Identifying the Elasticity of Taxable Income

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Abstract

We provide a systematic analysis of the elasticity of taxable income to key economic and statistical decisions by drawing on publically available repeated cross-section data from the Annual Social and Economic Supplement of the Current Population Survey for survey years 1980-2009. We first estimate a canonical model with two-year matched panels of CPS data, where we find elasticity estimates in the range of 0.1 to 0.4, which are in line with tax-panel based papers. The estimates, however, are sensitive to variations in the specification including the time period investigated, additional demographics, the weighting scheme, and nonrandom sample selection. Next the data are employed as a pseudo panel where we apply a grouping instrumental variables estimator adopted from the labor supply literature. Identification of the grouping estimator hinges on the differential impact of numerous tax reforms across birth-year and education cohorts. The estimated elasticity of taxable income is generally negative under this alternative identification scheme. A complimentary analysis of male and female labor supply models yields positive compensated and uncompensated wage elasticities for the grouping estimator, suggesting that the additional variation in the underlying wage structure over and above tax reforms is a crucial source of identification not available in the standard elasticity of taxable income model.

Understanding how taxable income responds to marginal tax rates has become a focal outcome of interest to those concerned with tax reform and the optimal design of the income tax and transfer system (Saez 2001; Brewer, Saez, and Shepherd 2010). For example, given the shape of the income distribution, how responsive income is to changes in the marginal tax rate determines the optimal revenue-maximizing rate of taxation for high earners—that tax rate where revenue is likely to fall with incremental increases in the marginal tax rate—sometimes known as the Laffer tax based on the supply-side debates in the 1980s. This implies that the greater the behavioral response the lower the optimal top marginal tax rate. There now exists a fairly substantial literature estimating the so-called elasticity of taxable income (ETI), and while the range on these estimates has converged over time towards a modal estimate of 0.25, a clear consensus has yet to be reached (Auten and Carroll 1999; Gruber and Saez 2002; Kopczuk 2005; Heim 2009; Giertz 2010; Saez, Slemrod, and Giertz 2011).

The canonical ETI model regresses the log change in annual income on the log change in the net-of-tax share, defined as one minus the marginal tax rate, while controlling for initial period income in order to admit mean reversion in income (Auten and Carroll 1999). In this framework the log change in the net-of-tax share is likely to be endogenous, and as a solution to this problem, Gruber and Saez (2002) implemented a clever instrument that exploits changes in tax policy to identify the ETI. This is found by calculating the marginal tax rate a person would face in year two if income from year one increased solely due to inflation, and then taking the log ratio of one minus the predicted year-two rate over one minus the actual year-one rate. The validity of the instrument hinges on the identifying assumption that the synthetic year-two tax rate reflects changes in policy alone and not a behavioral response to the policy reform.

This identification scheme differs from approaches commonly found in the labor supply and taxation literature, which tends to either exploit the nonlinearity of the tax code, socioeconomic exclusion restrictions in the first stage, or time-series restrictions on the model control variables and error term (Hausman 1981; MaCurdy, Green, and Paarsch 1990; Ziliak and Kniesner 1999, 2005; Moffitt and Wilhelm 2000; Keane 2011). In the static cross-sectional model that regresses hours of work on the after-tax wage and virtual income, instruments for the wage and nonlabor income terms include variables found in a standard Mincer wage equation such as the age composition of children, interactions of age and education, higher powers of age and education, along with other variables such as family background. With access to panel data, identification is achieved by imposing restrictions on the time-series of the model error term (e.g. rational expectations), and using lags of the endogenous variables as instruments, along with higher-order powers of time-varying demographics. With repeated cross-sectional data, a Wald estimator on grouped data, typically grouped by date of birth or date-of-birth and education, has been a promising avenue of identification, even in the presence of lagged dependent variables (Angrist 1991; Blundell, Duncan, and Meghir 1998; Vella and Verbeek 2005). In each of these cases identification comes from the structure of the economic model and implicitly, but not explicitly, on policy reforms as in Gruber and Saez (2002), and thus the validity of the instrumental variable estimator hinges on whether the exclusion assumptions of the economic model hold.

In this paper we provide a systematic analysis of the elasticity of taxable income to key economic and statistical decisions by drawing on repeated cross-section data from the Annual Social and Economic Supplement of the Current Population Survey (CPS) for survey years 1980-2009. Twenty-five years ago Mroz (1987) published a seminal paper on how the wage effects of married women's labor supply varied dramatically depending on whether and how one controlled for nonrandom selection into work as well as to alternative exclusion restrictions in the instrument set for wages. This was followed up a decade later by Blundell, Duncan, and Meghir (1998) who showed that it was possible to exploit the differential effects of tax reforms and changes in the wage structure across cohorts, while controlling for nonrandom selection into work, to better identify the labor supply response to after-tax wages. Perhaps surprisingly, these insights have not been applied to the burgeoning literature on the elasticity of taxable income.

With few exceptions, the ETI literature over the past decade uses some variant of taxpayer panel data. The advantage of a panel of tax returns is the quality of data for measuring income and the attendant tax liability, coupled with the fact that panels follow the same person over time, making it readily transparent to model income changes controlling for mean reversion. This is weighed against limitations such as the fact that the tax data are often not publically available, they have limited demographic information, they do not necessarily capture the low end of the distribution because many poor have frequent non-filing episodes, and thus the data may suffer from attrition bias. On the contrary, publically available CPS data have extensive demographic information, and are less likely to suffer from problems of attrition. Indeed, Saez, et al. (2011) note that the benefits of panel analysis relative to repeated cross-section analysis may be exaggerated and that repeated cross-section analysis could provide a more robust and transparent approach to identifying the ETI. To our knowledge this is the first application of the CPS to the ETI literature.[1] Importantly, it is possible to link upwards of 50 percent of the sample across two waves permitting the application of Gruber-Saez type identification strategies. That

[1] Singleton (2011) uses income data from the Social Security Detailed Earnings Record matched with the CPS. This analysis attempts to identify the response of earnings (not income) to tax changes through the marriage penalty relief provision contained in the 2001 Economic Growth and Tax Relief Reconciliation Act. Demographic data obtained from the CPS are used to control for observable differences in his treatment and comparison groups.

is, CPS respondents are in the sample for four months, out for eight months, and in for four more months. This means it is possible to create a series of two-year panels, which enables us to estimate models of log income change that defines most of the ETI literature.

Using the predicted log change in the after-tax share, we closely replicate the base-case estimates of Gruber and Saez (2002) of an ETI just under 0.2 for broad income and 0.4 for taxable income over the 1979-1990 period. We then subject the baseline model to a number of alternative assumptions including time period, income controls, calculation of tax rates, heterogeneity across race and family structure, and possible non-random sample selection. For the latter, it is standard in the ETI literature to truncate low-income families from the sample, e.g. with incomes below $20,000 in Auten and Carroll (1999), or $10,000 in Gruber and Saez (2002), under the assumption that this truncation is likely to impart little bias in the ETI. However, it has long been known in labor supply that nonrandom selection into the labor force may be economically important, at least for some groups (Mroz 1987; Blundell, et al. 1998). Thus we append to our models a standard sample selection correction proposed by Heckman (1979). Across these additional specifications, we find a wide divergence of estimates, from large negative values to large positive values.

