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# THE EFFECT OF MARGINAL TAX RATES ON INCOME: A PANEL STUDY OF 'BRACKET CREEP'

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## ABSTRACT

This paper uses a panel of individual tax returns and the `bracket creep' as source of tax rate variation to construct instrumental variables estimates of the sensitivity of income to changes in tax rates. From 1979 to 1981, the US income tax schedule was fixed in nominal terms while inflation was high (around 10%). This produced a real change in tax rate schedules. Taxpayers near the top-end of a tax bracket were more likely to creep to a higher bracket and thus experience a rise in marginal rates the following year than the other taxpayers. Compensated elasticities can be estimated by comparing the differences in changes in income between taxpayers close to the top-end of a tax bracket to the other taxpayers. These estimates, based on comparisons between very similar groups, are robust to underlying changes in the income distribution, such as a rise in inequality. The elasticities found are higher than those derived in labor supply studies but smaller than those found previously with the same kind of tax returns data.

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# 1 Introduction

The response of taxpayers to changes in marginal rates has long been of interest to economists. The magnitude of this response is of critical importance in the formulation of tax policy and the determination of the size of the government and welfare programs. However the empirical literature has failed to generate any consensus on the magnitude of the elasticities of income with respect to marginal tax rates: estimates range from no effect to extremely large effects.

The labor supply literature focuses mostly on the elasticity of hours of work with respect to marginal tax rates and finds in general small responses to taxation. This literature suffers from two major drawbacks. First, hours of work might not be the only dimension of the total behavioral response to taxation, which is the relevant variable for tax policy purposes. Second, the identification of elasticities in the labor supply literature rests in general on strong functional form assumptions. Estimates are therefore sensitive to these functional form assumptions.

Recent studies have looked directly at the sensitivity of overall income with respect to marginal rates using tax reforms to identify the parameters of interest. These studies have used the US tax reforms of 1981 and 1986 to estimate taxpayers' responses. They find very large responses to taxation. This recent literature also suffers from major problems. First, the tax reforms introduced many changes in the definition of taxable income besides tax rate changes and thus it is often problematic to compare reported income before and after the tax reform. Second, these studies compare high income taxpayers (who experienced large tax rate cuts) to low and middle income taxpayers (who experienced almost no tax rate changes). Therefore, this methodology amounts to attributing the widening in inequalities to the tax reform. Third, this literature is not able, as opposed to most labor supply studies, to tell apart income and substitution effects. The knowledge of the size of each of these effects is important for tax policy.

These objections suggest that a research design to estimate behavioral responses to marginal tax rates should meet two conditions. First, the tax change should affect only marginal tax rates without introducing many changes in tax rules. Second, the tax change should affect differently groups of taxpayers that are comparable (i.e., whose incomes and other economic characteristics are close). The 'bracket creep' in the US income tax of the early eighties is a tax change meeting these two conditions.

From 1979 to 1981, inflation was high (around 10%) but the tax schedule was fixed in nominal terms. Because the income tax was highly progressive-there were about 15 tax brackets with rates increasing from 0 to 70%-inflation had a strong real impact.[1] The kink points of the tax schedule, fixed in nominal terms, shifted down in real terms. Therefore, a taxpayer near the top-end of a bracket was likely to *creep* to the next bracket even if his income did not change in real terms. The other taxpayers (far from the top-end of a bracket), however, were not as likely to experience an increase in marginal rates the following year. This characteristic of 'bracket creep' is exploited in this study to estimate the elasticities of income with respect to marginal rates. The spirit of the empirical strategy is to compare changes in income of taxpayers near the top-end of a bracket to changes in income of other taxpayers.

This identification strategy has three advantages relative to the tax reform experiments of the eighties. First, I compare groups of taxpayers whose incomes are very close. Therefore, the estimates are likely to be robust to changes in the underlying distribution of income and in particular to underlying increases in inequality. Second, the 'bracket creep' phenomenon did not modify the definitions of reported income and thus incomes can be easily compared across years. Third, as a theoretical matter, I will show that the estimates obtained using 'bracket creep' are not a mix of income and substitution effects but rather pure compensated elasticities of income with respect to marginal tax rates. Three other important characteristics of the 'bracket creep' tax change should be mentioned. First, because I compare year to year changes, my study will capture only short term responses to tax changes which might be different from medium or long term responses. Second, changes in tax rates due to 'Bracket Creep' were relatively small com-

[1] The effect of bracket creep on the US income tax was so strong that it increased substantially the average marginal rates and was the main cause of the 'tax revolt' of the late 1970s and early 1980s (see Steuerle (1991), Chapters 2 and 3, for a more detailed discussion). By comparison, the income tax cuts of 1981-84 were in fact just enough to bring total federal income tax receipts over GNP back to their 1977 level.

pared to the changes induced by the large tax reforms of the eighties and thus it is harder to obtain precise estimates. Last, because 'bracket creep' was not a legislated change, it might have been harder for taxpayers to understand the effect of this change on marginal tax rates. I come back to these important points in more detail in the concluding Section.

The paper proceeds as follows. Section 2 briefly reviews the empirical literature on behavioral responses to taxation. Section 3 presents in detail the effects of 'bracket creep' on the tax schedule. This study requires the precise location of taxpayers on the tax schedule and also requires following taxpayers over several years. Therefore I use a publicly available panel dataset of tax returns constructed by the Internal Revenue Service (IRS). The dataset, summary statistics and raw differences-in-differences results are presented. Section 4 introduces the regression framework and specification and Section 5 displays the regression results. Section 6 presents caveats, discusses policy implications and concludes.

# 2 Literature

The basic approach of the traditional labor supply literature was to posit a linear budget constraint and regress hours of work on (after-tax) wage rates and non-wage income. This literature has in general found very small elasticities (both compensated and uncompensated) of labor supply with respect to wages rates (or equivalently to marginal tax rates) for prime age males. Pencavel (1986) is an extensive survey of these studies. Estimates for the uncompensated elasticity are usually slightly negative (around -0.1). The compensated elasticity estimates are in general slightly higher but usually below 0.2.

Hausman (1981) applied a new methodology taking full account of the non-linearity of the budget set due to the progressive structure of the US income tax and challenged the prevailing wisdom that taxes had almost no incentive effect on labor supply. This non-linear budget set methodology has been used in many papers to estimate labor supply elasticities (these studies are surveyed in Hausman (1985)). These studies tend to find small uncompensated elasticities but high income effects leading to substantial compensated elasticities (often around 0.5). Non-linear budget set estimates have been shown to be sensitive to small changes in specification (see MaCurdy *et al.* (1990) and Triest

(1990)).

Both the traditional labor supply literature and non-linear budget set studies suggest consistently that the elasticity is larger for secondary earners (married women): the elasticities found are often between 0.5 and 1 (e.g. Hausman (1985), Mroz (1987), Heckman (1993)).

The labor supply literature has been criticized along various lines. First, the estimates are dependent on the functional form chosen for the statistical inference. In other words, the identification of the key parameters comes from strong structural form assumptions. Note however that, because of these strong structural assumptions, labor supply studies can in general estimate both income and substitution effects. Second, hours of work may not be the only dimension of "effort": individuals can vary their labor supply in the short run not only by changing hours but also by changing the intensity of work. In the long run taxpayers can also change the types of job they choose (see Feldstein (1995) for a more detailed discussion of this point). What matters for tax policy is the total response of reported income with respect to tax rates. Therefore, labor supply estimates may be substantially lower than the relevant total income elasticity.

Looking directly at the income response of taxpayers to tax reforms seems to be a more promising approach to solve these two problems. First, tax reforms provide an *exogenous* time variation in marginal tax rates so that weaker functional forms assumptions can be used to identify the parameters of interest. Second, it is possible to study directly the total income response without need to focus only on hours of works. Previous research connecting the changes in reported income to changes in marginal tax rates include Lindsey (1987), Navratil (1995), Feldstein (1995) and Auten and Carroll (1997). The first two studies used the tax cuts of the Economic Recovery Tax Act (ERTA) of 1981 and the last two used the Tax Reform Act (TRA) of 1986 to identify the elasticities. All four studies used Internal Revenue Service (IRS) datasets of tax returns. The last three studies used a panel of tax returns whereas Lindsey (1987) had to use a repeated cross section because the panel was not yet available at the time he made his study.

Lindsey (1987) ranked the individual taxpayers by adjusted gross income before the ERTA and after the ERTA. His key assumption was that the successive fractiles corresponded to the same individuals in both years. He then related the change in average income for successive fractiles to predicted changes in their marginal net-of-tax rates (i.e., one minus the marginal rate). Lindsey's analysis implied very large elasticities: between 1 and 3, his preferred estimate being equal to 1.6.

Navratil (1995) used instead a panel of tax returns and compared years before the ERTA and year 1983 after the ERTA. He derived his elasticity estimates by regressing the log change in income on the predicted log change in net-of-tax rates.[2] It is important to note that this methodology does not lead to real elasticity estimates because this is a reduced form regression. To get estimates of the elasticity of income with respect to marginal tax rates, Navratil should have regressed the log change in income on the real log change in rates using the predicted log change in rates as an instrument (I discuss this point again in Section 4). Navratil finds overall elasticities of about 0.8 for taxable income and about 1 for wages and salaries. These estimates are smaller than Lindsey's but still very high compared to the labor supply literature.

Feldstein (1995) uses a similar methodology with the TRA of 1986 and the same panel of tax returns.[3] Feldstein divided his sample into three income groups: medium, high and highest. These groups experienced different marginal rates cuts. The tax cuts were smaller for medium income earners than for high income earners. The cuts for highest income earners were even larger than for high income earners. Feldstein then computes the change in taxable income between year 1985 and 1988 for each group and derives a differences-in-differences elasticity estimate by comparing changes across the different groups. As high income earners experienced a larger increase in revenue than low income earners, Feldstein obtains high elasticity estimates (ranging from 1 to 3). Some of Feldstein's results are based on very small samples and therefore the estimates are probably not precise (see Slemrod (1996) for a discussion of this point). These estimates are again reduced form estimates. The analysis is also complicated by the fact that the TRA of

[2]The predicted log change in net-of-tax rates is equal to $\log(1 - t_1/1 - t_0)$ where $t_0$ is the marginal rate before tax reform and $t_1$ is the post-reform marginal rate at the *before* tax reform taxable income level, adjusted for inflation.

[3]In fact, Feldstein's study preceded Navratil's study and thus was the first one to use the panel data of the IRS to estimate elasticities of taxable income with respect to marginal rates.

1986 introduced many changes in tax rules and therefore the definitions of adjusted gross income and taxable income were substantially modified. The large decrease in upper marginal rates may also have induced many wealthy taxpayers to shift corporate income which was taxed as corporate profits to S corporations and partnerships which are taxed as personal income tax (see Feldstein (1995) and Slemrod (1996) for a discussion of this point).

As pointed out by Navratil (1995) (Chapter 2), Feldstein's results depend critically on the assumption that the elasticities are the same for the three groups. Navratil computes elasticity estimates based on a simple pre-post reform comparison for each of the three groups. The three estimates are very different (though not statistically different because of the small size of the sample) but the three of them are substantially smaller than Feldstein's difference-in-differences estimate.

Auten and Carroll (1997) repeated the study of Feldstein but with a much larger panel dataset of tax returns available only to researchers at the US Treasury. They compute structural estimates using an instrumental variable method. They are also able to control for some non-tax factors such as age, state of residence and type of job. They obtain smaller estimates than Feldstein: their preferred estimate is equal to 0.66. It is however difficult to compare directly their results with Feldstein's because they present neither their first stage estimates nor the reduced form estimates that Feldstein reported.

The most important problem with the studies reviewed above is that the marginal rate cuts of the two Tax Reforms (ERTA and TRA) increased with income: wealthy taxpayers experienced larger marginal rates cuts than poorer taxpayers. Therefore, imputing the faster increase of high incomes compared to low incomes only to the tax reforms leads to upward biased estimates if increases in inequality are partly due to other factors than tax cuts. Economists have proposed many other explanations for increased income inequality: Murphy and Welch (1992) and Katz and Murphy (1992) found that the returns to human capital or education increased as a result of increased demand for skilled labor. Declining union membership (Freeman (1993)), increasing import competition (Bound and Johnson (1992)), increasing immigration (Topel (1994)) have also been proposed as potential explanations of the widening inequalities over the last 25 years in the US.

We have seen that the recent tax response studies have a decisive advantage compared to old structural labor supply studies because the identification problem is not solved artificially through strong functional form assumptions. However, this advantage has a cost: the tax reform studies are no longer able to tell apart substitution and income effects. These studies present a single elasticity estimate which is neither a pure compensated elasticity nor a pure uncompensated elasticity but a mix of both elasticities. In general, the studies using legislated tax reforms do not discuss this issue at all. It is important, though, to be able to tell apart each elasticity because optimal tax rates levels depend on the size of both elasticities (see Saez (1998)).

