat lower than the SIPP estimate of 0.97. The point estimates of the elasticities of AGI and taxable income are 0.51 and 4.8 respectively. These point estimates indicate substantial elasticities, broadly consistent with the SIPP estimates. However, the estimates are not statistically significant.

Recall that the SIPP elasticity of family labor income for families gaining a dependent is 0.23.

Running the same regression in the tax panel sample gives a very similar and strongly significant elasticity estimate: 0.16 with a standard error of 0.04. The results for AGI and taxable income are more mixed: the elasticity of AGI is close to zero and insignificant; the taxable income elasticity is similar to the wage income elasticity, but also insignificant. This may be because these income measures are more noisy than wage income, making it difficult to obtain precise estimates given our sample sizes.

6.6 Revisiting the Mulligan AFDC Experiment

The experiment closest in spirit to our work is Mulligan's estimation of intertemporal labor supply elasticities using AFDC recipients (Mulligan 1998). Mulligan uses PSID data to demonstrate that AFDC women increase their labor supply relative to non-AFDC women when their youngest children turn 18 and AFDC benefits are terminated. When we replicate Mulligan's methodology using the SIPP data, we find similar results. Upon closer examination, however, we find that this response appears to be a result of differential mean reversion in earnings rather than intertemporal substitution. We do find suggestive evidence in support of Mulligan's central argument: these changes do not appear to induce income effects because they are anticipated.

Mulligan compares labor supply of married mothers and unmarried mothers on welfare when their youngest child is 15 and when the child is 19. The termination of AFDC benefits upon the child's 18th birthday results in a decrease in the implicit tax on earnings for the unmarried mothers, increasing their effective wage by close to 50 percent. He finds that women who were AFDC recipients increase their labor supply, while non-AFDC women, who experience no change in implicit tax rates, do not change labor supply. Table A1 shows the Mulligan results; the estimates imply elasticities between 0.38 and 1.66.

We revisit the Mulligan experiment using the 1990-1996 SIPP panels (Table 10). We focus on changes in labor supply of mothers as their youngest children age from 17.5 to 18.5. We find that the share of non-AFDC mothers working does not change whereas the share of AFDC mothers working increases by 10 percentage points (80 percent). A potential concern with this comparison is that non-AFDC women may not be a good control group for AFDC women. We therefore consider two alternative control groups: first, AFDC women whose youngest children age from 16.5 to 17.5 and second, AFDC women whose second-youngest children age from 17.5 to 18.5. The first control group experiences no change in AFDC benefits, and the second control group experiences a decrease in the *level* of benefits, but not in the implicit tax rate since these families still have one qualifying child. Changes in labor supply for these control groups are very similar to the treatment group. The share of mothers working increases by 14 percentage points (89 percent) for the first control group and by 9 percentage points (43 percent) for the second control group. We find similar patterns for hours worked.

These results indicate that the observed effects can be more readily explained by mean reversion (mothers on AFDC are being observed at a time when earnings are particularly low) than by intertemporal substitution. We do find suggestive evidence that income effects, if any, are likely to be small: the second control group, which experiences a substantial reduction in income but no change in tax rates, has similar changes in labor supply as the treatment group and the first control group.

The Mulligan experiment illustrates more general concerns that differential income trends between treatment and control groups can bias estimates of the response to wage and tax changes. Similar concerns have been raised about studies identifying the elasticity of taxable income using income tax reforms (Slemrod 1996, Goolsbee 2000a, 2000b). Our empirical approach addresses these issues directly because treatment and control groups are very similar and because we include controls for base period income.

7 Conclusion

Our empirical strategy employs anticipated changes in marginal tax rates arising from changes in dependents to estimate the behavioral response to taxation. This strategy has certain advantages over strategies that examine the response to legislated changes in tax policy. Most importantly, the tax rate changes we study are fully anticipated and should induce no income effects. Therefore, we can interpret our estimates to be pure compensated elasticities. One reason why our estimates fall on the high side of conventional estimates may be because estimates of substitution elasticities from static models confound life-cycle wealth effects with intertemporal substitution effects.