Next the data are employed as a repeated cross-section, but rather than using a difference in difference approach that defines treatment and comparison groups based upon income or tax brackets (top 1% versus the next 9% for example), we apply the grouping estimator proposed by Blundell, Duncan, and Meghir (1998) for the estimation of labor supply elasticities using tax reforms. In doing so, we exploit the differential impact of numerous tax reforms as well as the differential changes in income across birth-year and education cohorts. We consider how the ETI changes when we control for cohort effects, cohort and time effects, and lags of the dependent variable. With the comprehensive specification we consistently find negative estimates of the ETI. We then estimate standard labor supply models for men and women separately using the Blundell, et al (1998) identification strategy where we obtain results consistent with economic theory and within the range typically reported in the literature. This suggests that technological change and other factors affecting the pre-tax wage structure differentially across cohorts provide an important source of variation not present in the standard ETI framework. The variation offered by tax reforms in isolation over and above a generic time effect does not appear to be sufficient to identify the ETI in the cohort framework.

II. Estimating the Elasticity of Taxable Income

The canonical approach to estimating the effect of taxation on labor supply is to assume that a taxpayer maximizes a utility function over a composite consumption good c and hours of work h, $U(c,h)$, subject to a budget constraint of $c = wh + V + N - T(wh+N)$, where V is nontaxable nonlabor income, N is taxable nonlabor income, w is the pre-tax hourly wage rate, $T(.)$ is the tax function, and the price of consumption has been normalized to 1. Solving the optimization problem results in an optimal hours of work function of $h(w(1-\tau),N^v)$, where τ is the marginal tax rate and N^v is virtual nonlabor income $N + V + \tau wh - T(.)$, which is that level of compensation needed to make the worker behave as if they faced a constant marginal tax rate on all taxable income. In this framework, both the after-tax wage and virtual nonlabor income are treated as endogenous in estimation since the tax rate an individual faces is an implicit function of hours of work. Feldstein (1995) argued that this approach missed other behavioral responses to tax law changes such as shifting compensation from taxable to nontaxable income, or changes in the timing of compensation. Instead, he posited that workers preferences were over consumption and an income supply function, y, $U(c,y)$, and solving the revised optimization problem resulted in an income supply function of *y(1-τ,N^v)* that depends on the net-of-tax share (1-τ) and virtual nonlabor income. Like the labor supply predecessor, both the net-of-tax share and virtual incomes are treated as endogenous in estimation.

Gruber and Saez (2002) extended the Feldstein approach by motivating the income supply model within the context of the Slutsky equation in elasticity form, which relates how income supply responds to infinitesimal changes in net-of-tax shares and captures both substitution and income effects of tax law changes. For the empirical counterpart of their model they replaced the continuous time derivative from the Slutsky equation with a discrete time change from period *t-1* to *t*:

(1) $\Delta lny_{it} = \beta \Delta ln(1-\tau_{it}) + \gamma \Delta lnN_{it}^{v} + \varepsilon_{it}$,

where $\Delta lny_{it} = lny_{it} - lny_{it-1}$, $\Delta \ln(1-\tau_{it}) = \ln(1-\tau_{it}) - \ln\,(1-\tau_{it-1})$, and $\Delta lnN_{it}^{v} = lnN_{it}^{v} - lnN_{it-1}^{v}$. In log first difference form β is the compensated ETI. As Gruber and Saez found that γ was near zero, or that income effects were small, most of the subsequent literature has ignored income effects in their empirical applications and thus remain silent on distinguishing whether the ETI reflects compensated or uncompensated effects. We follow the recent work and ignore income effects for the ETI model, but return later to this issue when we present labor supply estimates.

The actual empirical model estimated in the literature is more akin to

(2) $\Delta lny_{it} = \beta \Delta ln(1-\tau_{it}) + \delta f(y_{it-1}) + x_{it}\theta + \mu_t + \varepsilon_{it}$,

where $f(y_{it-1})$ is some function of lagged income such as the log of income or a spline in income to control for mean reversion in income growth as well as trends in inequality, x_{it} is a vector of demographics such as marital status and age, and μ_t is a control for aggregate time effects such as a linear trend or time dummies. Because the standard OLS assumption that

$E[\Delta \ln(1-\tau_{it})\, \varepsilon_{it}] = 0$ is likely to be violated it is necessary to instrument for the endogenous regressor. Gruber and Saez (2002) propose an exactly identified model based on the instrument $\Delta \ln \widehat{(1-\tau_{it})} = \ln(1-\hat{\tau}_{it}) - \ln\,(1-\tau_{it-1})$, where $\hat{\tau}_{it}$ is the marginal tax rate that the individual would face in year *t* if income in year *t* differed from its *t-1* value only by an inflation adjustment. This synthetic marginal tax rate is valid provided that it only reflects changes in tax law and not potentially endogenous behavioral responses to the tax law changes. In practice, equation (2) is estimated via weighted instrumental variables, where the weight is the initial year income of the individual, though there is some debate in the literature on the merits of income weighting (Saez, et al. 2011). We examine the sensitivity of the ETI to weighting, to demographic controls, to heterogeneity of responses across demographic groups, and to how one controls for initial income.

A. A Cohort-Based Approach to Estimating the ETI

Instead of approximating the continuous time Slutsky equation with its discrete time analogue, an alternative to equation (1) is to specify a functional form for the static income supply model from the utility maximization problem

(3) $lny = \beta ln(1-\tau) + \gamma lnN^{v} + \varepsilon$

where all variables are now in log levels. This specification is akin to the typical static labor supply equation estimated in scores of papers, but with income replacing hours of work and the net-of-tax share replacing the after-tax wage. Again ignoring income effects, estimation of the model is complicated by the possible correlation of the net-of-tax share and the model error term. However, with access to repeated cross-sectional data on individuals *i* in time period *t* that can be grouped into cohorts *c* in time period *t*, we can make the following assumptions (Blundell, et al. 1998):

(A.1) $E[\varepsilon_{it}|c,t] = \alpha_c + \mu_t$

(A.2) $(E[\ln(1-\tau_{it})\,|c,t] - E[\ln(1-\tau_{it})\,|c] - E[\ln\,(1-\tau_{it})|t])^2 \neq 0$

Combined these assumptions say that provided unobserved differences in net-of-tax shares across groups can be characterized by a fixed cohort effect, α_c, and a fixed time effect, μ_t, (A.1) and that net-of-tax shares grow differentially across groups (A.2), we can apply weighted least squares to the transformed regression

(4) $ln\bar{y}_{ct} = \beta ln(1-\bar{\tau}_{ct}) + \alpha_c + \mu_t + \varepsilon_{ct}$,

where $\bar{y}_{ct}, 1-\bar{\tau}_{ct}$ are the cohort-year specific means of income and the net-of-tax share, and the weight in the regression is the number of observations in each cohort-year.[2] This is a standard within estimator but applied to cohort-mean data rather than individual level data.