My paper will try to address these issues. After describing in details the tax changes due to 'bracket creep' , I argue in Section 3.1 why my estimates are free from the problems affecting the existing literature about the behavioral responses to taxation.

# 3 'Bracket Creep', Data and Descriptive Statistics

## 3.1 The 'Bracket Creep' phenomenon

The analysis presented here uses the same panel of tax returns as Feldstein's and Navratil's studies but does not use a tax reform to carry out the estimation. The paper focuses instead on a very different kind of tax change. From 1979 to 1981, the tax schedule was not indexed even though inflation was on the order of 10% per year. Non-indexation changed the tax schedule because the income tax was highly progressive; this phenomenon was called 'bracket creep'.

Figures 1 and 2 show the effect of inflation on the tax schedule and on marginal rates. After tax real income as a function of before tax real income is represented on Figure 1 for two consecutive years: the straight line represents the year 1 schedule and the dashed line the year 2 schedule. The kink points (i.e., the points where the marginal rate jumps) shift to the left because of inflation, but the slopes of the segments linking the kink points do not change. The marginal rates schedules are represented on Figure 2. If taxable income remains the same in *real* terms in year 2, then some taxpayers will face a higher rate: this is the "treatment" group. The other taxpayers will still face the same rate: this is the "control" group. These different groups are displayed on the figures. Formally, if the tax schedule is given by $T$ in year 1, then in real terms, the tax schedule in year 2 is $\hat{T}$ defined by:

$$\hat{T}(x) = T[x(1+\pi)]/(1+\pi)$$

where $\pi$ is the inflation rate, and $x$ is real income. Therefore,

$$\hat{T}'(x) = T'[x(1+\pi)]$$

The tax changes induced by 'Bracket creep' have several advantages compared to the studies of Lindsey, Navratil and Feldstein reviewed above. First, there were almost no changes in the income tax code during the three years I focus on, therefore the only change is due to inflation. Comparisons across years are thus straightforward compared to the tax reforms studies.

Second, and more importantly, kinks are regularly spaced along the whole income distribution. Therefore, control and treatment groups can be constructed over a large portion of the income distribution. Also noteworthy is the fact that controls and treatments *alternate* and thus for a given kink the treatment group and the two surrounding control groups are very similar in terms of income and very likely to share the same economic characteristics. Therefore the difference in changes in income between these groups can be confidently attributed to marginal rates effects. The estimates are thus likely to be robust to changes in the distribution of income and especially to changes in inequality.

Last, I will show in Section 4 that the elasticity estimates obtained using 'bracket creep' are in fact compensated elasticities of income with respect to marginal tax rates. Therefore, the usual deadweight burden approximations (which involve only the compensated elasticity) measuring the welfare costs of taxation could be easily computed. More generally, it is important for optimal income tax purposes to know the size of both compensated elasticity and income effects.[4] The analysis of 'bracket creep' provides estimates

[4]Saez (1998) shows that optimal income tax formulas can be expressed in terms of these two parameters and the shape of the income distribution.

of the first of these two key parameters.

However, the changes in marginal rates are not very large because there were many kink points at that time and the jumps in marginal rates were in general of 4-7% (see below). This is small compared to a decrease from 50% to 28% in marginal rates for the very high income earners following the TRA of 1986. However, Steuerle (1991) provides evidence that the 'bracket creep' of late 1970s and early 1980s was perceived as a major tax event. 'Bracket creep' triggered the strongest increase in marginal tax rates since World War II in just a few years. Federal income tax receipts over GNP increased very quickly from 1978 to 1981. According to Steuerle, this was the main cause of the 'tax revolt' and the tax cuts which took place in the 1980s. As 1980 was not the first experience of 'bracket creep' in the US (inflation was also high in the 1973-1975 period), it is very likely that 'bracket creep' was noticed and understood by most taxpayers.

## 3.2 Data

The IRS panel of tax returns which I use in this study covers the period 1979 to 1990. However, only the first three years are used for this project. This panel, known as the Continuous Work History File, contains most items on Form 1040, as well as numerous other items from the other forms and schedules. The IRS panel is constructed from all tax returns filed in a given year by selecting certain 4-digit endings of the social security number of the primary taxpayer listed on the form. Five such 4-digit endings were selected in 1979-1981, the three years used in this study. For each of these years, the panel contains about 46,000 observations. Due to budgetary limitations, only one 4-digit ending was chosen in 1982 and 1984 and two 4-digit endings were chosen in the other years. Thus Feldstein's and Navratil's studies were based on relatively small samples. After several deletions, Navratil used about 2,000 observations and Feldstein about 3,500.

Attrition in the panel can occur due to late filing or no filing (which can happen for example if the taxpayer does not owe any taxes and does not expect a refund from the IRS). Attrition may also result from a change in marital status if the name of the primary taxpayer listed on the return changes (see Christian and Frischmann (1989) for a more complete discussion of attrition in this panel).

In the US, there are different tax rate schedules for taxpayers filing as Singles, Married[5] or Heads of household). As singles and married constitute about 90% of all tax returns, I will consider only single and married taxpayers. I compare year 1980 to year 1979 and year 1981 to year 1980. These two differences are stacked to obtain a dataset of about 80,000 observations. I then exclude taxpayers whose marital status changes from year 1 to year 2. It is unlikely that 'bracket creep' affected specifically marriage strategies and therefore discarding those observations should not bias the results. I also exclude taxpayers who do not use the regular tax schedule in year 1.[6]

Real growth of GDP was small in 1980 and 1981: -0.5% in 1980 and 1.8% in 1981. The GDP deflator was 10.5% in 1980 and 9.5% in 1981. These figures are very close to the nominal growth of adjusted gross income for each year. The results I present are not sensitive to small changes in these parameters, which I call the "inflation parameters". Most items reported on tax returns can be considered to grow roughly at the inflation rate. This is the case for adjusted gross income (AGI), wages and salaries, itemized deductions. Therefore I can express these items for year 2 in year 1 dollars just by dividing them by the inflation rate.

Taxable income is the key item to divide the sample into control and treatment groups. Taxable income is computed in two different ways depending on whether the taxpayer itemizes deductions or chooses the standard deduction. A taxpayer itemizes when the total of his itemized deductions is larger than the standard deduction. The standard deduction is fixed in nominal terms: 3,400 dollars for married taxpayers and 2,300 dollars for singles. If the taxpayer does not itemize, taxable income is simply equal to AGI minus

[5]Married taxpayers can choose to fill either jointly or separately. The overwhelming majority of married taxpayers (more than 98%) chooses to fill jointly. Therefore, married taxpayers filing separately will be not be considered in my study.

[6]Most of these excluded taxpayers used the average income tax schedule which allowed taxpayers to replace their taxable income by an average of the last few years taxable income. This reduced the tax liability of taxpayers who had experienced a sharp rise in income. I also exclude taxpayers using the Maximum Tax Rate on Personal Service Income. The aim of the Maximum Tax Rate was to constrain the top rate on earned income to 50% (instead of 70%).

personal exemptions.[7] If the taxpayer itemizes, taxable income is equal to AGI minus personal exemptions minus itemized deductions plus the standard deduction.[8]

I write $taxinc_i$ for *nominal* taxable income in year i. $taxinc_i$ is simply taxable income reported on the tax form in year i. To assign a taxpayer to a treatment or control group, I compute predicted taxable income ($taxinc_p$) which is $taxinc_1$ expressed in year 2 dollars. If the marginal rate corresponding to $taxinc_p$ is above the one corresponding to $taxinc_1$,[9] the observation is assigned to the treatment group of the corresponding kink. If the marginal rates for $taxinc_1$ and $taxinc_p$ are the same, the taxpayer is assigned to the control group. In order to compute the real change in taxable income, I also express $taxinc_2$ in terms of year 1 dollars (this is denoted by $taxinc_{2R}$). The details of the computations of $taxinc_p$ and $taxinc_{2R}$ are given in appendix. From now on, I denote by $T'_i = T'(taxinc_i)$ the effective marginal rate in year i and $T'_p = T'(taxinc_p)$ the predicted marginal rate in year 2 if *real* income does not change.

## 3.3 Descriptive Statistics

Figures 3 and 4 display the actual marginal rate schedules of year 1979 and the real effect of 'bracket creep' on tax rates for married and single taxpayers respectively. These figures are the empirical counterpart of Figure 2; the nominal location of kink points are reported (in thousands of 1979 dollars) on the horizontal axis, the marginal tax rates are reported on the vertical axis. The solid line represents the nominal schedule for year 1 while the dashed line represents the *real* schedule in year 2 (assuming a 10% inflation rate). Tables I and II show the summary statistics for each control and treatment group, for married and single filers. The groups are ordered by increasing taxable income in year 1. For each kink, the nominal level of taxable income at which the kink takes place and the jumps in marginal rates are presented in columns (2) and (3). Therefore, these Tables describe fully the tax schedule of years 1979 to 1981 for married taxpayers and single taxpayers.

[7] Exemptions were fixed in nominal terms: 1,000 dollars for each person in the household.

[8] The definition of taxable income changed after the Tax Reform Act of 1986. The standard deduction is no longer included in taxable income and the zero tax bracket has disappeared.

[9] That is, by reporting the same real taxable income, the taxpayer would *creep* to the next bracket.

There were 15 kinks for married taxpayers and 16 kinks for singles.[10] I have constructed two control groups with incomes below the first treatment group (Control N and Control 0) in order to emphasize the mean reversion phenomenon for very low incomes. I have discarded the observations below Control N because taxpayers who report very low (or even negative) taxable income are often middle-high income earners which have faced a transitory sharp decline in taxable income. Slemrod (1992) discusses this point in detail. I indicate the number of observations for each group in column (4). The number of observations decreases quickly for the highest kink points because the panel does not overweight wealthy taxpayers.

Next, in column (5), the log ratios of predicted net-of-tax rates ($\log[(1-T_p')/(1-T_1')]$) are reported. The values are equal to zero for the controls because by definition, the marginal rate they face remains the same in year 2 if their *real* taxable income does not change. For treatments the values are negative: e.g. for the treatment corresponding to the kink 37/43 the value reported is $\log[(1-0.43)/(1-0.37)]$. This is the log change in net-of-taxes rates that a taxpayer in the corresponding treatment group would face if his *real* taxable income did not change from year 1 to year 2. Column (5) (or equivalently Figures 3 and 4) summarizes the effects of 'bracket creep' on tax rates. Except for the first jump in marginal rate (from 0 to 14%) the jumps in marginal rates are small at low income levels but become progressively larger as income increases. As displayed on Figures 3 and 4, treatment and control bands are roughly of the same size.

In column (6), I report the mean log difference of effective net-of-tax rates, $\log(1 - T_2'/1 - T_1')$ for each group. Because individual real incomes change from year to year, figures in column (5) and (6) differ. The corresponding values are plotted on figure 5 for married taxpayers and Figures 6 and 7 for singles. The curve corresponding to column (5) is plotted in straight line while the curve corresponding to column (6) is plotted in dashed line. The curve of real changes in marginal rates goes up and down exactly in the same way as the curve of predicted changes in marginal rates.[11] Therefore, predicted change in

[10] I have not reported statistics for the last jump in marginal rates from 68% to 70% because the size of this jump is small and there are very few observations around that last kink point. The last control group is composed of taxpayers below the treatment group for the kink 68/70.

[11] The only exception is for the kink 18/19 for singles (see Figures 6 or 7) which is by far the smallest

marginal rates is highly correlated with the real change in rates and therefore predicted change is a good instrument for real change. However, because the spikes of real changes are flatter than the spikes of predicted changes, reduced form estimates similar to the ones previous studies report (see Section 2) would be significantly lower than structural estimates. I come back this point again later on.

In columns (7), (8) and (9), I report the means of log changes of real taxable income ($\log(taxinc_{2R}/taxinc_1)$), real adjusted gross income ($\log(AGI_{2R}/AGI_1)$) and real wages ($\log(wages_{2R}/wages_1)$. There is mean reversion at both ends of the income distribution. The change in incomes are high and positive for low incomes-this change is quickly decreasing as income increases-whereas the change in incomes becomes in general highly negative for high income earners. This complicates the estimation of the elasticities at very low and very high incomes.