The intertemporal elasticity of labor supply has important implications for understanding aggregate employment and output fluctuations. If workers respond to changes in wages over time by reallocating labor across periods, then productivity shocks can generate large cyclical fluctuations (Prescott 1986a). One of the central questions in the debate about these real business cycle models is whether the magnitude of intertemporal substitution is large enough to explain fluctuations of the size we observe in the United States economy. Our high-end estimates are consistent with the range of elasticities implied by calibration of real business cycle models to the data on macroeconomic fluctuations.[14]

Most of our estimates of taxable income elasticities are unfortunately imprecise as a result of data limitations; implementing our empirical methodology with the larger samples available in the Treasury tax panel would provide more definitive and precise estimates. In theory, however, our estimates of labor income elasticities from the SIPP data should be lower bounds on the true elasticities of taxable income. The high-end estimates then imply substantial deadweight loss from taxation.

[14] There are other points of contention between proponents and opponents of real business cycle models. See Summers (1986), and Mankiw (1989) for discussion.

Exploiting child-related tax rate changes provides a consistent identification strategy for estimating elasticities across the income distribution and over different periods of time. There is no reason to expect a constant elasticity across income groups, and understanding elasticity heterogeneity is critical for the design of optimal tax systems. In addition, diffusion of asset ownership, changes in tax law and changes in tax enforcement might all affect the ability of individuals to respond to tax incentives. As Slemrod (1998) and others have pointed out, it is unclear whether differing elasticity estimates in the literature are a result of differences in methodology, differential biases across policy experiments, or differences in behavioral responses at different points in time. We believe that this method has great promise for identifying true heterogeneity in elasticities across different income groups as well potential changes in these elasticities over time.

The empirical strategy can also be extended to examine the behavioral response to anticipated tax changes arising from other types of changes in family demographics. In current work, for example, we are exploring the labor supply decisions of individuals when their spouses retire. The retirement of a spouse can have large, anticipated effects on an individual's marginal tax rates. The magnitude of the tax change can be very different for otherwise similar families based on the precise composition of pre-retirement earnings, allowing us to control for the direct effect of a spouse's retirement on labor supply. Using age as a predictor for retirement, we can examine intertemporal labor supply decisions of the elderly, measure complementarities of spousal leisure, and predict the effects of certain Social Security reforms.

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Figure 1
Federal Marginal Tax Rates for Married Couples by AGI and Number of Dependents (2000)