B. **Accounting for Nonrandom Selection**

Estimating equation (4) will provide consistent estimates of the ETI under A.1 and A.2. However, as Blundell, et al. (1998) were interested in identifying the after-tax wage elasticity of labor supply among married women, a focal concern was possible nonrandom sample selection into work. The typical paper in the ETI literature truncates the data below some threshold--$20,000 in Auten and Carroll (1999), $10,000 in Gruber and Saez (2002)—and maintains the assumption that the data below the threshold are missing (conditionally) at random. This assumption precludes changes in labor force composition in response to tax reforms, and also drops many low-income families whose incomes tend to be highly volatile and increasingly so over the past three decades (Hardy and Ziliak 2011). To our knowledge this assumption has not

[2] Blundell, et al. (1998) note that instead one could implement the Wald estimator as an IV estimator on the individual-level data. Specifically, in the first stage one regresses the net-of-tax share on the cohort effects, time effects, and a full interaction of cohort and time effects, and then use the fitted value as an instrument in the second stage regression of log income on the log net-of-tax, along with the cohort and time effects, but not the interactions which serve as exclusion restrictions in the first stage.

been tested formally in the literature (though some authors have tested the robustness of results to alternative thresholds).

Continuing with our cohort specification in equation (4), we follow Blundell, et al. (1998) and revise assumptions A.1 and A.2

(A.1') $E[\varepsilon_{it}|c,t,z] = \alpha_c + \mu_t + \rho\lambda_{ct}$

(A.2') $(E[\ln(1-\tau_{it})\,|c,t,z] - E[\ln(1-\tau_{it})\,|c,z] - E[\ln(1-\tau_{it})\,|t,z] - \rho_\tau\lambda_{ct})^2 \neq 0$

where $\lambda_{ct}$is the inverse mills ratio ($\lambda_{ct} = \frac{\phi(.)}{\Phi(.)}$) evaluated at $\Phi^{-1}(P_{ct})$, P_{ct} is the sample proportion of a given cohort with incomes above the income threshold *z*, and Φ^{-1} is the inverse normal distribution. Identification of the ETI now requires that incomes change differentially across groups, over time, and over changes in sample composition above the threshold *z*. Implementation of this estimator is straightforward. First calculate the sample proportion of each cohort-year with incomes above the threshold *z*, and compute the inverse mills ratio, $\hat{\lambda}_{ct} = \lambda(\hat{P}_{ct})$. Second apply weighted least squares to

(5) $ln\bar{y}_{ct} = \beta ln(1-\bar{\tau}_{ct}) + \alpha_c + \mu_t + \rho\hat{\lambda}_{ct} + \varepsilon_{ct},$

where again the weights are the number of observations in a cohort-year and the means are computed for those with incomes above the threshold.

Importantly, we note that controlling for nonrandom selection is not an issue specific to the cohort model. Indeed it may apply full force to the Gruber-Saez type specification, whereby in this case one would append to equation (2) a control function for nonrandom selection above the income threshold *z*, $\hat{g}_{it}$. For example, as Gruber and Saez require income to be in excess of $10,000 in each year of their two-year panels, one approach to controlling for selection is to estimate a first stage probit model of the probability that income exceeds $10,000 (in real terms) in both years, and construct the inverse mills ratio using the index function from the estimated probit. In this case, $\hat{g}_{it} = \lambda(m_{it}\hat{\eta})$, where m_{it} is a vector of demographics and $\hat{\eta}$ are the probit coefficients.[3] Below we examine the sensitivity of the ETI to sample selection in both the Gruber-Saez and cohort models.

C. Accounting for Dynamics in Income

The cohort model in equation (4) is static, and unlike its income growth counterpart in equation (2), it may yield a spurious link between income and the net-of-tax share if there are aggregate trends in income inequality not captured by the group and time effects. Fortunately, under assumptions A.1 and A.2 it is trivial to introduce dynamics as

(6) $$ln\bar{y}_{ct} = \beta ln(1-\bar{\tau}_{ct}) + \pi ln\,\bar{y}_{ct-1} + \alpha_c + \mu_t + \varepsilon_{ct},$$

where $ln\bar{y}_{ct-1}$ is the lagged level of income for cohort *c* in time *t-1*. Vella and Verbeek (2005) show that provided that $E[\varepsilon_{ct}\alpha_c] = 0$, i.e. the cohort dummies are uncorrelated with the error term, then (weighted) OLS applied to (6) will yield consistent estimates of model parameters. Moreover, they note that under this assumption the so-called Nickell (1981) bias that plagues dynamic panel data models on individual level data does not apply to cohort data because as a within-cohort average of individual errors that are uncorrelated with the cohort dummies, the error term in (6) is asymptotically zero.

It is well known that when T=2 the within estimator in equation (4) is identical to a first difference estimator (and when $T\rightarrow\infty$ they converge). In finite samples, however, they may differ owing to measurement error or nonstationarity in the data process. This suggests that an alternative approach that at once captures dynamics and possible nonstationarity in income is to estimate

(7) $$\Delta ln\bar{y}_{ct} = \beta\Delta ln(1-\bar{\tau}_{ct}) + \delta f(\bar{y}_{ct-1}) + \Delta\bar{x}_{ct}\theta + \Delta\mu_t + \Delta\varepsilon_{ct},$$

[3] Another approach would be to estimate the first stage for each period, assume that the period-by-period decisions are independent, and then append two inverse mills ratios to equation (2).

which is the cohort-mean analogue to the Gruber-Saez model in equation (2). We apply the various estimators in equations (2)-(7) to examine the sensitivity of the ETI to alternative identification schemes.

III. Data

The primary economic and demographic information used in this paper comes from the Annual Social and Economic Supplement of the Current Population Survey (CPS) for calendar years 1979-2008 (interview years 1980-2009). The CPS contains rich data on labor and non-labor income as well as detailed family demographics - including those relevant for tax purposes (marital status, dependents, etc...). We employ the data first as a short panel by matching individuals across annual files, and then as a true repeated cross-section. Our sample consists of family heads ages 25 to 60, where a family is defined as two or more persons related by birth, marriage, or adoption. The following contains detailed information on the income and tax data used within this analysis as well as the matching procedure.

A. Income and Tax Data

Tax rates, as well as various income definitions, are estimated for each family in each year using the NBER *TAXSIM* program in conjunction with basic information on labor income, taxable nonlabor income, dependents, and certain deductions such as property tax payments and child care expenses.[4] Two marginal tax rate definitions are explored within the analysis. The first is the sum of the federal and state rate. The second is the sum of the federal, state and FICA rate. The sum of the federal and state tax rate appears more commonly used in the literature (Auten and Carroll 1999; Gruber and Saez 2002; Kopczuk 2005; among others) though some

[4] The CPS does not have information on certain inputs to the *TAXSIM* program such as annual rental payments, child care expenses, or other itemized deductions. We set these values to zero when calculating the tax liability.

have augmented the MTR to include FICA (Heim 2009). The federal and state taxes include the respective EITC code for each tax year and state, thus allowing for the possibility of negative tax payments. We assume that the family bears only the employee share of the payroll tax rate.

Three definitions of income serve as the dependent variable for this analysis. The first, broad income, was defined by Gruber and Saez (2002) to be the sum of all items composing total income on Form 1040 less capital gains and social security benefits. Using the CPS, we are able to construct this variable as total family income less social security income. Total family income includes earnings of the head (and spouse if present) as well unemployment compensation, worker compensation, social security, public assistance, retirement benefits, survivor benefits, interest income, dividends, rents, child support, alimony, financial assistance, and other income. Broad income is a wider measure than Adjusted Gross Income (AGI) and has the advantage of being consistently observed over time ((Gruber and Saez (2002), Kopczuk (2005)). Our second measure of income, AGI, is obtained directly from the NBER *TAXSIM* program. The final and most narrow income definition is taxable income, defined as Broad Income less estimated exemptions and deductions obtained from *TAXSIM* (Gruber and Saez 2002).