If marginal rates matter for taxpayers, we should find that treatment groups experience larger decreases in incomes than the surrounding control groups. To check whether this pattern is apparent in the data, I have also plotted the log changes of taxable income and AGI on Figure 5 for married taxpayers and Figures 6 and 7 for singles.[12]

Figure 5 gives striking evidence of responsiveness of married taxpayers to tax rates. From the Treatment5 group (kink 21/24) to Control10 group (kink 43/49), the log change in taxable income presents exactly the same shape as the predicted changes in marginal rates: the value for the treatment group is always smaller than for the two surrounding control values. The same is true for log changes in adjusted gross income though the differences between treatments and controls are somewhat smaller. This is not the case for lower incomes because jumps in marginal rates were very small (less than 3%) except at the first kink (large jump of 14%). However, around this first kink, the mean reversion phenomenon is very important (this is not represented on Figure 5 but can be easily seen on Table I). Higher kink points do not reveal the same evidence but this may well be due

jump in the tax schedule. The predicted change does not follow as closely the real change for higher kink points (these kinks are not represented on the figures) because of the noise due to the small number of observations for high income earners.

[12]The log change in wages is also plotted on Figure 7.

to the small number of observations in that range[13] and to mean reversion. The pattern of wage earnings[14] is not similar to the pattern of taxable income or adjusted gross income: even at the middle income kinks, there is no clear evidence that wages of treatments tend to be systematically smaller than wages of surrounding controls. This already suggests that the response of taxpayers is probably not the consequence of reduced labor supply.

The pattern for singles on Figure 6 is less clear, even for middle income earners. Until Treatment8 group, the kinks were small (except the first one, the jumps were of less than 3%) and thus no systematic response is observed. From Treatment8 to Control12, there is some evidence of taxpayer behavior for adjusted gross income and taxable income. Above Control12, the number of observations becomes very small and no clear pattern would be observed. As for married taxpayers, wages for singles reveal no clear evidence of behavioral responses.

However, the first kink point for singles deserves particular attention. Figure 7 focuses more particularly on low income singles. There is a clear break in the pattern of AGI and wages around the first kink point consistent with a behavioral response to marginal rates: although the general pattern of the curves is declining (due to mean reversion), wages and AGI go up from Treatment1 to Control1.[15] There is no such pattern for taxable income because mean reversion in taxable income at the bottom is even larger than for AGI or wages.[16] Therefore, Figure 7 suggests that low income singles reacted to marginal rates by reducing labor supply.

These figures suggest that taxpayers are responsive to changes in tax rates and that married taxpayers are more responsive to tax rates than singles. However, except for low income singles, wages do no seem to be responsive to changes in tax rates. I will now try

[13]I have not plotted the curves for the highest kink points but this can be figured out looking at Table I.

[14]The curve for wages is not plotted to avoid packing too many plots on the figure.

[15]Wages and AGI curves are very close for low income singles because most of them report only wage income.

[16]This is explained by the deduction of exemptions and net itemized deductions from AGI to compute taxable income: this overstates mechanically positive log changes in taxable income compared to AGI for very low income earners.

to put numbers on these first qualitative results.

### 3.4 Wald Estimates

From the Tables described above, it is easy to compute Wald estimates of the elasticity for each kink. Wald estimates relate the difference in changes in income between treatments and controls to the difference in changes in real marginal rates between treatments and controls. This gives simple estimates of the elasticity of income with respect to marginal rates. Treatments are observations in a given treatment group and controls are observations belonging to the two surrounding control groups. The Wald estimate can be written as:

$$\hat{\zeta} = \frac{\hat{E}[\log(z_2/z_1)|Tr] - \hat{E}[\log(z_2/z_1)|C]}{\hat{E}[\log(1-T_2'/1-T_1')|Tr] - \hat{E}[\log(1-T_2'/1-T_1')|C]}$$

where $\hat{E}$ means empirical mean, $Tr$ is for treatment and $C$ for control. $z_1$ is income in year 1 and $z_2$ is income in year 2 in terms of year 1 dollars. This estimate is equivalent to an IV regression of $\log(z_2/z_1)$ on $\log[(1-T_2')/(1-T_1')]$ (and a constant) using a binary instrument (1 if in treatment and 0 if in control). This method leads to consistent estimates if the difference in changes in income between treatments and controls is entirely due to the fact that treatments are more likely to experience an increase in rates than controls. This assumption is likely to be satisfied because incomes of treatments and surrounding controls are very close and therefore treatments and controls are similar except for their treatment/control status.[17] The IRS panel does not contain covariates (such as age or educational attainment) which could have been used to test formally whether Treatments and surrounding Controls are similar. Though not attempted in this paper, it would be possible to use another dataset with many covariates (such as the CPS or the PSID) and define the income groups corresponding (roughly) to Treatment and Control groups so as to test formally whether they are the same.

Reduced form estimates can also be derived by simply running an OLS regression

[17] This assumption is much more likely to be satisfied for middle income earners where mean reversion is not an issue. That is why I give Wald estimates only for middle income kinks.

of $\log(z_2/z_1)$ on $\log(1 - T'_p/1 - T'_1)$ (and a constant). This corresponds exactly to the methodology used by Navratil (1995).

I have reported Wald and Reduced form estimates for middle income kinks for taxable income, Adjusted Gross Income (AGI) and wages for married taxpayers and singles on Table III. Each Wald estimate was computed using observations of the corresponding treatment and both surrounding controls.

Columns (1) and (2) display the location of kink points (in current dollars) and the corresponding jumps in marginal rates. Column (3) presents the difference in the change in taxable income between the treatment group and the two surrounding control groups. This difference can be derived directly from Tables I and II using column (6) (which gives the average change in taxable income for each treatment and control group) and column (7) (which gives the sample weights for each treatment and control group).[18] Column (4) gives the values of the instrument $\log[(1 - T'_p)/(1 - T'_1)]$ for each Treatment group.

Column (5) presents the difference of the log change in marginal rates ($\log[1 - T'_2/1 - T'_1]$) between the treatment group and the two surrounding control groups.[19] Column (6) presents the reduced form estimates: this is just column (3) divided by column (4). Column (7) presents the Wald estimates (this is column (3) divided by column (5)). Wald estimates for adjusted gross income and wages have been computed in the same way (the different steps are not reported) and are presented in columns (8) and (9). All standard errors have been computed by running the corresponding OLS (for the reduced form estimates) and IV (for the Wald estimates) regressions.

I have tried alternative estimates. Removing taxpayers at the frontier between control and treatment bands did not change much the estimates. Keeping only the controls close to the treatments (i.e. discarding the controls which are the further away from the treatments) did not modified much my results either.

Looking at Table III, we can note that the elasticities are in general positive; this means that treatments tend to experience larger decreases in income than controls and thus that

[18] For example, the first number 0.0006 in Column (3) of Table III is obtained as $[(-0.0691 \cdot 2241) - (-0.0665 \cdot 2991 - 0.0727 \cdot 3264)]/[2241 + 2991 + 3264]$.

[19] This difference can be derived from Tables I and II exactly in the same way as Column (3) of Table III.

taxpayers are responsive to marginal rates. The estimates confirm the patterns of Figures 5 and 6: the estimates are significant and large for married middle income earners for taxable income and AGI. The estimates are in general larger for taxable income than for AGI. The estimates for wages are usually much more smaller, often very near 0. As pointed out before, the estimates for singles are lower and not significant. The reduced form estimates are equal to about one-half of the structural estimates.

Therefore, simple Wald estimates confirm our first qualitative results. The response is higher for married taxpayers than for singles. The response of taxable income is higher than AGI and especially than wages. The response of wages is almost never significantly different from 0. However, the results are not estimated with great precision and there is large variability across kink points. The aim of next sections is to compute estimates based on larger portions of the income distribution in order to obtain more precise results.

# 4 Model and Identification Strategy

This section uses a regression framework to aggregate estimates over several kink points. A simple model will illustrate the issues at hand and show that the estimated elasticities are in fact pure compensated elasticities. The budget constraint of a taxpayer on a linear part of the tax schedule is given by $c = z(1-\tau) + R$, where $z$ represents before tax income, $\tau$ is the marginal rate and $R$ is virtual income. The virtual income $R$ is the post-tax income that the taxpayer would get if he reported no income and was allowed to stay on the same budget set line (with constant marginal rate $\tau$). From individual utility maximization, we can derive a income supply function which depends on the slope of the budget line and on virtual income.

$$z = z(1-\tau, R)$$

From this income supply function, the uncompensated elasticity of income (denoted by $\zeta^u$) and income effects (denoted by $\eta$) can be defined as follows:

$$\zeta^u = \frac{1-\tau}{z}\frac{\partial z}{\partial(1-\tau)}$$

and,

$$\eta = \frac{\partial z}{\partial R}$$

Let $z^c = z^c(1-\tau, u)$ be the compensated income supply.[20] The compensated elasticity of income ($\zeta^c$) is defined by:

$$\zeta^c = \frac{1-\tau}{z^c} \frac{\partial z^c}{\partial(1-\tau)}$$

The two elasticities and income effects are related by the Slutsky equation:

$$\zeta^c = \zeta^u - (1-\tau)\eta \tag{1}$$

'Bracket creep' can be seen as a change in both virtual income $R$ and marginal rate $\tau$. Small changes in $R$ and $\tau$ affect income supply $z$ as follows,

$$dz = -\frac{\partial z}{\partial(1-\tau)} d\tau + \frac{\partial z}{\partial R} dR$$

Using the definition of elasticities, we get:

$$dz = -\zeta^u \, z \frac{d\tau}{1-\tau} + \eta \, dR$$

Using the Slutsky equation (1) and rearranging,

$$\frac{dz}{z} = -\zeta^c \frac{d\tau}{1-\tau} + \eta \frac{dR - z d\tau}{z}$$

To introduce randomness in the model, I suppose that the income supply function $z$ also shifts randomly (i.e. $dz/z = \epsilon$) from year to year for reasons unrelated with the tax change. The random variable $\epsilon$ can be considered as taste shocks (resulting for example from a change in the composition of the household) or random changes in work opportunities (such as unexpected unemployment or job change). Therefore, the equation giving the total change in income from year 1 to year 2 ($dz/z$) can finally be written as:

[20] $z^c(1-\tau, u)$ is the income supply which minimizes costs to attain utility level $u$ for a given tax rate $\tau$.

$$\frac{dz}{z} = -\zeta^c \frac{d\tau}{1-\tau} + \eta \frac{dR - zd\tau}{z} + \epsilon \tag{2}$$

Let us first neglect the income effect term (i.e., assume that $\eta = 0$). In that case, by the Slutsky equation (1), compensated and uncompensated elasticities are the same (I note $\zeta = \zeta^c = \zeta^u$). Assuming that changes from year to year are small, we have, $dz/z \simeq \log(z_2/z_1)$ and $-d\tau/(1-\tau) \simeq \log[(1-T_2')/(1-T_1')]$ (with the same notation as in the previous section). The corresponding regression framework would then be the following:

$$\log(z_2/z_1) = \zeta \log[(1-T_2')/(1-T_1')] + \epsilon$$

Now clearly, $\log[(1-T_2')/(1-T_1')]$ is correlated with the error term because if $\epsilon$ is large, income goes up and thus, because marginal tax rates are increasing with income, $1-T_2'$ decreases. Therefore an OLS regression leads to estimates badly biased downward.[21] However, it is possible, using the variation in tax rates due to 'bracket creep', to construct instrumental variables. Consider the following dummy variable,

$$instr_{is} = 1(taxinc_1 \in Treatment\ for\ Kink\ i, mars = s)$$

These are binary instruments equal to 1 exactly for taxpayers whose taxable income in year 1 (denoted by $taxinc_1$) is in the treatment for Kink $i$ and whose marital status is $s$. The marital status $mars$ can take two values: 0 for singles and 1 for married taxpayers. The instruments $instr_{is}$ depend only on the level of income in year 1. Therefore, in this simple model, the instruments depend only on $z_1$ and are uncorrelated with $\epsilon$ if $\epsilon$ is independent of $z_1$. In this case we would just have to run the following regression:

$$\log(z_2/z_1) = \zeta \log[(1-T_2')/(1-T_1')] + \epsilon \tag{3}$$

using $instr_{is}$ as instruments for the real variation in marginal rates. The elasticity parameter $\zeta$ would be estimated consistently. Note that this set-up leads exactly to the

[21] In fact, running OLS regressions always leads to elasticity estimates well below zero (in general below -3).

simple Wald estimates presented above where we restricted ourselves to small portions of the distribution of income so that only one instrument was used for each regression.

However, if we consider large portions of the income distribution, it is more realistic to assume that the size of the random change in incomes (i.e $\epsilon$) varies as we move along the distribution of income. We have seen in the previous section that there is mean reversion and therefore that if $z_1$ is low in year 1 then $z_2$ is very likely to be above $z_1$. In this case, the distribution of the random shock in income $\epsilon$ is likely to be skewed toward the right. This works in the other direction for high income earners in year 1. On the other hand, if there is an underlying increase in inequalities (i.e., the rich get richer and the poor get poorer), a component of $\epsilon$ will be positively related to income in year 1 because high income earners will tend to do even better whereas low income earners will tend to do worse.