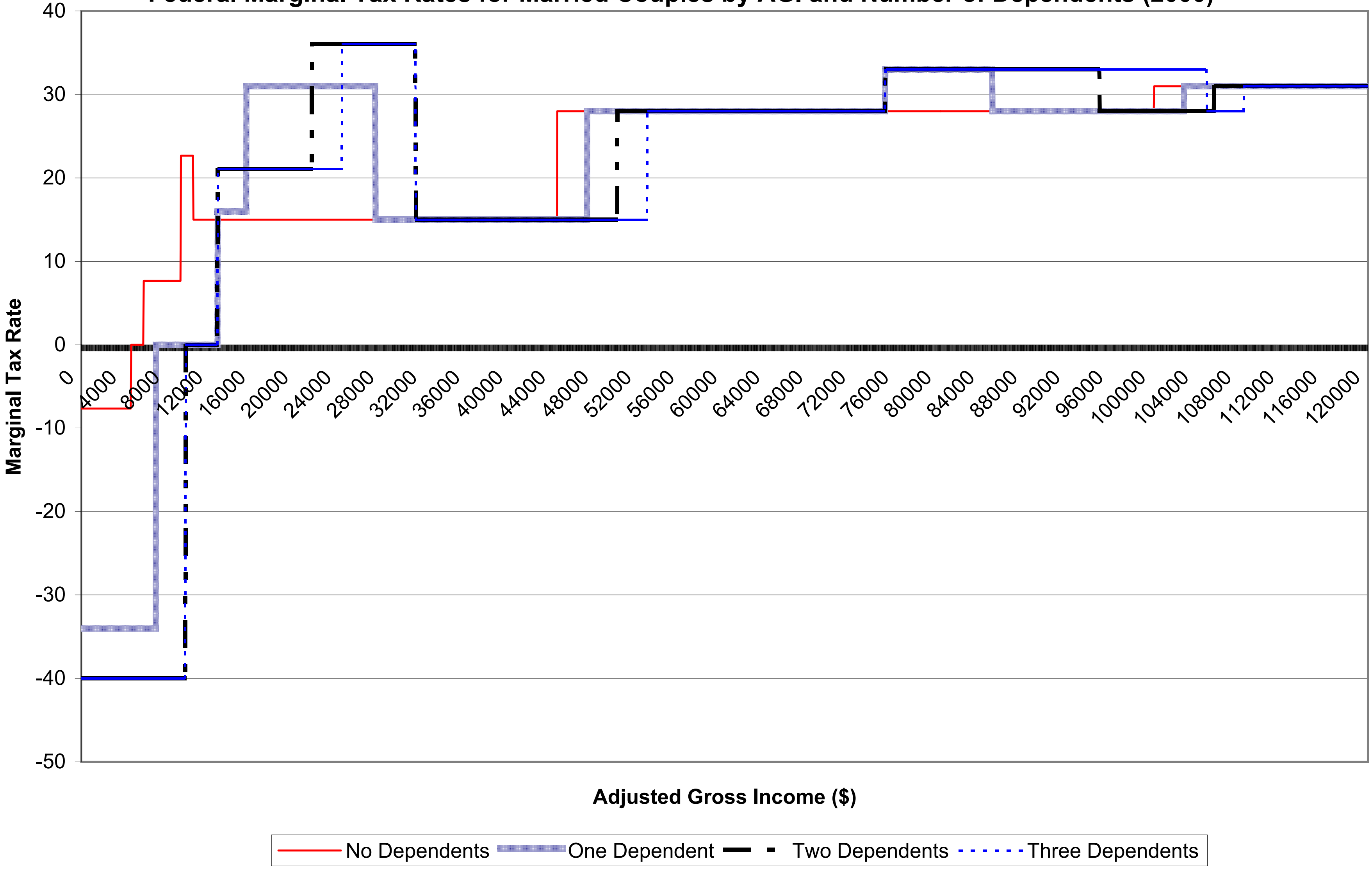

Figure 2: The Effect of a Change in Dependents on Marginal Tax Rates

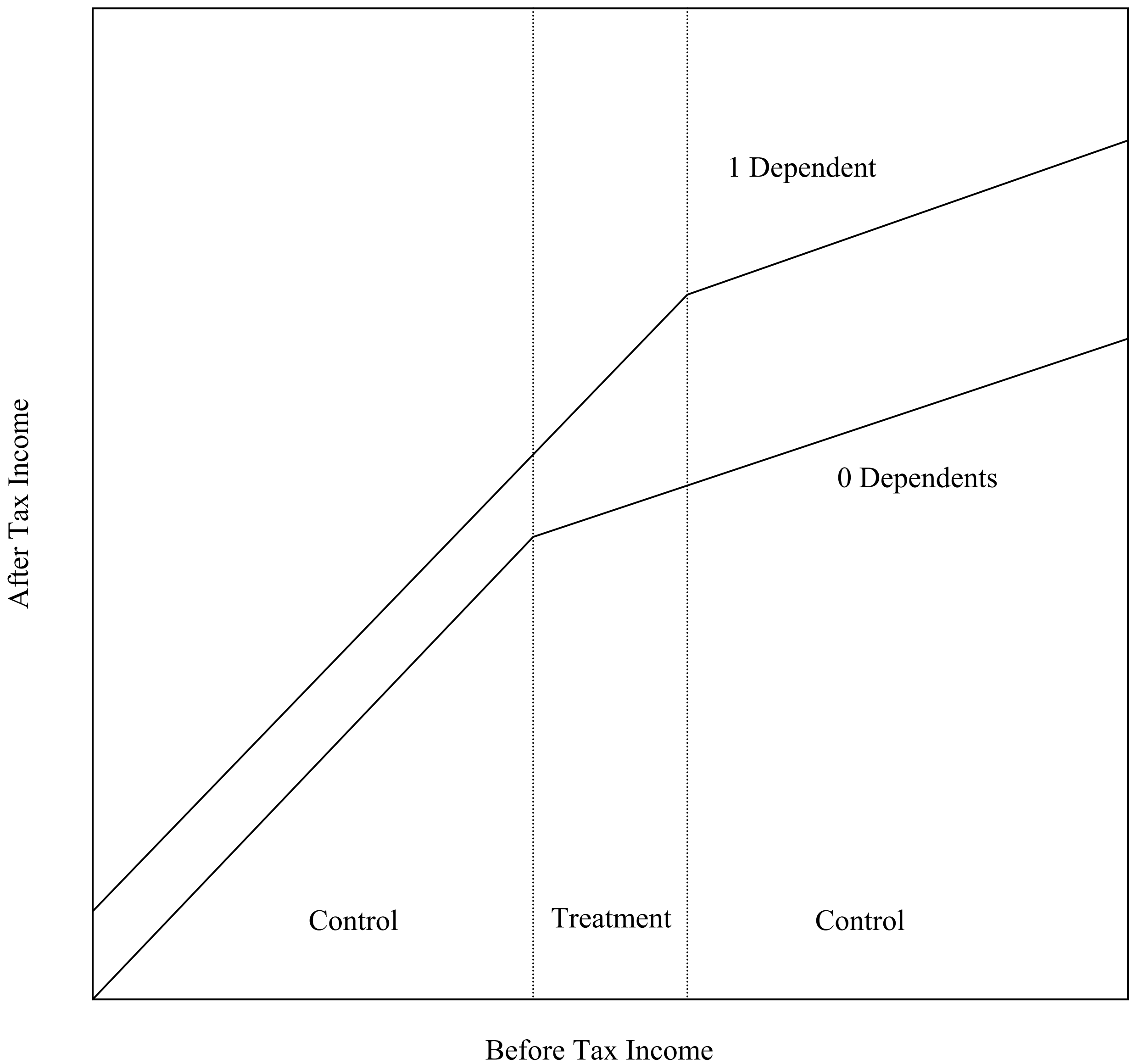

Table 1
Summary Statistics: SIPP sample

Income range: $30-75K	All	No change in # dependents	1 less dependent
Family earnings	51203	51409	50644
	(18950)	(18866)	(20947)
Age of mother	38.3	38.3	45.2
	(7.9)	(7.6)	(5.9)
Age of father	40.6	40.7	47.5
	(12.9)	(8.2)	(6.8)
Non-white (mother)	0.11	0.11	0.12
	(0.32)	(0.31)	(0.33)
Years of school (mother)	12.9	12.9	12.4
	(1.8)	(1.7)	(1.9)
# Year 2 dependents	1.75	1.79	0.97
	(1.07)	(1.04)	(1.05)
Observations	30469	25928	1954

Notes: Data: 1990-1996 SIPP panels. Includes married couples with children living in their home with income between $30,000 and $75,000. See text for details. We drop families who experience a change of more than 50 percentage points in the marginal tax rate between year 1 and 2. Family earnings refers to labor income earned by the mother and father only. Standard deviations in parentheses. Dollar figures adjusted to 2000$ using the CPI-U.

Table 2
Summary Statistics: Tax Panel

Income range: $30-75K	All	No change in # dependents	1 less dependent
Family earnings	50168	50270	50430
	(18351)	(17554)	(28937)
AGI	56365	56096	56759
	(39758)	(29789)	(31880)
Taxable Income	36689	36766	37734
	(30159)	(26139)	(28478)
# Year 2 dependents	1.31	1.26	1.04
	(1.22)	(1.21)	(1.15)
Observations	11731	10024	708

Notes: Data: Tax Panel 1987-1990. Includes married couples with children living in their home with income between $30,000 and $75,000. See text for details. We drop families who experience a change of more than 50 percentage points in marginal tax rate between year 1 and 2. Family earnings refers to labor income earned by the mother and father only. Standard deviations in parentheses. Dollar figures adjusted to 2000$ using the CPI-U.