Given the limited nature of the tax data available in the CPS, we believe the AGI and taxable income definitions constructed using *TAXSIM* to be consistent over time and reasonably in line with those used in the literature.[5] Unless noted otherwise, all income data are deflated by the Personal Consumption Expenditure Deflator with 2008 base year.

B. Longitudinally Linking CPS Families

The CPS employs a rotating survey design so that a respondent is in sample for 4 months, out 8 months, and in another 4 months. This makes it possible to match approximately one-half

[5] We do not observe many adjustments (moving expenses, IRA deductions, health saving account deductions, student loan deductions, etc) used to arrive at AGI and taxable income. However, these are typically omitted in the construction of income definitions in order to achieve a definition consistent over the years.

of the sample from one March interview to the next. Following the recommended Census procedure we perform an initial match of individuals on the basis of five variables: month in sample (months 1-4 for year 1, months 5-8 for year 2); gender; line number (unique person identifier); household identifier; and household number. We then cross check the initial match on three additional criteria: race, state of residence, and age of the individual. If the race or state of residence of the person changed we delete that observation, and if the age of the person falls or increases by more than two years (owing to the staggered timing of the initial and final interviews), then we delete those observations on the assumption that they were bad matches. These additional criteria were very important prior to the 1986 survey year, but thereafter the five base criteria match most observations. Lastly, in accordance with the literature, we exclude individuals whose marital status changes from one year to the next as large changes in income unrelated to tax policy are expected for this group.

Prior to matching across years, we delete those observations with imputed income (Bollinger and Hirsch 2006), and we adopt the consistent set of income top codes constructed by Larrimore, et al. (2009) to mitigate the influence of changes in Census top code procedures starting in the mid 1990s (Ziliak, et al. 2011). There were major survey redesigns in the mid 1980s and mid 1990s so it is not possible to match across the 1985-1986 waves and the 1995-1996 waves. This yields an interrupted time series across 29 years with gaps in calendar years 1984-1985 and 1994-1995.

Declining match rates occur after the mid 1990s reflecting in part a rise in allocation within the CPS after adoption of CATI-CAPI interviewing. A possible concern with declining match rates is with sample attrition affecting our income series. Under the assumption that the probability of attrition is unobserved and time invariant (i.e., a fixed effect), then differencing the variable will remove the latent effect (Ziliak and Kniesner 1998; Wooldridge 2001). If there is a time-varying factor loading on the unobserved heterogeneity then differencing will not eliminate potential attrition bias. A conservative interpretation, then, is that data from matched CPS provides estimates of the elasticity of taxable income among the population of non-movers.

The matched data is used to construct an individual's change in income between year 1 and 2 as well as their change in net-of-tax shares.[6] Over the full period, 1979-2008, we obtain 206,665 observations when broad income is the dependent variables. For AGI and taxable income, we obtain 204,610 and 192,171 observations, respectively.[7] Because the change in net-of-tax rates is endogenous to the change in income, we follow Gruber and Saez (2002) and instrument the actual change in tax rates with a predicted tax change, $\Delta \ln \widehat{(1-\tau_{it})}$. To obtain $\hat{\tau}_{it}$, we inflate each individual's year one income by the increase in the PCE and run it through *TAXSIM* as year two income. Lastly when allowing for non-random selection, we require additional control variables (exclusion restrictions) to predict the probability of having income over $10,000. The set of variables selected for this purpose are state level variables including employment per capita, the poverty rate, minimum wage, gross state product, personal income per capita, and the TANF and SNAP benefits for a family of 3. These are obtained from the University of Kentucky's Center for Poverty Research's Welfare Database.[8]

C. Constructing Cohort Panels

When moving into the cohort analysis, we return to the initial CPS data set before matching. We drop individuals whose month in sample is greater than 4 to ensure there are no

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[6] It should be noted that we are using one year differences rather than the more commonly used three year differences. The use of one year differences may result in elasticities reflecting more income shifting behavior.
[7] These observations only include individuals with broad income exceeding $10,000 in year one. Sample sizes fall as the income definition narrows due to missing data or income values for which we cannot take logs (i.e zeros or negatives).
[8] See http://www.ukcpr.org/EconomicData/UKCPR_National_Data_Set_12_14_11.xlsx

repeat observations. This results in a repeated cross-section of over 400,000 individuals who are then grouped into fourteen 5-year birth cohorts and three education levels (less that high school, high school only, and more than high school) for a total of 42 5-year birth by education cohorts. We then construct a pseudo panel of these birth by education cohorts. The panel is unbalanced as cohorts age into and out of the sample. Because the consistency of the grouping estimator is based in part on the number of observations per cell being large, we follow Blundell et al. (1998) and drop cohort-education cells with fewer than 50 observations. For the 1979-1990 period, we obtain a total of 288 cohort years, and 718 cohort-year for the full 1979-2008 time period.

IV. Results

For purposes of comparison, we begin by estimating equation (2) using the same identification strategy and time period as Gruber and Saez (2002) but applied to the matched two-year CPS samples. We then explore the sensitivity of results to weighting, different tax rates, a longer time period, the inclusion of additional demographics, and nonrandom selection. Results for the estimation of equations (4), (5), and (6), the cohort models, are presented in Section B.

A. Gruber-Saez Identification Strategy with Matched CPS

Table 1 contains estimates of equation (2), where for ease of presentation we report only the elasticity of taxable income. All regressions contain controls for marital status as well as for each base year. The table has three rows, each corresponding to one of three selected income controls – the log of year one income, a 10 piece spline in log year one income, and indicators for deciles of year one income. For each of the income definitions (broad income, AGI, and taxable income), we estimate the model with the two different marginal tax rates both weighted and un-weighted.[9]

The base-case weighted estimates of the effect of federal and state tax rates on broad income (the income definition which we can most closely replicate) are remarkably close to those reported in Gruber and Saez (2002). When applying their preferred model containing the ten piece income spline, we obtain a broad income ETI of 0.168, which is quite close to the 0.192 they obtain from tax panel data when looking at one year changes. As expected, income responsiveness to changes in the marginal tax rates increases as the income definition is narrowed toward AGI and taxable income.

[Table 1 here]

Table 1 illustrates the sensitivity of estimates to weighting, choice of marginal tax rate, and income controls. With the exception of AGI, weighting tends to raise the estimated ETI. In all cases one observes a larger elasticity estimate when the marginal tax rate is the sum of the federal and state rates as opposed to also including FICA. This result is somewhat surprising in light of the focus on high-income taxpayers in the literature, but whose earnings exceed the Social Security earnings cap. Interestingly, when replacing the income spline function with income decile dummies, a less restrictive approach, we obtain negative ETIs for the 1979-1990 period. Gruber and Saez expressed concern that added flexibility in income controls may threaten identification, and the evidence in Table 1 based on income deciles seems to bear this out.