So if $\epsilon$ depends on $z_1 = taxinc_1$, the instrument (which is also a function of $z_1$) is likely to be correlated with the error term $\epsilon$. However by controlling for any smooth function of $taxinc_1$ in the regression set-up in both stages, it is possible to get rid of the correlation between $\epsilon$ and the instruments. The parameter of interest remains identified as long as the dependence of $\epsilon$ with respect to $taxinc_1$ does not reproduce the shape of the instruments. This dependence is due to mean reversion, macro-economic shocks and underlying trends in the income distribution and therefore is probably very smooth compared to the dummy shape of the instruments. Therefore, the system is very likely to be well identified.[22] Note that previous tax reform studies (which were reviewed in Section 2) cannot control for income because the marginal cuts were increasing in income (thus their instrument is monotone in income) and therefore controlling for income would destroy the identification.

Let us now analyze the case with income effects in equation (2). $dR - z\ d\tau$ is the

[22] This strategy is conceptually close the Regression Discontinuity Design (RDD), used in Angrist and Lavy (1999) and Van der Klauw (1996). The idea in both papers is to use the fact that the treatment (class size for Angrist and Lavy, financial help decision for Van der Klauw) is assigned on the basis of a discontinuous function of a continuous variable. The strategy is to use the rule as a source of identification, controlling in the regression for smooth functions of the variables on which the selection is based.

change in after-tax income due to the tax change for a given before tax income $z$: this is the vertical distance between the tax schedule for year 1 and the tax schedule for year 2 on Figure 1. This quantity is *continuously* increasing in income[23] and thus affects treatments and controls in roughly the same way. Therefore, this additional income effect term can be incorporated in the error term. The dependence of this term on income will be controlled for by the functions in $taxinc_1$ included as controls in the regression. Therefore, even with income effects, the parameter $\zeta$ that I estimate is in fact the compensated elasticity $\zeta^c$. Intuitively, at a given kink point, the increase in tax liability due to 'bracket creep' is roughly the same for treatments and controls but the change is tax rates is different for the two groups. Therefore, the difference in behavioral responses between the two groups is due to pure substitution effects. Thus, the 'bracket creep' experience allows the estimation of a conceptually well defined parameter. This point is important because the tax reform studies reviewed in Section 2 were only able to identify elasticity estimates which were a mix of substitution and income effects.

Let me now describe precisely the regression framework and the covariates I will use. To allow more generality, I run regressions in levels: $\log(z_2)$ is the dependent variable instead of $\log(z_2/z_1)$ and I include $\log(z_1)$ in the list of controls on the right hand side. When I run a regression for both married taxpayers and singles, I add a dummy $mars$ for marital status ($mars$ is equal to one if married and zero if single). I also add a dummy *item* for being an itemizer in year 1. Being an itemizer in year 1 is predetermined and therefore *item* can be considered as an independent variable.[24] Therefore the specification is as follows:

$$\log(z_2) = \alpha_0 + \alpha_1 \log(z_1) + \alpha_2 mars + \alpha_3 item + \zeta^c \log[(1-T_2')/(1-T_1')] + \beta f(taxinc_1) + \epsilon \quad (4)$$

[23]This quantity is not increasing smoothly because, as displayed on Figure 1, it is constant over Control regions and linearly increasing over Treatment regions. However, the important point here is that the quantity does not jump discontinuously.

[24]I add the dummy *item* because I show below that the elasticities of taxpayers itemizing in year 1 are significantly higher than the elasticities of non-itemizers.

The first stage being:

$$\log[(1-T_2')/(1-T_1')] = \sum_{i,s} \gamma_{is} instr_{is} + \pi_0 + \pi_1 \log(z_1) + \pi_2 mars + \pi_3 item + \delta f(taxinc_1) + \nu \tag{5}$$

where:

• $z_i$ is real income in year i (this can be taxable income, wages or AGI),

• $T_i'$ is the marginal rate in year i (i.e. T'(nominal taxable income)),

• $\zeta^c$ is the parameter of interest: compensated elasticity of income with respect to marginal rates.

The controls $f(taxinc_1)$ are smooth functions of $taxinc_1$ (polynomials in $taxinc_1$). Polynomials are added until the elasticity estimate is stabilized (3 or 4 polynomials are enough is most cases).

An alternative would be define a single instrument: $\log(1 - T_p'/1 - T_1')$ for all the regressions (Auten and Carroll (1997) used this type of instrument in their study). This single instrument would impose a relation between the size of the jump in marginal rates and the value of the instrument. Results with a single instrument are very similar to the results I present. Increasing the number of instruments, however, increases the power of the first stage and therefore reduces a little bit the standard errors, that is why I choose the multi-instrument set-up. I do not use exactly one instrument for each kink because some low kinks are very small and I have few observations for the highest kinks. Therefore I have grouped some kinks together to avoid using too weak instruments. The precise grouping is described in appendix.

# 5 Regression results

The first stage always leads to very significant coefficients for all the binary instruments. The F-statistic for the joint test of all the coefficients of the instruments being null is always higher than 50. This confirms that the instruments are good in the sense that they are significantly correlated with the endogenous regressor.

I estimated equation (5) for three types of incomes: wages, adjusted gross income

(AGI) and taxable income and different portions of the income distribution.[25] I divided my sample according to marital status - Single taxpayers and Married taxpayers filing jointly - and into year 1 itemizers and year 1 non-itemizers and estimated elasticities for those different groups. I did not split the sample of singles into itemizers versus non intemizers because very few singles choose to itemize and thus estimates would have been fairly imprecise for that sub-group. The elasticity results are presented in Tables IV and V.[26] Table IV presents estimates for a wide range of incomes (columns (1) to (3)) and for middle income earners (columns (4) to (6)). Table V focuses on high income earners (columns (1) to (3)) and on low income earners around the first kink point (columns (4) and (5)).

Column (1) of Table IV suggests that elasticities of taxable income are smaller than those found in previous studies using tax reforms: around 0.3 for married taxpayers and singles together, around 0.4 for married taxpayers and around 0.2 for singles. The elasticities of adjusted gross income are slightly lower: around 0.2 (see column (2)). The elasticities of wages are even smaller (around 0.1). The elasticities are in general higher for married taxpayers than for singles. Note however that the elasticities are not estimated with very high precision and therefore most of the estimates are not significantly different from 0. The estimated elasticities suggest that the labor supply response to marginal rates is small. This is consistent with the estimates of traditional labor supply literature.

The most striking fact in Table IV is that the elasticity for non-itemizers is always much smaller (and often slightly negative) than the elasticity of itemizers. Elasticity for married itemizers are high and significant: 0.65 for taxable income and 0.4 for adjusted gross income. The difference between the elasticity estimates of itemizers and those of non-itemizers persists for adjusted gross income and wages, though it is in general smaller than the difference for taxable income. This means that itemizers react more than non-itemizers not only through an increase in their itemized deductions but also through a larger reduction in reported income. This suggests that the population of itemizers is

[25]All income levels are expressed in 1979 dollars; a dollar of 1979 corresponds to 2.3 dollars of 1997.

[26]In both Tables, the list of polynomial controls in $taxinc_1$ is reported in the note. $\log(z_1)$ is always included as a covariate in the regressions.

different from the population of non-itemizers. The possibility of itemizing plays the role of a screening device where elastic taxpayers choose to itemize and non-elastic taxpayers choose the standard deduction.

Columns (4), (5) and (6) of Table IV report the same kind of estimates but restricted to middle income earners. The general pattern is the same as in columns (1) to (3). However, the elasticities for this group are, in general, significantly higher than for the wider range of income: 0.4 for taxable income, 0.3 for adjusted gross income for married taxpayers and singles together, 0.7 for taxable income of married itemizers. Note that this high value is close to the results of Navratil (1995) and Auten and Carroll (1997). The wage elasticity of married taxpayers, which is around 0.3, is also somewhat higher than before.

Table V focuses more specifically on high income earners (columns (1), (2) and (3)) and on low income earners around the first kink point (columns (4) and (5)). The elasticities of high income earners are smaller than those of middle income earners: around 0.3 for taxable income, around 0 for adjusted gross income and negative (though never significant) for wages. The elasticities, however, are not estimated with very high precision. This seems to indicate that high income earners did not react as much as middle income earners to 'bracket creep'. The discrepancy between the results for adjusted gross income and taxable income probably means that most of the response of high income earners was through increased itemized deductions and not through a reduction in real earnings.

Columns (4) and (5) in Table V report estimates around the first kink point. The estimates confirm our previous qualitative results in Section 3. The elasticity of adjusted gross income and wages is large and significant for singles: 1.1 for adjusted gross income and 1.3 for wages. These are the largest elasticities found in this study. This suggests that the elasticity of labor supply is potentially high for singles with low incomes. Low income earners have few possibilities of altering their tax liabilities through a change in reporting behavior and therefore the decrease in reported wages is likely to be the consequence of reduced labor supply. Note however that elasticities of low income earners can be high even if the response to taxation is small in absolute levels. This is due to the fact that the elasticity measures the response relative to the size of income (which is small for low income earners).

The elasticity is about 0 (even slightly negative) for married taxpayers. Wald estimates would not have shown accurate results because mean reversion is important in the low end of the income distribution and therefore it is important to control for income.[27] The mode of the income distribution is slightly on the left of the first kink point for singles and many singles have their permanent income around this point and are likely to react to taxes at this level. Note also that tax liabilities begin at the first kink point and therefore taxpayers may perceive more accurately this jump in marginal rates than those of other kink points. This may partly explain the high response of singles around this point. Mean reversion is stronger for married taxpayers because many low income married taxpayers are only transitorily around that point and are less likely to react to 'bracket creep' than singles.

The estimates shown in Tables IV and V broadly confirm the results of Section 3 where we noticed that married middle income earners are the most responsive but that the response of low income singles was also significant. Except for this last group, the response of wages is small, therefore income response to marginal rates may be due to changes in reporting behavior rather than reduced labor supply. Most of the response comes from the population of itemizers who is more elastic and can partly decrease its tax liability through increased itemized deductions.

# 6 Conclusion

This paper has made an attempt at identifying the impact of marginal rates on various types of reported income using 'bracket creep' as a source of variation in tax rates. The particular nature of this tax change allowed me to divide the sample between treatments and controls over the whole range of income distribution. Therefore, the estimates presented are not biased by possible underlying trends in income distribution such as mean

[27] Plugging too many covariates is not possible either, because there is only one instrument in these regressions and too many covariates would destroy the identification. I have therefore included only two covariates: $\log(z_1)$ and $taxinc_1$.

reversion or a rise in income inequality. Most results point to the general conclusion that there is a response of taxpayers to tax rates: incomes of taxpayers in the treatment groups tend to decrease more than incomes of taxpayers in the control groups. Moreover, the estimates are somewhat higher than traditional labor supply estimates but smaller than those found in previous studies using tax reforms. The estimates are in general higher for married taxpayers than for singles and higher for itemizers than for non-itemizers. Moreover, the estimates are in general higher for taxable income than for adjusted gross income and higher for adjusted gross income than for wages. This suggests that most of the response is due to changes in reporting behavior rather than reduced labor supply. Except for singles at the bottom of the income distribution,[28] wage elasticity estimates found in this study are very small and comparable to the estimates found in most labor supply studies. Part of the higher elasticities of married taxpayers compared to singles may be due to the higher responsiveness of secondary earners to tax rates, which is well documented in the literature. Indeed, wage elasticity estimates for married taxpayers are almost always higher than the estimates for singles.

Three caveats should be mentioned. First, my study captures only short term effects of marginal rates because it compares outcomes only across consecutive years. If responses to marginal rates are slow, my estimates may be smaller than medium or long term elasticities. However, several studies about behavioral responses to taxation suggest that short term responses are likely to be higher than long-term responses. Slemrod (1995) argues that the timing of economic transactions is the most responsive to tax incentives (the response of real economic activities seems to be much lower). Goolsbee (1997), using a panel data on corporate executive compensation, showed that the income tax increase of 1993 led to large short term inter-temporal income shifting but that the long term response was small. In the 'bracket creep' experience, as inflation was expected, there may also be an inter-temporal substitution effect. People know that taxes will be higher in the following year and therefore try to increase their income now at the expense of next year's income. Moreover, after Reagan's election in 1980 people knew that taxes would be

[28] In any case, this result must be considered with caution because it is based on behavior around a single kink point.

cut by 1982. This gave another incentive to shift income away from years 1980 and 1981. However, this expected reduction in taxes probably affected treatments and controls in the same way and therefore my estimates are not affected by this expectation component.

