American Economic Review 2016, 106(3): 807–835
http://dx.doi.org/10.1257/aer.20131189

Taxpayer Confusion: Evidence from the Child Tax Credit†

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We develop an empirical test for whether households understand or misperceive their marginal tax rate. Our identifying variation comes from the loss of the Child Tax Credit when a child turns 17. Using this age discontinuity, we find that despite this tax liability increase being lump-sum and predictable, households reduce their reported wage income upon discovering they have lost the credit. This finding suggests that households misinterpret at least part of this tax liability change as an increase in their marginal tax rate. This evidence supports the hypothesis that tax complexity can cause confusion and leads to unintended behavioral responses. (*JEL* D12, D14, H24, H31)

A fundamental assumption in public finance is that individuals consider taxes when making economic choices. Indeed, a voluminous literature shows that taxes significantly influence behavior along several margins including labor supply, portfolio allocations, and savings.[1] The literature typically assumes that individuals understand the tax schedule they face, and therefore the standard interpretation is that these behavioral responses arise from changes in tax rates.

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† Go to http://dx.doi.org/10.1257/aer.20131189 to visit the article page for additional materials and author disclosure statement(s).

[1] See, for example, Eissa (1995) or Eissa and Liebman (1996) for labor force participation of women, Looney and Singhal (2005) for the intertemporal elasticity of labor earnings, Goolsbee (2000) for the timing of income realization, Poterba and Samwick (2003) for risk-taking and portfolio behavior, and Feldstein (1995); Auten and Carroll (1999); Gruber and Saez (2002); and Kopczuk (2005) for reported and taxable income. On the other hand, Saez (2004) finds that only the top 1 percent of incomes show evidence of behavioral responses to taxation.

However, the US tax code is highly complex.[2] As a result, it may be costly for taxpayers to fully understand the tax rates that they face and, thus, their tax-induced incentives. The costs associated with learning details of tax provisions include cognitive effort, time, and money. In response to these costs, some taxpayers may be rationally inattentive to their past, current, and future tax environment.[3] Instead, taxpayers may opt to form their beliefs about the relevant tax rates by applying simple heuristics based on information that becomes readily available when interacting with the tax system. For example, using a laboratory experiment, de Bartolome (1995) finds that if taxpayers are uninformed of their marginal tax rate (MTR) but are aware of their potential tax liability for various levels of income, many behave as though their MTR is given by their average tax rate (ATR) (a type of behavior later dubbed "ironing" by Liebman and Zeckhauser 2004).

In this paper, we devise a test for whether households understand or misperceive changes in their tax schedule. We identify a provision in the tax code that generates a predictable, lump-sum change in tax liability. This variation comes from an age-discontinuity in eligibility for the Child Tax Credit (CTC). To qualify for the CTC in a given year, a child must be younger than 17 at the end of that year. For example, a household with a child who turns 17 in December of year t is not eligible for the CTC for that child for that year; a household with a child who turns 17 in January of the following year is eligible for the CTC for that child for year t (but not for year $t + 1$). This credit loss generates variation in tax liabilities that is lump-sum, predictable in advance, and plausibly exogenous.[4] We examine how this variation affects reported household wage and salary income.

To motivate our empirical strategy, we develop a model of the evolution of beliefs over the tax system in the presence of potential misperceptions of its underlying parameters. We identify three types of taxpayers that differ in their beliefs about their current and future tax schedules, with different implications for their labor supply response. First, *fully informed* parents anticipate the credit loss in advance and so, barring liquidity constraints, should not adjust their labor supply. Second, *ex post informed* parents fail to anticipate the CTC loss, but understand that they experienced a lump-sum credit loss, ex post. These households may increase their labor supply in response to the negative income shock. Lastly, *ex post confused* parents fail to anticipate the credit loss and additionally misinterpret why their tax liability increased. In particular, this type attributes at least some of the tax liability increase to an increase in their MTR. If the substitution effect dominates the income effect, ex post confused parents will decrease their labor supply in response to losing the CTC.[5] Under our model, a negative wage income response can only be supported by households misperceiving at least some of the credit loss as an increase in their MTR, on average.