[Table 2 here]

Table 2 contains the same analysis for the full 1979-2008 period. When moving to the longer time frame we observe larger ETI estimates in most instances. For example, the elasticity

[9] We follow Gruber and Saez (2002) and censor our income weights at $1 million.

of broad income with respect to the net of federal and state tax rate increases from 0.168 to 0.217 while the comparable elasticity of AGI increases from 0.181 to 0.240, and for taxable income from 0.214 to 0.275 – all increases of approximately 30%. We also note that across the three dependent variables the weighted ETI estimates that include FICA are now positive and in the range of 0.10 to 0.13. This suggests that within the Gruber-Saez framework the longer time span that includes several additional tax reforms provides added variation over and above secular trends in inequality to identify the ETI.

A.1 Demographics and Heterogeneity in the ETI

Table 3 presents the results for the full sample period when the model is augmented with additional demographics affecting both the growth rate in income as well as the responsiveness to changes in tax rates. We use the weighted regression model for the federal and state net of tax share and the 10-piece spline in initial income in Table 2 as the benchmark model. The additional demographic controls include age, age squared, gender (female), race (controls for black and other, with white as the omitted group), education (high school and more than high school, with high school dropout as omitted group), number of children under 6, number of children under 18, and state fixed effects. The table contains four columns for each of the income definitions. The first column shows the elasticity once these additional factors are added to the model as controls for income growth. In the second through fourth columns, indicators for race, gender, and achieving more than a high school education are additionally interacted with the change in net of tax shares and log income, respectively. The inclusion of the additional demographics in column (1) reduces the elasticity estimates for broad income and taxable income by over 40 percent while reducing the elasticity estimates for AGI by approximately 30 percent, highlighting the interaction between deductions from dependents and responsiveness to tax changes (Kopczuk 2005). In the models where indicators for an African American or female head are interacted with the change in net of tax shares we obtain slightly larger overall elasticity estimates but negative interaction estimates. Indeed, the total effect of African Americans and female heads is negative (the sum of the direct effect and the interaction), and statistically significant.

[Table 3 here]

In column (4) we interact the net-of-tax share with an indicator for more than high school. The literature has previously found that the ETI increases as one moves up the income distribution, suggesting that it is high income taxpayers driving the results. A concern, however, is that these results may be biased for the same reasons we described earlier on selection and mean reversion. As an alternative, the consumption and labor supply literatures have frequently used education as a proxy for permanent income (Attanasio and Weber 2010). Although education decisions may be affected by tax policy, with our sample of older individuals most formal education is completed and not likely to be affected by contemporaneous tax policy. The results show that allowing for a heterogeneous response based on education results in a significantly large and positive ETI for those with more than high school, suggesting it is this subpopulation of highly educated that is driving the overall positive estimated elasticity.

A.2 Nonrandom Selection

Table 4 presents estimates for the models reported in Tables 2 and 3 with the additional control for nonrandom selection on unobservables above the $10,000 threshold. That is, in the upper panel we present weighted estimates with the 10-piece spline akin to those in Table 2 but with the addition of the inverse mills ratio, while in the lower panel we present estimates akin to those in Table 3 where we control for selection on observables via additional demographics as well as for unobservables via the inverse mills ratio. In the first step, we estimate a probit model of the probability that income exceeds $10,000 in both years, and construct the inverse mills ratio using the index function from the estimated probit. In this case, $\hat{g}_{it} = \lambda(m_{it}\hat{\eta})$, where m_{it} is a vector of demographics and $\hat{\eta}$ are the probit coefficients. We then estimate equation (2) for the full sample appending $\hat{g}_{it}$ to control for non-random selection, using both individual-level demographics and state-level socioeconomic variables described in the Data section as exclusion restrictions to assist in identifying the selection term in the top panel, and just state-level variables in the bottom panel (since those individual-level demographics are entered directly in the regression model in the lower panel).

[Table 4 here]

Comparing the middle row to Table 2 to the first row of Table 4 suggests that controlling for nonrandom selection reduces the ETI by 30-40% (e.g. 0.217 in Table 2 to 0.151 in Table 4), much like we saw in comparing estimates across Tables 2 to 3 with the addition of observed demographics. Indeed, adding the individual-level demographics in the bottom panel leaves the estimated ETI little changed (in some cases it is lower, and in others it is higher). Even though the inverse mills ration is generally statistically significant, the estimates in Tables 3 and the bottom of Table 4 suggest that controlling for selection on observables via demographics is sufficient for estimating the ETI.

A.3 Synthetic Cohort Instrument

Before moving into the repeated cross-section cohort models, we consider an alternative identification strategy within the Gruber-Saez framework. Though the synthetic tax rate instrument used in the previous analysis has been most common in the ETI literature, it has not gone without criticism. It is well acknowledged that this instrument, which is a function of taxable income in year t-1, (y_{it-1}), is likely to be correlated with the error term. Researchers have attempted to remedy this problem by including different controls for ln(y_{it-1}). We instead utilize an alternative approach by replacing the synthetic tax rate instrument, $\Delta\ln\widehat{(1-\tau_{it})} = \ln(1-\hat{\tau}_{it}) - \ln(1-\tau_{it-1})$, with an instrument based on our cohort grouping strategy. Specifically, we instrument the change in an individual's net-of-tax share with the change in the net-of-tax share faced by the birth-year education cohort to which they belong, $\Delta\ln(1-\tau_{ct}) = ln(1-\tau_{ct}) - \ln(1-\tau_{ct-1})$.

Results are shown in Table 5. Here we obtain a broad income ETI of approximately .02 (though it is statistically insignificant). For AGI and taxable income we find statistically significant but economically small ETI estimates ranging between 0.05-0.10.[10] This suggests that using plausibly more exogenous instruments to identify the ETI results in estimates that are small, but within the range found in the literature.[11]

[Table 5 here]

B. Repeated Cross-Section Cohort Models

Next we present the results obtained when applying the cohort models to the repeated cross-section samples. This involves invoking assumptions (A.1) and (A.2) for the estimation of equation (4) and assumptions (A.1') and (A.2') for the estimation of equation (5). Recall equation (4) is the weighted regression of cohort-year specific means of income on cohort-year specific means of the net-of tax-share while equation (5) is simply equation (4) augmented to account for non-random selection. Tables 6-8 are is divided into two panels—one for the 1979-

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[10] The use of the cohort instrument results in strong first stages with F-tests well over 200.

[11] After completing the paper we became aware of a dissertation by Caroline Weber (2011) who addresses some of the issues we cover in this section. Specifically she demonstrates the Gruber-Saez instrument remains endogenous regardless of the additional income controls used, and thus instead suggests using lags of ln(y(it-1)) to construct the predicted tax rate instrument. With only two years of individual level data in the matched CPS we cannot use further lags as instruments.

1990 period, and one for the full period (1979-2008). The first column of each panel labeled "no selection" contains estimates of equation (4), the second column labeled "with selection" contains estimates of equation (5), the third column labeled "no selection with demographics" is like the first column but with the addition of the cohort-group by year mean values of demographics used in Tables 3 and 4 (e.g. fraction married, number of children by age group, race), the fourth column "with demographics and selection" contains both demographics and the inverse mills ratio, and the fifth column "no selection or year effects" is a parsimonious model that contains only cohort fixed effects and no time effects, demographics, or selection.