Second, as 'bracket creep' was not a tax reform, taxpayers may not have been fully aware of the marginal tax increases and thus did not respond to the change. This seems unlikely because 'bracket creep' was perceived as a major income tax event which triggered what has been called the 'tax revolt' of the late 1970s and early 1980s. If we assume that only a part of all taxpayers were aware of the effects of 'bracket creep', then the responses I measure are due only to these 'aware' taxpayers. The elasticity estimates for these taxpayers would therefore be equal to my estimates divided by the proportion of 'aware' taxpayers. However, to get elasticity estimates as high as those found in previous tax reform studies, the proportion of 'aware' taxpayers should have been unrealistically low.

Last, my study measured the response of relatively small changes in tax rates and found smaller elasticity estimates than previous studies. It may be the case that the response for larger tax rates cuts (such as ERTA or TRA) cannot be directly predicted from the results presented here. In other words, responses of taxpayers may be non-linear: a small change can lead to almost no effect while a big change can have a dramatic impact on reported income.[29]

Despite these caveats, the present study using 'bracket creep' has important advantages over studies exploiting tax reforms and has taught us interesting facts about the behavioral responses to marginal tax rates. In future work, I plan to develop the model presented in Section 4 in order to derive a general method to estimate both income and substitution effects using panel data on tax returns and several tax reforms at the same time. The method would be less dependent on structural form assumptions than most labor supply studies because the identification would come directly from tax reforms. The methodology would however impose more structure than previous tax reform studies to

[29] This is probably what happened after the TRA of 1986 for very rich taxpayers who have the possibility to change the way and the timing in which they report income. See Feenberg and Poterba (1993) and Slemrod (1996). This non-linear behavior is probably much less relevant for low and middle income earners.

allow the estimation of both income and substitution effects.

# Appendix

**Computations of Deflated Taxable Income and Predicted Taxable income:**

I denote by *exempt* the level of exemptions, by *stdded* the level of the standard deduction and by *itemz* the nominal level of itemized deductions in year 2. From taxable income in year 1 ($taxinc_1$), I compute predicted taxable income ($taxinc_p$) which is $taxinc_1$ expressed in year 2 dollars. I assume that nominal ajusted gross income and nominal itemized deductions grow at the inflation rate denoted by $\pi$. Nominal exemptions and standard deductions stay constant, therefore for non-itemizers, we have,[30]

$$taxinc_p = AGI_p - exempt = (1+\pi)AGI_1 - exempt = (1+\pi)taxinc_1 + \pi\ exempt$$

For itemizers, we have,

$$taxinc_p = (1+\pi)AGI_1 - exempt - (1+\pi)item + stdded = (1+\pi)taxinc_1 + \pi(exempt - stdded)$$

We now have to express the value of $taxinc_2$ in year 1 dollars. Again, we have to take into account the fact that *exempt* and *stdded* are not indexed, therefore we compute real taxable income in year 2 (denoted by $taxinc_{2R}$) as follows:

$$taxinc_{2R} = \frac{AGI_2}{1+\pi} - exempt = \frac{taxinc_2}{1+\pi} - \frac{\pi\ exempt}{1+\pi}$$

for non-itemizers in year 2.

$$taxinc_{2R} = \frac{AGI_2}{1+\pi} - exempt - \frac{itemz_2}{1+\pi} + stdded = \frac{taxinc_2}{1+\pi} - \frac{\pi(exempt - stdded)}{1+\pi}$$

for itemizers in year 2 such that[31] $itemz_2/(1+\pi) \leq stdded.$

[30] Note that because of inflation, a non-itemizer may become an itemizer if his potential itemized deductions are just below the standard deduction. This would change $taxinc_p$ by a small amount ($\pi\, stdded$) and we thus neglect this possibility.

[31] That is, taxpayers whose itemized deductions are large enough so that even with deflated itemized deductions, it is still advantageous to itemize in year 1.

$$taxinc_{2R} = \frac{taxinc_2}{1+\pi} - \frac{\pi\ exempt}{1+\pi} + \frac{itemz_2 - stdded}{1+\pi}$$

for itemizers in year 2 such that[32] $itemz_2/(1+\pi) < stdded$.

**Description of the grouping of instruments:**

To avoid using too weak instruments, I have grouped the instruments for each kink as follows:

- (14/16 and 16/18), (18/19 and 19/21), (21/24 and 24/26), (34/39 and 39/44), (44/49 and 49/55), (55/63 and 63/68 and 68/70) for singles
- (14/16 and 16/18), (18/21 and 21/24), (43/49 and 49/54), (54/59 and 59/64 and 64/68 and 68/70) for married taxpayers.

Therefore I have 9 instruments for each marital status instead of 15 for married taxpayers and 16 for singles. When I have grouped several kinks, I have given values proportional to $\log(1-T'_p/1-T'_1)$ for each kink and thus some instruments are no longer binary but can take 3 to 5 different values. This grouping device does not noticeably affect the results and avoids using too weak instruments.

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Figure 1: Bracket Creep Experiment, Tax Schedules