Table 3
Predicted Tax Parameters

	SIPP		Tax Panel	
	No change in # dependents	1 less dependent	No change in # dependents	1 less dependent
Marginal tax rate (year 1)	29.5	29.4	27.9	27.7
	(5.6)	(5.6)	(5.4)	(6.0)
Predicted MTR (year 2)	29.2	29.8	27.9	28.6
	(5.6)	(6.0)	(5.4)	(6.1)
Taxes paid (year 1)	12687	12656	10732	10875
	(4465)	(4451)	(7595)	(9932)
Predicted taxes (year 2)	12348	12565	10732	11285
	(4128)	(4167)	(7595)	(9965)
Observations	25928	1954	10024	708

Notes: Source: 1990-1996 SIPP Panels and NBER Tax Panel 1987-1990. Tax parameters calculated using TAXSIM. Standard deviations in parentheses.

Table 4
Comparison of means: Families losing a dependent

	SIPP		Tax Panel	
	No change in tax rate	Change in tax rate	No change in tax rate	Change in tax rate
Earnings (year 1)	48616	59490	51193	52720
	(11746)	(12729)	(12413)	(11485)
Earnings (year 2)	49499	57759	50543	49355
	(21001)	(19170)	(29914)	(17318)
Δearnings	883	-1730	-649	-3365
	(18004)	(15307)	(27017)	(14986)
$\Delta\ln(1-\tau)$ (predicted)	0	-0.04	0	-0.13
		(0.11)		(0.08)
Observations	1683	271	640	68

Notes: Source: 1990-1996 SIPP Panels and NBER Tax Panel 1987-1990. Tax parameters calculated using TAXSIM. Family earnings refers to labor income earned by the mother and father only. Standard deviations in parentheses.

Table 5
Elasticity Estimates of Family Labor Income (SIPP)

	Treatment Group			Placebo Groups		
	(1)	(2)	(3)	(4)	(5)	(6)
$[\Delta\ln(1-\tau)]_{IV}$	0.972	0.958	1.022	-0.004	0.124	-0.094
	(0.307)*	(0.304)*	(0.333)*	(0.066)	(0.107)	(0.099)
ln(year 1 fam labor inc)	-0.078	-0.076	-0.074	-0.068	-0.051	-0.085
	(0.036)+	(0.036)+	(0.036)+	(0.013)**	(0.019)*	(0.020)**
# year 2 dependents		0.023	0.019	0.007	0.004	0.008
		(0.007)*	(0.007)*	(0.002)*	(0.004)	(0.003)*
Mother age			-0.002	-0.002	-0.002	-0.004
			(0.002)	(0.000)**	(0.002)	(0.001)**
Year dummies	yes	yes	yes	yes	yes	yes
Constant	0.742	0.701	0.795	0.736	0.575	1.032
	(0.389)+	(0.383)	(0.391)+	(0.140)**	(0.147)**	(0.217)**
Observations	1948	1948	1948	25900	3964	6071

Notes: Data: 1990-1996 SIPP panels. Includes all married couples with children living in their home with income between $30,000 and $75,000. The sample in columns 1-3 consists of those families who lost a dependent between years 1 and 2. The sample in column 4 consists of families who did not experience a change in the number of dependents. Column 5 restricts the column 4 sample to families whose oldest child is between 16 and 18 in year 2; column 6 restricts the column 4 sample to families whose oldest child is between 19 and 24 in year 2. Results are robust to richer controls for base year income and parents' ages. All regressions include person weights. Robust standard errors clustered by year in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%.

Table 6
Elasticity Estimates For Alternate Treatment Groups

	$30-75K	EITC range ($15-40 K)		
	(1) gain a dependent	(2) lose a dependent	(3) gain a dependent	(4) Placebo (no change)
$[\Delta \ln(1-\tau)]_{IV}$	0.232	0.089	0.116	0.0002
	(0.083)*	(0.304)	(0.040)*	(0.043)
ln(year 1 wage inc)	-0.125	-0.127	(0.080)	-0.172
	(0.024)**	(0.065)+	(0.038)+	(0.024)**
Year dummies	yes	yes	yes	yes
Constant	1.242	1.237	0.738	1.746
	(0.255)**	(0.664)	(0.384)+	(0.246)**
Observations	2367	1081	1351	14261

Notes: Data: 1990-1996 SIPP panels. Includes all married couples with children living in their home with income between $15,000 and $40,000. The sample in column 4 consists of families who did not experience a change in the number of dependents. All regressions include person weights. Robust standard errors clustered by year in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%.