[Tables 6-8 here]

Table 6 contains the results for broad income, while Tables 7 and 8 present the parallel estimates for AGI and taxable income, respectively. In all three tables for the 1979-1990 period we find strong evidence of nonrandom selection as well as negative estimates of the ETI. In the last column where we drop the time effects, we actually find an estimate of the ETI akin to the Gruber-Saez type estimates in Table 1 (0.163-0.466 across Tables 6-8). When we extend the analysis to the full sample period, we find that the ETI is negative in all specifications. In results not tabulated, if we instead use a linear trend in lieu of time dummies, we again find negative estimates of the ETI. That we find a positive ETI in the earlier period without time effects, but not the full period, indicates that the tax reforms of the 1980s yielded substantial variation via consolidation of marginal tax brackets not present in the 1990s and 2000s reforms. This suggests that there is not adequate variation across birth-year by education cohorts over time in net-of-tax shares over and above a generic time effect.

[Table 9 here]

Table 9 takes a step further with the cohort models and presents estimates of the dynamic cohort model of equation (6) in specification (1) of the table and first difference estimates of equation (7) in specification (2) of the table. Again, without exception, all estimates of the ETI are large in size and negative in sign.

C. A Cohort-Based Labor Supply Model

As discussed previously, identifying the elasticity of taxable income with the grouping estimator requires variation in the net-of-tax share over and above the fixed cohort and time effects. This is a narrower source of variation than that generally relied upon to identify models of labor supply. Here we estimate labor supply models making use of variation arising from the same tax reforms as well as technological change and other factors differentially impacting the pre-tax wage structure across cohorts.

Estimation of the hours worked equation requires two steps. The first step is to estimate the reduced-form prediction equations for net wages, virtual income, labor force participation, and having an income greater than \$10,000.[12] Let the vector of first stage dependent variables be denoted by $d_{it}^{r} = [lnw_{it,}N_{it}^{v}, P_{it}, I_{it}]$, and the vector of covariates as Z_{it}^{r}. Then the reduced-form equations are

(8) $$d_{it}^{r} = Z_{it}^{r}\rho + \delta_{t}^{r} + \delta_{j}^{r} + \delta_{s}^{r} + \delta_{t}^{r} \otimes \delta_{j}^{r} + \upsilon_{it}^{r},$$

where r denotes the equation being estimated (i.e. net wage, virtual income, participation, income greater than \$10,000), δ_{t}^{r} is a time effect, δ_{j}^{r} is a cohort effect, δ_{s}^{r} is a state effect, $\delta_{t}^{r} \otimes \delta_{j}^{r}$ are interactions of cohort and time effects,and υ_{it}^{r} is an error term assumed to be uncorrelated with the observed covariates and latent heterogeneity.

[12] A prediction equation for income greater than \$10,000 is not typical in the labor supply literature. It is included here to keep the labor supply analysis as parallel as possible to the ETI analysis.

Following Blundell et. al, (1998), we estimate the equations for the after-tax wage and virtual income via least squares on the sample of workers only, saving the fitted residuals $\hat{\upsilon}_{it}^{w}$ and $\hat{\upsilon}_{it}^{N^{\upsilon}}$. These residuals will be included in the hours worked equation to control for the endogeneity of the after tax wage and virtual income. The reduced form equations for employment and income greater than \$10,000 are estimated via probit maximum likelihood on the sample of workers and non-workers for all income levels, and those with income greater than \$10,000, respectively.[13] The parameters of these equations are used to construct sample selection correction terms. We then estimate the conditional hours worked equation via OLS for workers only appending various controls for selection and endogeneity,

$$(9) \qquad h_{it} = \alpha + \beta lnw_{it} + \gamma N_{it}^{\upsilon} + X_{it} + \delta_t + \delta_j + \delta_s + \theta_w \upsilon_{it}^{w} + \theta_N \upsilon_{it}^{N^{\upsilon}} + \delta \hat{\lambda}_{it} + \varepsilon_{it}.$$

This analysis is a first application of the grouping estimator that we are aware of to the wider population of men and women in the U.S. (Blundell, et al. (1998) focused on married women in the U.K.).

Table 10 contains the results for the hours worked equation separately for men and women. The first column presents the results for the full sample of workers (ignoring the \$10,000 threshold). In the second specification, we append two selection terms, one for the decision to work and one for annual broad income greater than \$10,000. In the third specification we append only one inverse mill ratio constructed from a first stage prediction equation for simultaneously working *and* having income greater than \$10,000. Each specification controls for marital status, the number of kids under age 6, the number of kids under age 18, race, and cohort, year, and state fixed effects.

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[13] Variables needed for the estimation of the labor supply models are constructed as follows. Wages are constructed as the ratio of annual earnings to annual hours of work (annual weeks worked times usual hours per week). Our earnings variable includes income from self employment. We retain self employed individuals to keep the samples used across the ETI and labor supply analyses consistent. The after-tax wage is constructed using the marginal tax rates described above. Observations with wages exceeding \$500 per hour are dropped from the sample.

[Table 10 here]

All models produce positive uncompensated wage effects, and virtual non-labor income effects are found to be negative for men and negative or statistically zero for women. There is substantial evidence that it is important to control both for the endogeneity of wages and virtual income, as well as nonrandom selection. The bottom panel contains the corresponding wage and income elasticities evaluated at the mean of hours. For males we obtain uncompensated wage elasticities between 0.04-0.07 and compensated wage elasticities of about 0.16. For women, compensated wage elasticities range from 0.10-0.19, and given the near zero income effects, the uncompensated elasticities are similar in magnitude. Both sets of estimates are in accord with the canonical theory and well within the range found in the survey on labor supply and taxation by Keane (2011). The significant work disincentive effects of taxation in Table 10 suggests that the additional variation in the pre-tax wage structure provides much needed power to identify the model that is not available in the standard ETI empirical identification scheme relying on tax reforms alone.

V. Conclusion

We present new estimates of the elasticity of taxable income using repeated cross sectional data from the Current Population Survey. With the exception of Moffitt and Wilhelm (2000), the literature has relied upon taxpayer panel data to identify the ETI. Although the merits of tax data in terms of data quality are many, they do suffer from some shortcomings including limited demographic information. We find that using two-year matched panels from the CPS, along with the synthetic instrumental variable estimator popularized by Auten and Carroll (1999) and Gruber and Saez (2002), enables us to replicate estimates from the literature on tax panels, with a typical estimate in the range of 0.1 to 0.4 depending on measure of income. We also find substantial heterogeneity in the ETI, showing that the positive estimates seem to be driven by family heads with more than a high school education.

Identification of the ETI, however, is quite fragile. If we use a flexible specification to control for regression to the mean effects, then the ETI becomes negative. If we do not weight the regression model by initial income then the estimated ETI is generally zero. If we include FICA as part of the net-of-tax share then the estimated ETI tends toward zero. If we fail to control for selection, whether on observables or unobservables, leads to an upward bias in the ETI by about one-third. And finally, we find that if we use an alternative identification strategy based on a grouping estimator adopted from the labor supply literature, the estimated ETI is negative. On the contrary, the same grouping estimator yields compensated wage elasticities of labor supply for men and women of 0.16-0.19, suggesting that there is scope for welfare-improving tax reforms. The labor supply model is identified not only from differential effects across cohorts in tax policy, but also from changes in the pre-tax wage structure. Our take away is that reliance upon the variation arising solely from tax reforms does not appear sufficient for identifying key parameters necessary for welfare analysis.