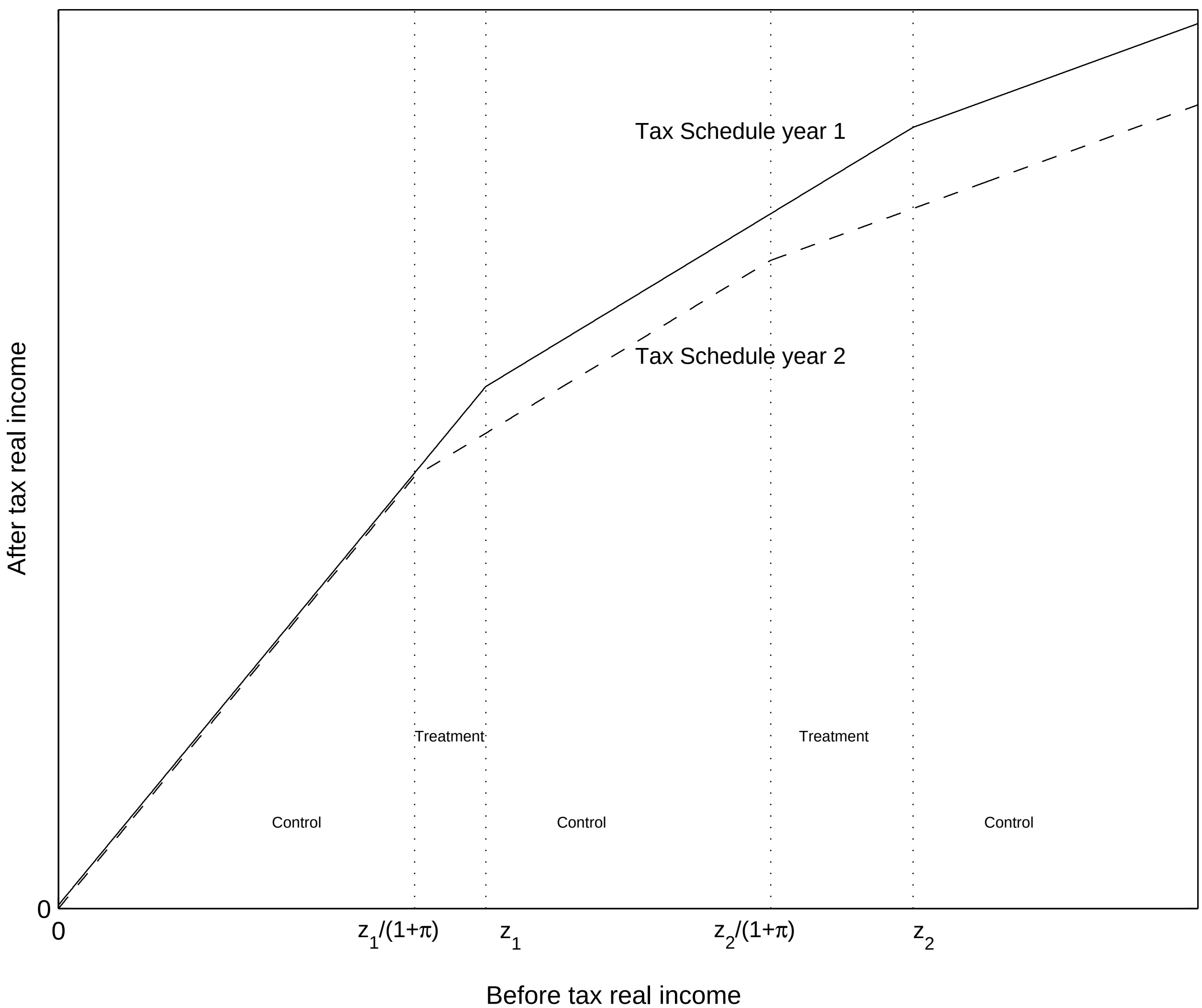


Figure 2: Bracket Creep Experiment, Marginal Rates

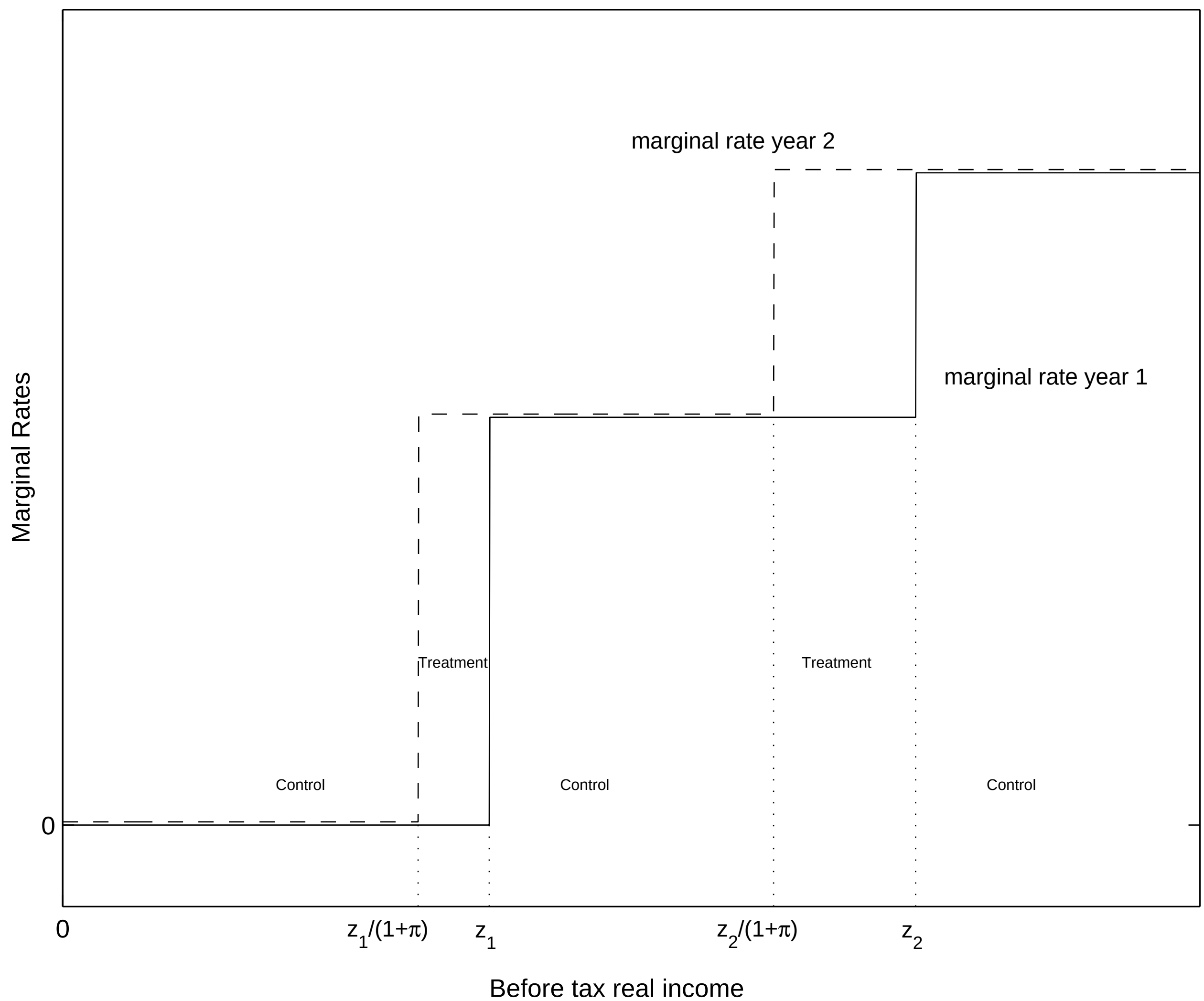


Figure 3: Shift in Marginal Rates for Married Taxpayers

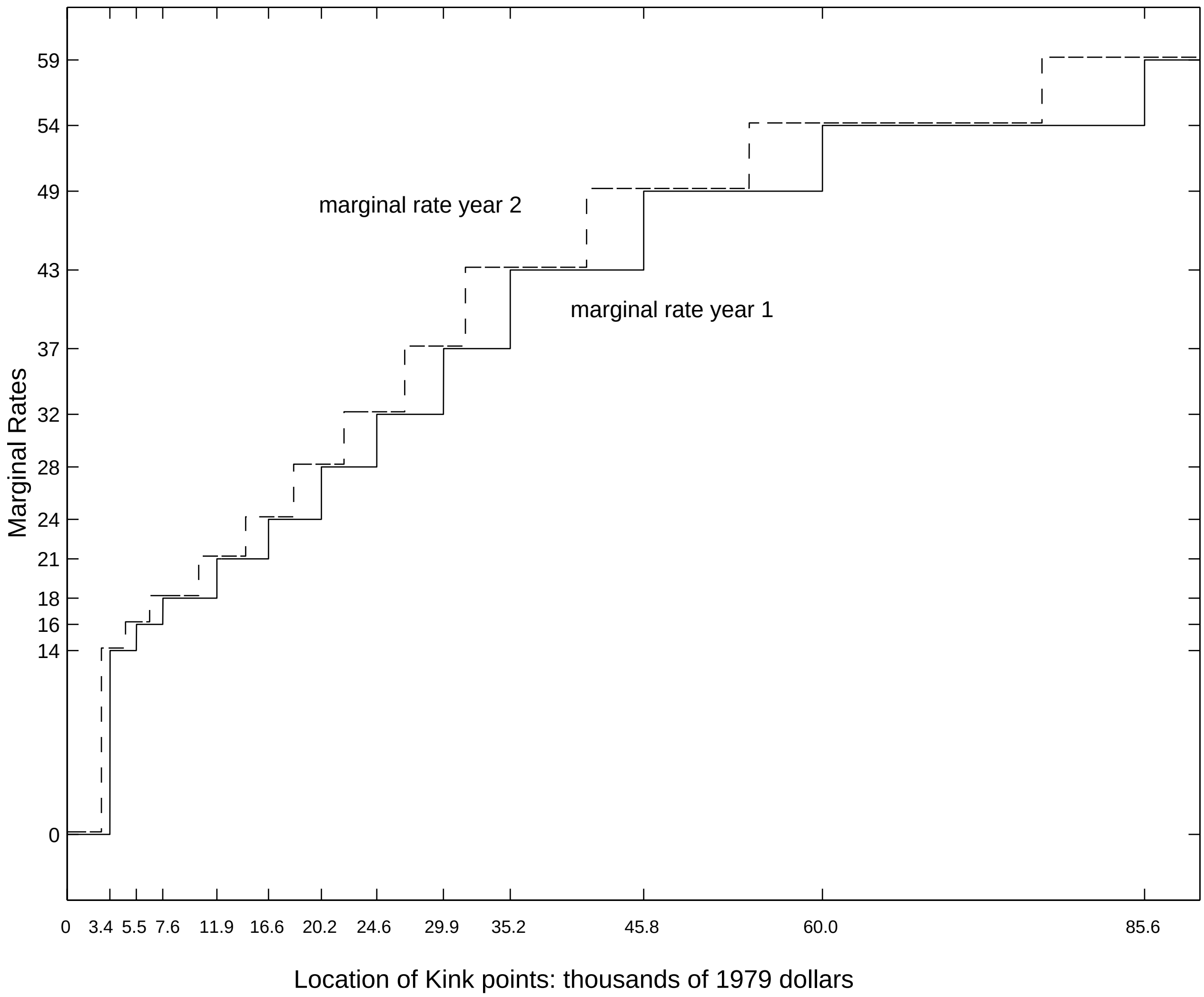


Figure 4: Shift in Marginal Rates for Single Taxpayers

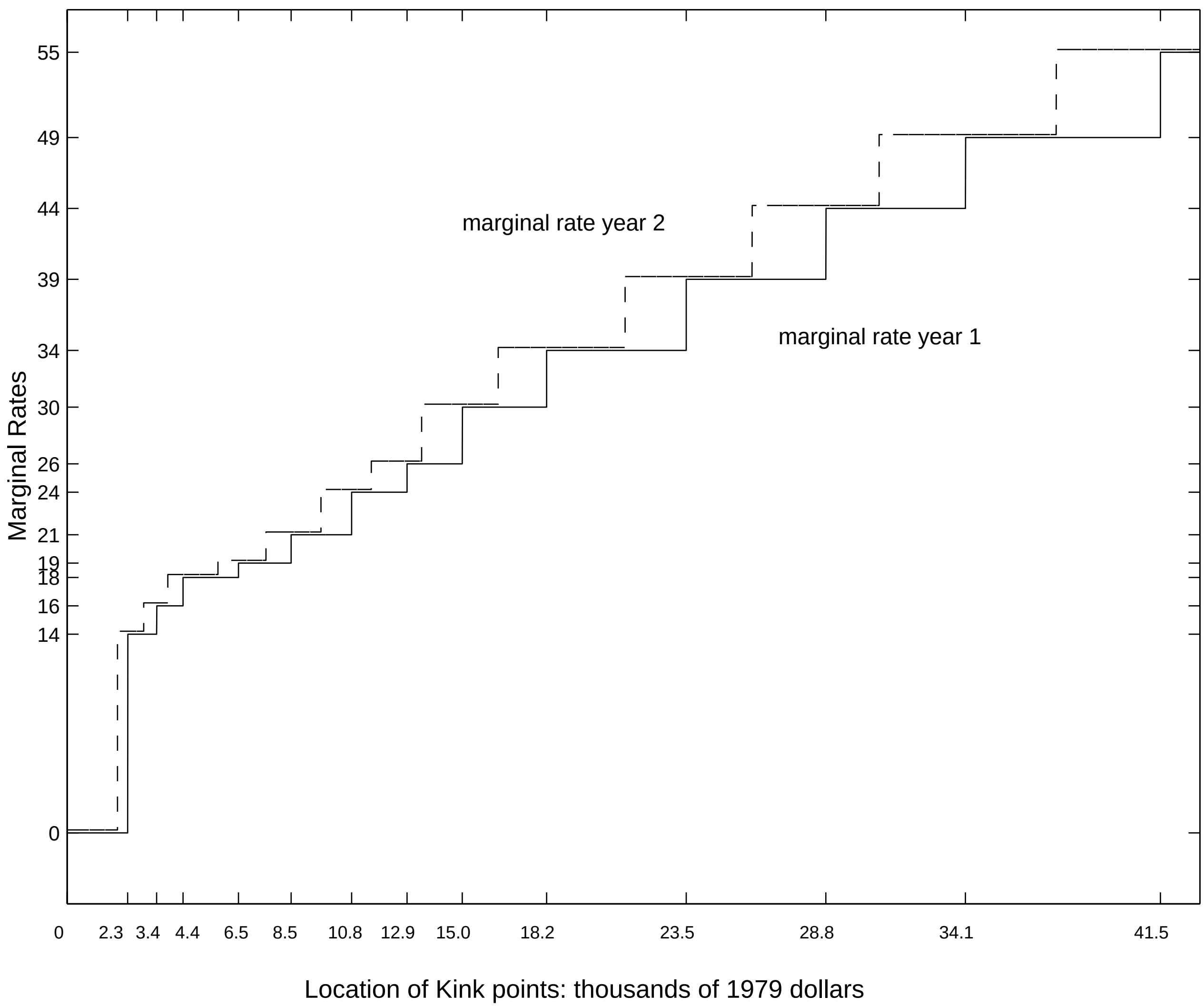
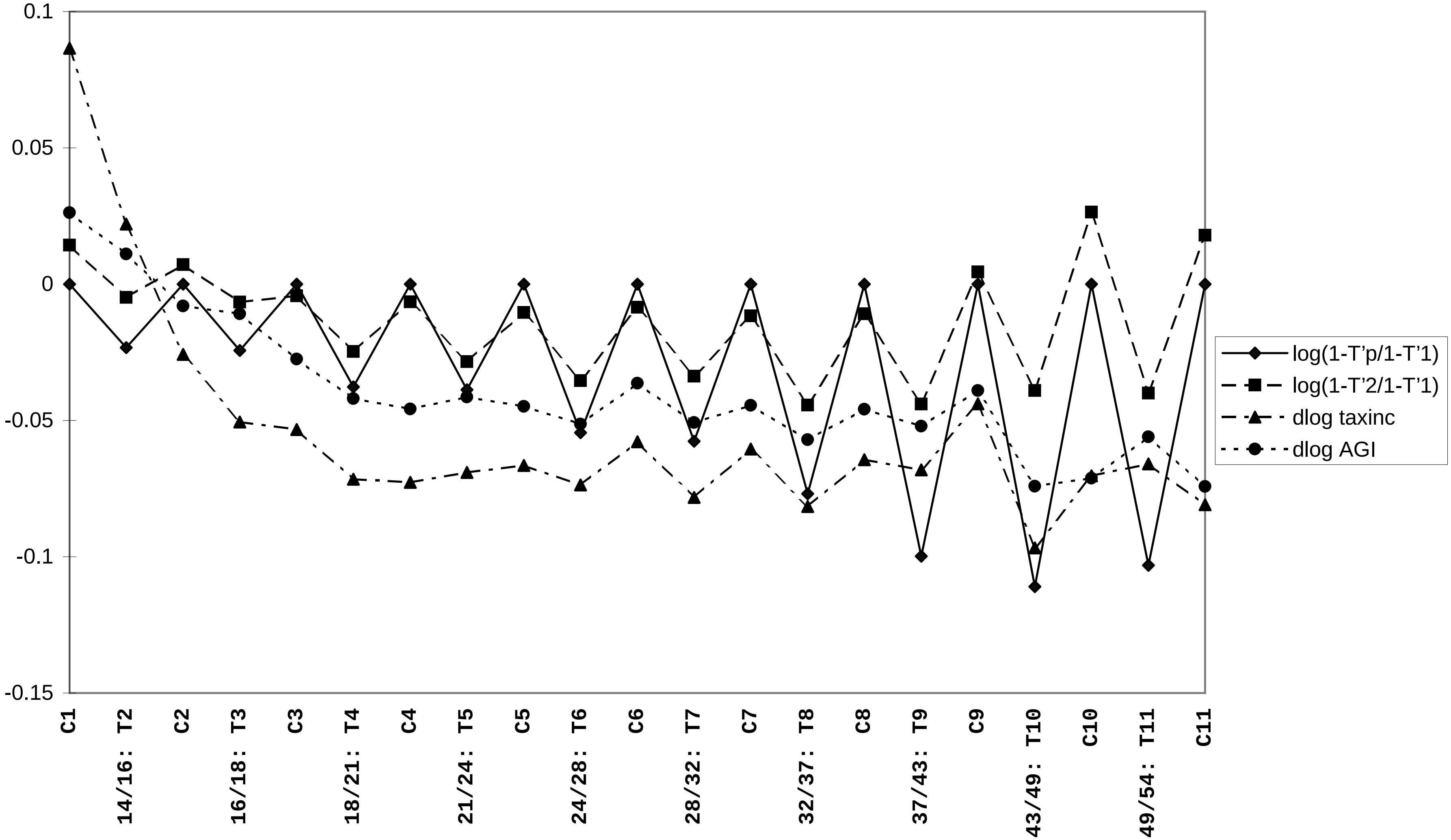
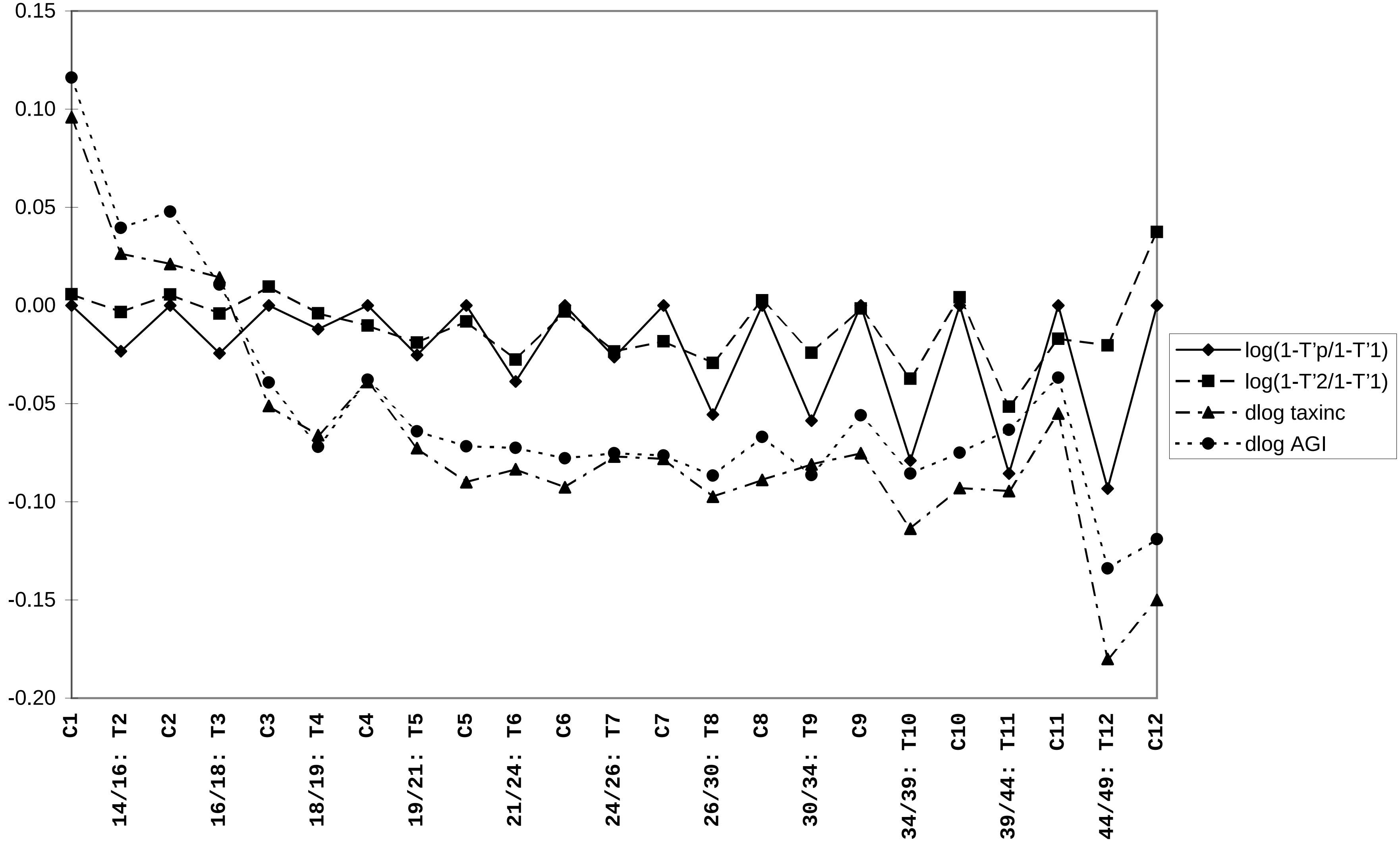