Table 7
Tests of Identification Strategy Using Simulated Tax Schedules

Panel A: Families in the $30-75K Income Range

	losing a dependent		gaining a dependent	
	(1)	(2)	(3)	(4)
	+ $10K	- $10K	+ $10K	- $10K
$[\Delta \ln(1-\tau)]_{IV}$	-0.056	-0.458	-0.049	0.161
	(0.307)	(0.413)	(0.155)	(0.127)
ln(year 1 wage inc)	-0.086	-0.034	-0.119	-0.104
	(0.026)*	(0.020)	(0.002)**	(0.020)**
Year dummies	yes	yes	yes	yes
Constant	0.828	0.267	1.187	1.016
	(0.277)*	(0.216)	(0.189)**	(0.219)**
Observations	1934	1950	2351	2372

Panel B: Families in the EITC Phaseout Range ($15-40K)

	losing a dependent		gaining a dependent	
	(1)	(2)	(3)	(4)
	+ $10K	- $10K	+ $10K	- $10K
$[\Delta \ln(1-\tau)]_{IV}$	-0.041	0.508	0.299	-0.415
	(0.132)	(0.343)	(0.176)	(0.523)
ln(year 1 wage inc)	-0.014	-0.046	-0.024	-0.058
	(0.091)	(0.090)	(0.051)	(0.044)
Year dummies	yes	yes	yes	yes
Constant	0.079	0.406	0.174	0.517
	(0.933)	(0.916)	(0.515)	(0.457)
Observations	1051	1092	1309	1372

Notes: Data: 1990-1996 SIPP panels. +10 K (-10 K) elasticities calculated by assigning families $10,000 less (more) in income when calculating tax parameters to simulate a right (left) shift in the tax schedule of $10,000. All regressions include person weights. Robust standard errors clustered by year in parentheses. * significant at 5%; ** significant at 1%.

Table 8
Decomposition of the Effects

Panel A: Families in the $30-75K Income Range Losing a Dependent

	Husbands			Wives		
	(1) Δln(inc)	(2) Δparticip.	(3) Δln(hours)	(4) Δln(inc)	(5) Δparticip.	(6) Δln(hours)
$[\Delta \ln(1-\tau)]_{IV}$	1.566	0.302	0.283	-1.164	-0.658	-1.230
	(0.318)**	(0.353)	(0.385)	(0.687)	(0.466)	(1.148)
Observations	1886	1922	1797	1567	1922	1563

Panel B: Families in the $30-75K Income Range Gaining a Dependent

	Husbands			Wives		
	(1) Δln(inc)	(2) Δparticip.	(3) Δln(hours)	(4) Δln(inc)	(5) Δparticip.	(6) Δln(hours)
$[\Delta \ln(1-\tau)]_{IV}$	0.293	-0.067	0.051	0.204	0.187	0.118
	(0.163)	(0.065)	(0.118)	(0.464)	(0.081)+	(0.383)
Observations	2332	3250	2256	1792	2350	1752

Panel C: Families in the $15-40K Income Range Losing a Dependent

	Husbands			Wives		
	(1) Δln(inc)	(2) Δparticip.	(3) Δln(hours)	(4) Δln(inc)	(5) Δparticip.	(6) Δln(hours)
$[\Delta \ln(1-\tau)]_{IV}$	0.122	-0.091	0.031	-0.168	0.025	0.506
	(0.324)	(0.114)	(0.171)	(0.281)	(0.086)	(0.405)
Observations	980	1016	907	659	1016	657

Panel D: Families in the $15-40K Income Range Gaining a Dependent

	Husbands			Wives		
	(1) Δln(inc)	(2) Δparticip.	(3) Δln(hours)	(4) Δln(inc)	(5) Δparticip.	(6) Δln(hours)
$[\Delta \ln(1-\tau)]_{IV}$	0.125	-0.008	0.055	1.420	0.027	0.030
	(0.071)	(0.009)	(0.044)	(0.502)*	(0.053)	(0.644)
Observations	1312	1326	1264	769	1326	749

Notes: Controls for log husband's and wife's base year earnings, year dummies and a constant term are included in all regressions. The samples in columns (1), (3), (4), and (6) are for those individuals who did not change their participation decision. Robust standard errors clustered by year in parentheses. * significant at 5%; ** significant at 1%.