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Table 1. Estimates of Elasticity of Taxable Income with Synthetic Tax Rate Instrument, 1979-1990

	Broad Income				AGI				Taxable Income			
	Fed + State		Fed +State+ Fica		Fed + State		Fed +State+ Fica		Fed + State		Fed +State+ Fica	
	weighted	unweighted	weighted	unweighted	weighted	unweighted	weighted	unweighted	weighted	unweighted	weighted	unweighted
ln(income)												
Elasticity	0.145**	0.090**	-0.020	-0.004	0.308***	0.314***	0.067	0.194***	0.245***	0.106*	0.011	-0.016
	(0.060)	(0.046)	(0.048)	(0.038)	(0.075)	(0.064)	(0.055)	(0.056)	(0.070)	(0.063)	(0.055)	(0.054)
Spline of ln(income)												
Elasticity	0.168**	-0.103**	0.085	-0.050	0.181**	0.025	0.077	0.019	0.214***	-0.157**	0.083	-0.110**
	(0.067)	(0.045)	(0.052)	(0.038)	(0.071)	(0.061)	(0.055)	(0.049)	(0.081)	(0.067)	(0.062)	(0.055)
Deciles of ln(income)												
Elasticity	-0.365***	-0.306***	-0.317***	-0.217***	-0.333***	-0.135**	-0.315***	-0.110**	-0.432***	-0.656***	-0.389***	-0.507***
	(0.061)	(0.043)	(0.049)	(0.036)	(0.066)	(0.059)	(0.053)	(0.049)	(0.072)	(0.069)	(0.057)	(0.056)
Observations	86272	86272	86272	86272	85317	85317	85317	85317	85683	85683	85683	85683

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1 All regression include controls for marital status and initial year. Income range is broad income greater than $10,000.

Table 2. Estimates of Elasticity of Taxable Income with Synthetic Tax Rate Instrument, 1979-2008

	Broad Income				AGI				Taxable Inocme			
	Fed + State		Fed +State+ Fica		Fed + State		Fed +State+ Fica		Fed + State		Fed +State+ Fica	
	weighted	unweighted	weighted	unweighted	weighted	unweighted	weighted	unweighted	weighted	unweighted	weighted	unweighted
ln(income)												
Elasticity	0.202***	0.016	0.098***	0.013	0.247***	0.116***	0.133***	0.108***	0.224***	-0.014	0.108***	-0.004
	(0.034)	(0.022)	(0.027)	(0.019)	(0.037)	(0.028)	(0.030)	(0.025)	(0.040)	(0.033)	(0.032)	(0.029)
spline of ln(income)												
Elasticity	0.217***	0.003	0.113***	0.010	0.240***	0.069**	0.126***	0.055**	0.272***	0.044	0.139***	0.044
	(0.037)	(0.023)	(0.028)	(0.019)	(0.039)	(0.028)	(0.030)	(0.024)	(0.045)	(0.036)	(0.035)	(0.030)
Deciles of ln(income)												
Elasticity	-0.009	-0.071***	-0.088***	-0.055***	0.043	0.068**	-0.055*	0.047*	-0.020	-0.246***	-0.115***	-0.198***
	(0.035)	(0.022)	(0.027)	(0.019)	(0.038)	(0.029)	(0.030)	(0.024)	(0.043)	(0.036)	(0.034)	(0.030)
Observations	198428	198428	198427	198427	196450	196450	196449	196449	196486	196486	196485	196485

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1 All regression include controls for marital status and initial year. Income range is broad income greater than $10,000.

Table 3. Demographics and Heterogeneity in the Elasticity of Taxable Income

	Broad Income				AGI				Taxable Income			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Elasticity	0.148***	0.177***	0.210***	-0.001	0.170***	0.193***	0.272***	0.048	0.152***	0.198***	0.224***	-0.139**
	(0.035)	(0.036)	(0.045)	(0.044)	(0.038)	(0.039)	(0.049)	(0.050)	(0.043)	(0.044)	(0.053)	(0.056)
Elasticity*African American		-0.427***				-0.303**				-0.635***		
		(0.117)				(0.136)				(0.152)		
Elasticity*Female			-0.257***				-0.363***				-0.292***	
			(0.067)				(0.075)				(0.082)	
Elasticity*more than high school				0.291***				0.244***				0.532***
				(0.064)				(0.073)				(0.077)
Observations	198428	198428	198428	198428	196450	196450	196450	196450	196486	196486	196486	196486

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1.The net-of-tax share is federal+state. All regressions are weighted, include a 10 piece income spline, and demographic controls of age, age squared, gender (female), race (controls for black and other, with white as the omitted group), education (high school and more than high school, with high school dropout as omitted group), number of children under 6, number of children under 18, and state fixed effects. Income range is broad income over $10,000, and the sample period is 1979-2008.

Table 4. Estimates of the Elasticity of Taxable Income with Synthetic Tax Rate Instruments, Allowing for Non-random Sample Selection

	Broad Income		**AGI**		**Taxable Income**	
	Fed+State	Fed+State+Fica	Fed+State	Fed+State+Fica	Fed+State	Fed+State+Fica
With Selection						
spline of ln(income)						
Elasticity	0.151***	0.061**	0.164***	0.066**	0.204***	0.089***
	(0.036)	(0.028)	(0.038)	(0.030)	(0.044)	(0.034)
Inverse Mills Ratio	-1.227***	-1.209***	-1.256***	-1.237***	-1.281***	-1.260***
	(0.033)	(0.032)	(0.033)	(0.032)	(0.050)	(0.049)
With Selection & Controlling for Additional Demographics						
spline of ln(income)						
Elasticity	0.149***	0.081***	0.172***	0.093***	0.153***	0.083**
	(0.035)	(0.027)	(0.038)	(0.030)	(0.043)	(0.034)
Inverse Mills Ratio	0.109**	0.105**	0.144***	0.138***	0.089	0.083
	(0.053)	(0.053)	(0.050)	(0.049)	(0.088)	(0.087)
Observations	198428	198427	196450	196449	196486	196485

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. All models include dummies for marital status. All regressions are weighted and include 10 piece income spline. All regressions are weighted and include controls for marital status and initial year. Income range is broad income greater than $10,000, and the sample period is 1979-2008.

Table 5. Estimates of the Elasticity of Taxable Income Using a Cohort-Mean Synthetic Instrument

	Broad Income		AGI		Taxable Income	
	fed+state	Fed+state+fica	Fed+state	Fed+state+fica	Fed+state	Fed+state+fica
ln(income)						
Elasticity	0.022	0.026	0.091***	0.103***	-0.007	0.018
	(0.024)	(0.021)	(0.030)	(0.027)	(0.035)	(0.030)
spline of ln(income)						
Elasticity	0.011	0.022	0.060*	0.052**	0.056	0.061*
	(0.025)	(0.021)	(0.032)	(0.026)	(0.038)	(0.031)
Observations	197939	197938	195976	195975	196020	196019

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1 All regression include controls for marital status and initial year. Income range is broad income greater than $10,000, and the sample period is 1979-2008.