**Figure 5: Married Taxpayers**



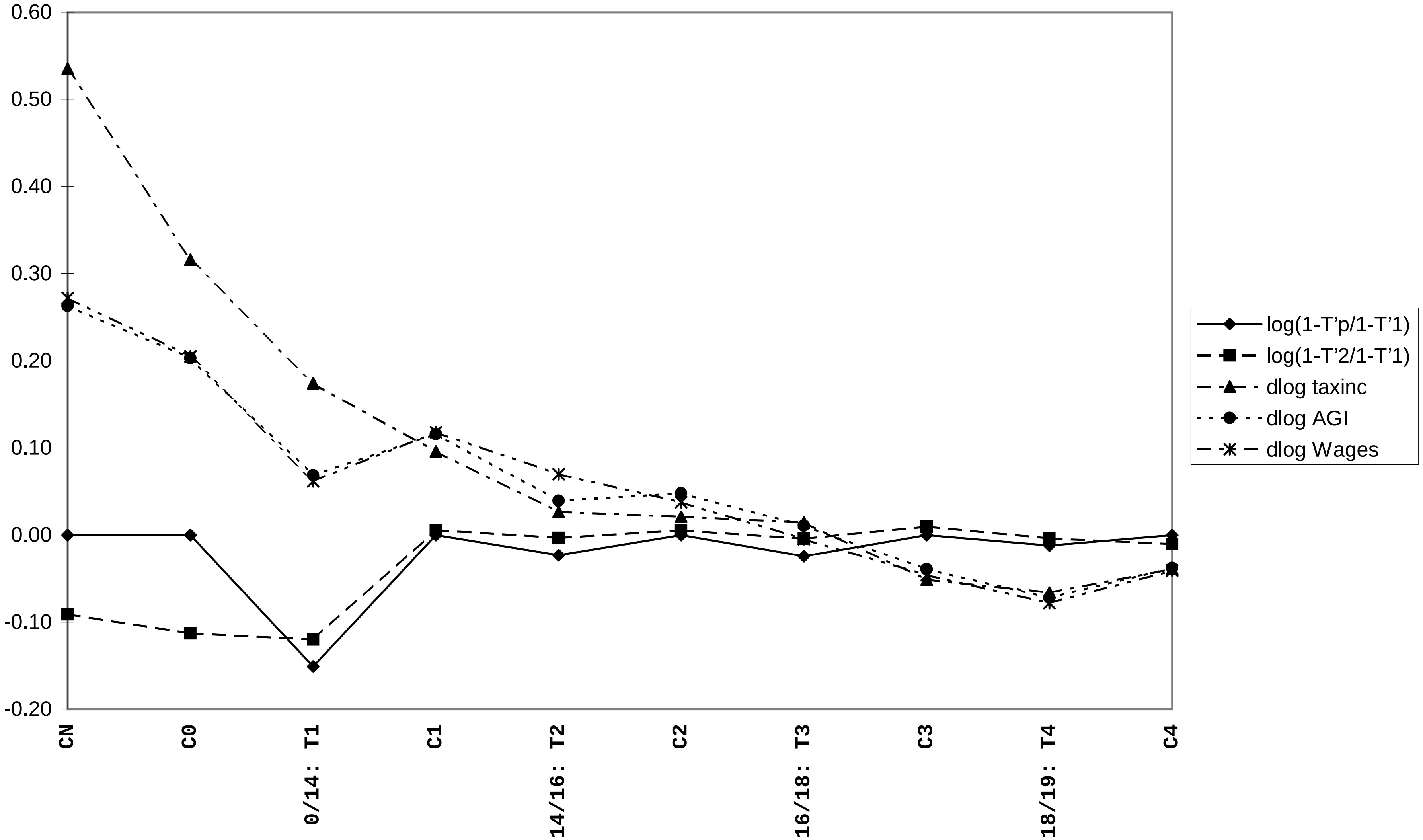

Notes: This Figure displays the log-changes in predicted net-of-tax rates (diamonds), effective net-of-tax rates (squares), taxable income (triangles) and Ajusted Gross Income (squares) for each Control and Treatment group for Married Taxpayers.

## Figure 6: Single Taxpayers




Notes: This Figure displays the log-changes in predicted net-of-tax rates (diamonds), effective net-of-tax rates (squares), taxable income (triangles) and Ajusted Gross Income (squares) for each Control and Treatment group for Single Taxpayers.

**Figure 7: Singles, low income earners**




Notes: This Figure displays the log-changes in predicted net-of-tax rates (diamonds), effective net-of-tax rates (squares), taxable income (triangles), AGI (squares) and wages (stars) for each Control and Treatment group for Low income Single Taxpayers.

# Table I: Summary statistics for Married Taxpayers

| Groups | Kinks Location | Kinks Jump | Number of observations | log(1-T'p/1-T'1) | log(1-T'2/1-T'1) | dlog of taxable income | dlog of adjusted gross income | dlog of wages |
|---|---|---|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Control N | | | 392 | 0 | -0.0723 | 0.684 | 0.131 | 0.103 |
| Control 0 | | | 852 | 0 | -0.0861 | 0.340 | 0.0859 | 0.069 |
| Treatment 1 | $3,400 | 0/14 | 605 | -0.1508 | -0.1134 | 0.141 | 0.0700 | 0.0537 |
| Control 1 | | | 1,383 | 0 | 0.0143 | 0.0865 | 0.0262 | 0.0180 |
| Treatment 2 | $5,500 | 14/16 | 783 | -0.0233 | -0.0049 | 0.022 | 0.0111 | -0.0185 |
| Control 2 | | | 1,222 | 0 | 0.0072 | -0.0258 | -0.00797 | -0.0265 |
| Treatment 3 | $7,600 | 16/18 | 1,084 | -0.0243 | -0.0066 | -0.0506 | -0.0109 | -0.0307 |
| Control 3 | | | 3,550 | 0 | -0.0043 | -0.0533 | -0.0275 | -0.0536 |
| Treatment 4 | $11,900 | 18/21 | 1,615 | -0.0377 | -0.0247 | -0.0716 | -0.0420 | -0.0542 |
| Control 4 | | | 3,264 | 0 | -0.0065 | -0.0727 | -0.0458 | -0.0531 |
| Treatment 5 | $16,000 | 21/24 | 2,241 | -0.0387 | -0.0284 | -0.0691 | -0.0414 | -0.0545 |
| Control 5 | | | 2,991 | 0 | -0.0104 | -0.0665 | -0.0448 | -0.0553 |
| Treatment 6 | $20,200 | 24/28 | 2,580 | -0.0545 | -0.0354 | -0.0737 | -0.0513 | -0.0613 |
| Control 6 | | | 2,294 | 0 | -0.0093 | -0.0578 | -0.0364 | -0.0560 |
| Treatment 7 | $24,600 | 28/32 | 2,230 | -0.0576 | -0.0338 | -0.0782 | -0.0508 | -0.0470 |
| Control 7 | | | 1,908 | 0 | -0.0116 | -0.0605 | -0.0445 | -0.0535 |
| Treatment 8 | $29,900 | 32/37 | 1,634 | -0.0769 | -0.0444 | -0.0816 | -0.0571 | -0.0797 |
| Control 8 | | | 883 | 0 | -0.0109 | -0.0644 | -0.0459 | -0.0487 |
| Treatment 9 | $35,200 | 37/43 | 971 | -0.0998 | -0.0440 | -0.0681 | -0.0521 | -0.0669 |
| Control 9 | | | 1,057 | 0 | 0.0045 | -0.0439 | -0.0390 | -0.0723 |
| Treatment 10 | $45,800 | 43/49 | 418 | -0.1109 | -0.0390 | -0.0968 | -0.0741 | -0.0754 |
| Control 10 | | | 339 | 0 | 0.0264 | -0.0703 | -0.0712 | -0.0797 |
| Treatment 11 | $60,000 | 49/54 | 151 | -0.1031 | -0.0400 | -0.0659 | -0.0560 | -0.0945 |
| Control 11 | | | 195 | 0 | 0.0179 | -0.0809 | -0.0742 | -0.0685 |
| Treatment 12 | $85,600 | 54/59 | 67 | -0.1154 | -0.0794 | 0.0097 | 0.0413 | 0.142 |
| Control 12 | | | 73 | 0 | 0.0554 | -0.1137 | -0.0812 | 0.0167 |
| Treatment 13 | $109,400 | 59/64 | 31 | -0.1301 | -0.0374 | -0.0790 | -0.0430 | 0.591 |
| Control 13 | | | 102 | 0 | 0.0388 | -0.111 | -0.0891 | -0.104 |
| Treatment 14 | $162,400 | 64/68 | 22 | -0.1177 | -0.0037 | -0.181 | -0.0448 | 0.181 |
| Control 14 | | | 26 | 0 | 0.1480 | -0.423 | -0.2588 | -0.169 |

Notes: Control N contains taxpayers whose taxable income in year 1 is between $900 and $1,600. Control 0 contains all taxpayers below Treatment 1 with taxable income in year 1 abo