Table 9
Elasticity Estimates of Income (Tax Panel)

Panel A: Families in the $30-75K Income Range Losing a Dependent

	SIPP	Tax Panel		
	(1)	(2)	(3)	(4)
	Δln(wage inc)	Δln(wage inc)	Δln(AGI)	Δln(taxable inc)
$[\Delta \ln(1-\tau)]_{IV}$	0.972	0.649	0.505	4.755
	(0.307)*	(0.812)	(0.972)	(3.675)
ln(year 1 wage inc)	-0.078	0.021	0.047	-0.112
	(0.036)+	(0.018)	(0.118)	(0.252)
Year dummies	yes	yes	yes	yes
Constant	0.742	-0.286	-0.539	1.128
	(0.389)+	(0.190)	(1.274)	(2.705)
Observations	1948	702	705	704

Panel B: Families in the $30-75K Income Range Gaining a Dependent

	SIPP	Tax Panel		
	(1)	(2)	(3)	(4)
	Δln(wage inc)	Δln(wage inc)	Δln(AGI)	Δln(taxable inc)
$[\Delta \ln(1-\tau)]_{IV}$	0.232	0.158	-0.03	0.169
	(0.083)*	(0.040)**	(0.148)	(0.506)
ln(year 1 wage inc)	-0.125	-0.041	-0.021	0.188
	(0.024)**	(0.038)	(0.039)	(0.133)
Year dummies	yes	yes	yes	yes
Constant	1.242	0.410	0.210	-2.267
	(0.255)**	(0.412)	(0.425)	(1.436)
Observations	2367	819	821	821

Notes: Source: NBER Tax Panel 1987-1990. Families are assigned taxable income of $1 if taxable income is zero and AGI is positive. Robust standard errors clustered by year in parentheses. * significant at 5%; ** significant at 1%.

Table 10
Reanalyzing the Mulligan AFDC Experiment

	(1) non AFDC youngest child (17.5-18.5)	(2) Treatment youngest child (17.5-18.5)	(3) Control 1 youngest child (16.5-17.5)	(4) Control 2 2nd youngest child (17.5-18.5)
Before				
AFDC>0	0	1	1	1
Fraction hours>0	0.76	0.12	0.16	0.22
Annualized hours	1419.9	159.5	202.3	317.9
After				
AFDC>0	0	0.29	0.72	0.68
Fraction hours>0	0.76	0.22	0.30	0.31
Annualized hours	1433	340.7	452.4	434.6
Difference				
AFDC	0	-0.71	-0.28	-0.32
Fraction hours>0	0.00	0.10	0.14	0.09
%	-0.01	0.80	0.89	0.43
Annualized hours	13.1	181.2	250.1	116.7
%	0.01	1.14	1.24	0.37
Observations	1643	41	57	65

Notes: Source: 1990-1996 SIPP panels; authors' calculations.

Table A1
Mulligan AFDC Results

	(1) non AFDC	(2) AFDC	(3) AFDC female heads	(4) AFDC female heads afdc19=0	(5) female heads afdc19=0 hdage<62
Before (age 15)					
Fraction hours>0	0.69	0.30	0.28	0.32	0.35
Annualized hours	1051	348	314	382	387
After (age 19)					
Fraction hours>0	0.67	0.33	0.34	0.38	0.43
Annualized hours	1090	422	406	486	560
Difference					
Fraction hours>0	-0.02	0.03	0.06	0.06	0.08
%	-0.03	0.10	0.19	0.17	0.21
Annualized hours	39.00	74.00	92.00	104.00	173.00
%	0.04	0.19	0.26	0.24	0.37
Observations	1622	79	65	53	46

Notes: Source: Mulligan (1998); Table 4.