Table 6. Cohort-Based Estimates of the Elasticity of Taxable Income, Dependent Variable is Broad Income

	1979-1990					1979-2008				
	No Selection (Eq 4)	With Selection (Eq 5)	No Selection With Demographics	With Demographics and Selection	No selection or Year effects	No Selection (Eq 4)	With Selection (Eq 5)	No Selection With Demographics	With Demographics and Selection	No selection or Year effects
state+federal tax:										
Elasticity	-1.314***	-1.196***	-1.016***	-0.990***	0.565***	-1.787***	-1.532***	-1.249***	-1.221***	-0.082
	(0.152)	(0.142)	(0.170)	(0.158)	(0.113)	(0.104)	(0.099)	(0.108)	(0.104)	(0.090)
Inverse Mills Ratio		-0.593***		-0.533***			-0.536***		-0.370***	
		(0.093)		(0.084)			(0.058)		(0.060)	
state+federal+fica tax:										
Elasticity	-1.116***	-1.008***	-0.740***	-0.732***	0.685***	-1.704***	-1.426***	-1.060***	-1.032***	-0.019
	(0.170)	(0.161)	(0.187)	(0.176)	(0.111)	(0.121)	(0.113)	(0.119)	(0.115)	(0.097)
Inverse Mills Ratio		-0.641***		-0.549***			-0.592***		-0.378***	
		(0.099)		(0.087)			(0.061)		(0.063)	
Observations	288	288	288	288	288	718	718	718	718	718

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include controls for cohort marital status.

Table 7. Cohort-Based Estimates of the Elasticity of Taxable Income, Dependent Variable is AGI

	1979-1990					1979-2008				
	No Selection (Eq 4)	With Selection (Eq 5)	No Selection With Demographics	With Demographics and Selection	No selection or Year effects	No Selection (Eq 4)	With Selection (Eq 5)	No Selection With Demographics	With Demographics and Selection	No selection or Year effects
state+federal tax:										
Elasticity	-1.572***	-1.444***	-1.367***	-1.336***	0.584***	-2.057***	-1.808***	-1.619***	-1.589***	-0.131
	(0.182)	(0.172)	(0.206)	(0.193)	(0.126)	(0.125)	(0.119)	(0.130)	(0.125)	(0.100)
Inverse Mills Ratio		-0.642***		-0.609***			-0.521***		-0.396***	
		(0.121)		(0.115)			(0.067)		(0.070)	
state+federal+fica tax:										
Elasticity	-1.395***	-1.278***	-1.103***	-1.093***	0.707***	-1.994***	-1.720***	-1.429***	-1.400***	-0.068
	(0.204)	(0.196)	(0.227)	(0.215)	(0.125)	(0.146)	(0.137)	(0.145)	(0.140)	(0.108)
Inverse Mills Ratio		-0.695***		-0.631***			-0.583***		-0.405***	
		(0.126)		(0.117)			(0.070)		(0.073)	
Observations	288	288	288	288	288	718	718	718	718	718

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include controls for cohort marital status.

Table 8. Cohort-Based Estimates of the Elasticity of Taxable Income, Dependent Variable is Taxable Income

	1979-1990					1979-2008				
	No Selection (Eq 4)	With Selection (Eq 5)	No Selection With Demographics	With Demographics and Selection	No selection or Year effects	No Selection (Eq 4)	With Selection (Eq 5)	No Selection With Demographics	With Demographics and Selection	No selection or Year effects
state+federal tax:										
Elasticity	-1.359***	-1.258***	-1.239***	-1.214***	0.196	-2.190***	-1.921***	-1.643***	-1.611***	-0.365***
	(0.192)	(0.188)	(0.203)	(0.194)	(0.129)	(0.139)	(0.135)	(0.135)	(0.133)	(0.109)
Inverse Mills Ratio		-0.506***		-0.496***			-0.563***		-0.435***	
		(0.133)		(0.117)			(0.084)		(0.084)	
state+federal+fica tax:										
Elasticity	-1.103***	-1.008***	-0.889***	-0.881***	0.210	-2.101***	-1.805***	-1.394***	-1.361***	-0.355***
	(0.217)	(0.216)	(0.224)	(0.217)	(0.136)	(0.160)	(0.153)	(0.151)	(0.149)	(0.120)
Inverse Mills Ratio		-0.560***		-0.516***			-0.631***		-0.445***	
		(0.138)		(0.121)			(0.087)		(0.088)	
Observations	288	288	288	288	288	718	718	718	718	718

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Estimates of the Elasticity of Taxable Income from Dynamic Cohort Models

	Broad Income				**AGI**				**Taxable Income**			
	Fed+State		Fed+State+Fica		Fed+State		Fed+State+Fica		Fed+State		Fed+State+Fica	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Elasticity	-1.211***	-1.801***	-1.149***	-1.457***	-1.556***	-2.208***	-1.493***	-1.858***	-1.562***	-2.072***	-1.495***	-1.689***
	(0.086)	(0.131)	(0.096)	(0.136)	(0.118)	(0.167)	(0.132)	(0.171)	(0.120)	(0.192)	(0.133)	(0.195)
Additional Demographics												
Elasticity	-0.996***	-1.761***	-0.885***	-1.415***	-1.340***	-2.163***	-1.210***	-1.812***	-1.293***	-1.935***	-1.130***	-1.549***
	(0.092)	(0.131)	(0.102)	(0.136)	(0.126)	(0.169)	(0.138)	(0.173)	(0.126)	(0.189)	(0.140)	(0.190)
Observations	679	679	679	679	679	679	679	679	679	679	679	679

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. All models are weighted and include controls for cohort marital status and initial income. The sample period is 1979-2008. Specification (1) refers to equation (6) of the text and is a cohort version of a dynamic panel model, while specification (2) refers to equation (7) of the text, a first difference cohort model.

Table 10. A Cohort-Based Model of the Effects of Taxes on the Labor Supply of Men and Women

	Male			Female		
	(1)	(2)	(3)	(1)	(2)	(3)
After-Tax Wage	57.625**	54.871**	102.694***	129.110***	89.559***	78.482***
	(22.441)	(21.823)	(21.541)	(29.369)	(28.151)	(28.350)
Virtual Non-Labor Income	-7.928***	-7.157***	-5.752***	-1.477	0.606	0.498
	(0.841)	(0.817)	(0.804)	(0.946)	(0.906)	(0.896)
Wage Residual	-125.172***	-145.144***	-192.536***	-141.003***	-120.413***	-109.365***
	(22.509)	(21.890)	(21.618)	(29.419)	(28.177)	(28.363)
Virtual Income Residual	7.745***	6.496***	5.121***	-0.179	-2.970***	-2.882***
	(0.843)	(0.818)	(0.806)	(0.947)	(0.907)	(0.898)
lambda	-484.813***	-586.663***		-171.685***	-91.285***	
	(29.005)	(31.354)		(31.325)	(32.559)	
lambda2		409.779***			-83.594***	
		(31.584)			(29.717)	
lambda3			-325.693***			-161.853***
			(25.701)			(27.901)
Uncompensated Wage Elasticity	0.04	0.04	0.07	0.17	0.12	0.10
Compensated Wage Elasticity	0.16	0.15	0.16	0.19	0.11	0.10
Income Elasticity	-0.12	-0.11	-0.09	-0.02	0.01	0.01
Observations	295310	289010	289010	141136	132747	132747

Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. Each specification controls for marital status, the number of kids under age 6, the number of kids under age 18, race, and cohort, year, and state fixed effects.