## Table II: Summary statistics for Single Taxpayers

| Groups | Kinks: Location | Kinks: Jump | Number of observations | log(1-T'p/1-T'1) | log(1-T'2/1-T'1) | dlog of taxable income | dlog of adjusted gross income | dlog of wages |
|---|---|---|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Control N | | | 1,633 | 0 | -0.0911 | 0.535 | 0.263 | 0.272 |
| Control 0 | | | 1,341 | 0 | -0.113 | 0.316 | 0.203 | 0.205 |
| Treatment 1 | $2,300 | 0/14 | 741 | -0.1508 | -0.120 | 0.174 | 0.0687 | 0.0615 |
| Control 1 | | | 1,764 | 0 | 0.0057 | 0.0959 | 0.116 | 0.118 |
| Treatment 2 | $3,400 | 14/16 | 879 | -0.0233 | -0.00335 | 0.0264 | 0.0395 | 0.0698 |
| Control 2 | | | 1,045 | 0 | 0.0056 | 0.0211 | 0.0478 | 0.0376 |
| Treatment 3 | $4,400 | 16/18 | 975 | -0.0243 | -0.0042 | 0.0142 | 0.0106 | -0.0048 |
| Control 3 | | | 2,678 | 0 | 0.0096 | -0.0512 | -0.0392 | -0.046 |
| Treatment 4 | $6,500 | 18/19 | 1,155 | -0.0121 | -0.00396 | -0.066 | -0.072 | -0.078 |
| Control 4 | | | 1,770 | 0 | -0.0102 | -0.039 | -0.0378 | -0.0407 |
| Treatment 5 | $8,500 | 19/21 | 1,273 | -0.0253 | -0.0189 | -0.0727 | -0.0641 | -0.0828 |
| Control 5 | | | 1,619 | 0 | -0.0082 | -0.090 | -0.0718 | -0.0684 |
| Treatment 6 | $10,800 | 21/24 | 1,161 | -0.0387 | -0.0276 | -0.0834 | -0.0725 | -0.0981 |
| Control 6 | | | 868 | 0 | -0.0031 | -0.0926 | -0.0778 | -0.0884 |
| Treatment 7 | $12,900 | 24/26 | 1,085 | -0.0263 | -0.0235 | -0.0769 | -0.0753 | -0.0681 |
| Control 7 | | | 522 | 0 | -0.0183 | -0.0781 | -0.0764 | -0.091 |
| Treatment 8 | $15,000 | 26/30 | 972 | -0.0555 | -0.0293 | -0.0974 | -0.0866 | -0.099 |
| Control 8 | | | 810 | 0 | 0.0026 | -0.0888 | -0.067 | -0.084 |
| Treatment 9 | $18,200 | 30/34 | 687 | -0.0587 | -0.0241 | -0.0810 | -0.0864 | -0.0777 |
| Control 9 | | | 900 | 0 | -0.0016 | -0.0753 | -0.0560 | -0.0896 |
| Treatment 10 | $23,500 | 34/39 | 384 | -0.0790 | -0.0373 | -0.1137 | -0.0856 | -0.087 |
| Control 10 | | | 234 | 0 | 0.0041 | -0.093 | -0.075 | -0.112 |
| Treatment 11 | $28,800 | 39/44 | 177 | -0.0856 | -0.0515 | -0.0946 | -0.0633 | -0.100 |
| Control 11 | | | 91 | 0 | -0.0170 | -0.055 | -0.0368 | -0.0824 |
| Treatment 12 | $34,100 | 44/49 | 67 | -0.0932 | -0.0203 | -0.210 | -0.134 | -0.081 |
| Control 12 | | | 47 | 0 | 0.0375 | -0.171 | -0.119 | -0.490 |
| Treatment 13 | $41,500 | 49/55 | 25 | -0.1256 | -0.0370 | -0.094 | -0.0615 | -0.0528 |
| Control 13 | | | 48 | 0 | 0.0390 | -0.0975 | -0.134 | -0.108 |
| Treatment 14 | $55,300 | 55/63 | 11 | -0.1960 | -0.169 | 0.0271 | 0.0453 | -0.049 |
| Control 14 | | | 25 | 0 | 0.0651 | -0.142 | -0.099 | -0.0583 |
| Treatment 15 | $81,800 | 63/68 | 3 | -0.1450 | 0.0652 | -0.304 | -0.328 | -0.254 |
| Control 15 | | | 4 | 0 | 0.0565 | -0.111 | -0.0674 | -0.416 |

Notes: Control N contains taxpayers whose taxable income in year 1 is between $900 and $1,500. Control 0 contains all taxpayers below Treatment 1 with taxable income in year 1 ab

## Table III: Wald estimates

| Kink | | Taxable income | | | | | AGI | Wages |
|---|---|---|---|---|---|---|---|---|
| Location | jump in marginal rates | Difference treatment/control in dlog(taxinc) | log(1-T'p/1-T'1) treatment group | Difference treatment/control in log(1-T'2/1-T'1) | Reduced form estimate ((3) divided by (4)) | Wald estimate ((3) divided by (5)) | Wald estimate | Wald estimate |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **PANEL A: Married taxpayers** | | | | | | | | |
| $16,000 | 21/24 | 0.0006 | -0.0387 | -0.0200 | -0.016 | -0.032 | -0.188 | 0.016 |
| | | | | | (0.281) | (0.569) | (0.398) | (0.530) |
| $20,200 | 24/28 | -0.0110 | -0.0545 | -0.0255 | 0.202 | 0.431 | 0.403 | 0.229 |
| | | | | | (0.173) | (0.376) | (0.298) | (0.371) |
| $24,600 | 28/32 | -0.0192 | -0.0576 | -0.0235 | 0.333* | 0.817 | 0.464 | -0.327 |
| | | | | | (0.163) | (0.483) | (0.333) | (0.377) |
| $29,900 | 32/37 | -0.0199 | -0.0769 | -0.0330 | 0.258* | 0.602 | 0.363 | 0.838* |
| | | | | | (0.130) | (0.340) | (0.260) | (0.364) |
| $35,200 | 37/43 | -0.0149 | -0.0998 | -0.0415 | 0.172 | 0.398 | 0.288 | 0.275 |
| | | | | | (0.120) | (0.335) | (0.281) | (0.423) |
| $45,800 | 43/49 | -0.0465 | -0.1109 | -0.0488 | 0.419* | 0.987* | 0.580 | 0.027 |
| | | | | | (0.162) | (0.510) | (0.429) | (0.554) |
| $60,000 | 49/54 | 0.0083 | -0.1031 | -0.0633 | -0.081 | -0.131 | -0.331 | 0.206 |
| | | | | | (0.300) | (0.503) | (0.455) | (0.841) |
| **PANEL B: Singles taxpayers** | | | | | | | | |
| $10,800 | 21/24 | 0.0075 | -0.0387 | -0.0212 | -0.194 | -0.354 | -0.071 | 1.133 |
| | | | | | (0.485) | (0.782) | (0.764) | (1.103) |
| $15,000 | 26/30 | -0.0128 | -0.0555 | -0.0237 | 0.230 | 0.540 | 0.671 | 0.489 |
| | | | | | (0.317) | (0.832) | (0.815) | (0.865) |
| $18,200 | 30/34 | 0.0007 | -0.0587 | -0.0245 | -0.012 | -0.028 | 1.052 | -0.327 |
| | | | | | (0.299) | (0.669) | (0.861) | (0.659) |
| $23,500 | 34/39 | -0.0347 | -0.0790 | -0.0369 | 0.440 | 0.942 | 0.661 | -0.184 |
| | | | | | (0.306) | (0.711) | (0.609) | (0.598) |
| $28,800 | 39/44 | -0.0122 | -0.0856 | -0.0497 | 0.143 | 0.246 | -0.019 | -0.056 |
| | | | | | (0.467) | (0.850) | (0.627) | (0.720) |

Notes: The numbers in column (3) are calculated, using Tables I and II, as the difference between income in treatment group and the average of the income in the two surrounding control groups (weighted by the number of observations). Similar calculations are perfomed in column (5) for the change in marginal rates. Standard errors in parenthesis. * for estimates significant at 5% level.

# Table IV: 2SLS Elasticity Estimates
## All income earners and Middle income earners

| | All income earners: Taxable income (1979 $) | | | Middle income earners: Taxable income (1979 $) | | |
|---|---|---|---|---|---|---|
| | Singles: $3,000-$40,000-Married: $5,000-$70,000 | | | Singles: $12,000-$28,000. Married: $16,000-$36,000 | | |
| | Taxable income | AGI | Wages | Taxable income | AGI | Wages |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **PANEL A: Married and Single taxpayers** | | | | | | |
| PANEL A1: Itemizers and non itemizers | | | | | | |
| log(1-T'2)/(1-T'1) | 0.282 | 0.181 | 0.080 | 0.395* | 0.334* | 0.120 |
| | (0.199) | (0.157) | (0.188) | (0.199) | (0.165) | (0.196) |
| N. obs. | 49,816 | 50,326 | 44,993 | 21,018 | 21,084 | 19,800 |
| PANEL A2: Itemizers | | | | | | |
| log(1-T'2)/(1-T'1) | 0.393 | 0.356* | 0.105 | 0.619* | 0.374* | 0.096 |
| | (0.244) | (0.178) | (0.232) | (0.265) | (0.197) | (0.246) |
| N. obs. | 18,764 | 18,906 | 17,210 | 11,546 | 11,590 | 11,003 |
| PANEL A3: Non itemizers | | | | | | |
| log(1-T'2)/(1-T'1) | -0.046 | -0.089 | -0.012 | 0.017 | 0.191 | 0.183 |
| | (0.296) | (0.241) | (0.287) | (0.298) | (0.271) | (0.320) |
| N. obs. | 31,052 | 31,420 | 27,783 | 9,472 | 9,494 | 8,797 |
| **PANEL B: Married taxpayers** | | | | | | |
| PANEL B1: Itemizers and non itemizers | | | | | | |
| log(1-T'2)/(1-T'1) | 0.389 | 0.202 | 0.087 | 0.437* | 0.383* | 0.272 |
| | (0.217) | (0.154) | (0.197) | (0.240) | (0.190) | (0.242) |
| N. obs. | 30,675 | 30,929 | 28,260 | 15,630 | 15,675 | 14,947 |
| PANEL B2: Itemizers | | | | | | |
| log(1-T'2)/(1-T'1) | 0.651* | 0.421* | 0.231 | 0.705* | 0.521* | 0.332 |
| | (0.274) | (0.186) | (0.244) | (0.305) | (0.234) | (0.289) |
| N. obs. | 15,924 | 16,033 | 15,015 | 9,964 | 9,998 | 9,632 |
| PANEL B3: Non itemizers | | | | | | |
| log(1-T'2)/(1-T'1) | -0.091 | -0.193 | -0.167 | -0.148 | 0.028 | 0.114 |
| | (0.345) | (0.252) | (0.327) | (0.384) | (0.314) | (0.436) |
| N. obs. | 14,751 | 14,896 | 13,245 | 5,666 | 5,677 | 5,315 |
| **PANEL C: Single taxpayers** | | | | | | |
| log(1-T'2)/(1-T'1) | 0.170 | 0.188 | -0.077 | 0.275 | 0.472 | -0.155 |
| | (0.451) | (0.376) | (0.406) | (0.442) | (0.454) | (0.439) |
| N. obs. | 19,141 | 19,397 | 16,733 | 5,388 | 5,409 | 4,853 |

Notes: All regressions include log(z1), taxinc1, taxinc1^2, taxinc1^3 and taxinc1^4 as control variables. Regresssions in panel A control in addition for marital status. Regressions including both itemizers and non itemizers control in addition for itemizer status. Standard errors in parenthesis. * for estimates significant at 5% level.

# Table V: 2SLS Elasticity Estimates
## High income earners and Low income earners

| | High income taxpayers Taxable income (1979 dollars) Singles:$21,000-$65,000-Married:$31,000-$90,000 | | | Low income taxpayers Taxable income (1979 dollars) Singles:$0-$3,400-Married:$0-$5,000 | |
|---|---|---|---|---|---|
| | Dependent variable | | | Dependent variable | |
| | Taxable income | Adjusted gross income | Wages | Adjusted gross income | Wages |
| | (1) | (2) | (3) | (4) | (5) |
| **PANEL A: Married and Single taxpayers** | | | | | |
| log(1-T'2)/(1-T'1) | 0.277 | 0.022 | -0.441 | | |
| | (0.252) | (0.197) | (0.282) | | |
| N. obs. | 4,618 | 4,629 | 4,174 | | |
| **PANEL B: Married taxpayers** | | | | | |
| log(1-T'2)/(1-T'1) | 0.332 | 0.067 | -0.342 | -0.289 | -0.052 |
| | (0.268) | (0.218) | (0.335) | (0.281) | (0.457) |
| N. obs. | 3,466 | 3,474 | 3,207 | 3,895 | 2,733 |
| **PANEL C: Single taxpayers** | | | | | |
| log(1-T'2)/(1-T'1) | 0.159 | -0.223 | -0.587 | 1.082* | 1.310* |
| | (0.597) | (0.409) | (0.495) | (0.433) | (0.480) |
| N. obs. | 1,152 | 1,155 | 967 | 8,713 | 7,622 |

Notes: Regressions for high incomes include log(z1), taxinc1, taxinc1^2, taxinc1^3 and taxinc1^4 as controls. Regressions for low incomes include log(z1), taxinc1 as control variables. All regressions include itemization status as control variables. Regresssions in panel A control in addition for marital status.
Standard errors in parenthesis. * for estimates significant at 5% level.