We construct a panel from population-level US federal tax returns filed between 2004 and 2011. We identify all married-couple households with a child who turns 17 in January or December during this period. Using a regression-discontinuity (RD) design, we find that upon losing the CTC, households reduce their reported wage

[2] For an expert evaluation, see the President's Advisory Panel on Federal Tax Reform (2005).

[3] For references to rational inattention, see Sims (2003) and Reis (2006).

[4] Of course, this age-eligibility rule generates a lump-sum tax credit loss, conditional on claiming the child.

[5] Although changes to reported wage income could come through an increase in tax evasion or tax avoidance behaviors, we refer to all such responses as a "labor supply" or "wage income" response.

income by approximately 0.5 percent relative to households who have just retained the credit for another year. This result is obtained even though losing the CTC has no mechanical impact on MTRs for the selected sample. We show that this effect is not driven by a direct effect of child aging or a spurious correlation between the timing of birth and income. We further show that the CTC has no significant effect on reported wage income in the years prior to its loss, suggesting that the effect is not driven by a strategic re-timing of income. We thus interpret our result as evidence that taxpayers confuse at least part of the credit loss as due to an increase in their MTR. Finally, we find that this confusion is concentrated among younger households, lower-income households, and households without self-employment income or capital gains realizations. These patterns suggest that experience and exposure to more pieces of the income tax code can ameliorate the ex post confusion that might otherwise occur upon losing the CTC.

To understand better the magnitude of our estimated treatment effect, we interpret it in terms of the implied elasticity of wage income. This exercise is complicated because it requires assumptions about households' beliefs about the MTR that they face prior to the credit loss, and the portion of the credit loss that they misattribute to a change in their MTR. As such, we do this elasticity calculation under a number of alternative assumptions. First, attributing the entire change in tax liability to a change in the perceived MTR, of which "ironing" is a special case, implies a wage income elasticity of 0.3. We then generalize this intuition and allow households to perceive their tax liability increase as due to a mixture of an increase in their MTR and a lump-sum change in liability. Using a benchmark elasticity estimate from the literature, we find support for households confusing between one-quarter and one-half of the CTC loss as due to an increase in their MTR.

Our paper contributes to a broader literature on taxpayers' misperceptions of the tax system, which we view as encompassing the oft-intertwined issues of tax complexity and salience. For example, evidence from surveys reveals that taxpayers are often unable to report their correct MTRs on income (Brown 1968; Fujii and Hawley 1988; Romich and Weisner 2000).[6] Using administrative data, Chetty, Friedman, and Saez (2013) document that responsiveness to Earned Income Tax Credit (EITC) differs by how knowledgeable taxpayers are about the policy in a given neighborhood. Tax misperceptions have also been investigated in the case of consumption taxation. A growing literature provides ample evidence that individuals are more responsive to consumption taxes that are more visible (i.e., more salient) than the ones that are less visible even if the final tax-inclusive price is the same (Chetty, Looney, and Kroft 2009; Goldin and Homonoff 2013; Feldman and Ruffle 2015; Feldman, Goldin, and Homonoff 2015).[7]

[6] There is also evidence of net wage illusion in real-effort experiments. Subjects tend to work harder under higher per-piece gross wages, irrespective of the fact that the net per-piece wage is identical (Blaufus et al. 2013; Fochmann and Weimann 2013; Fochmann et al. 2013; Abeler and Jäger 2015). Also, working at the intersection of income and consumption taxation, Blumkin, Ruffle, and Ganun (2012) find that effort is smaller under an income tax than under an equivalent consumption tax.

[7] Analogous findings appear in the literature on salience of prices and their components. See, for example, Hossain and Morgan (2006) and Brown, Hossain, and Morgan (2010), who examine salience of shipping charges in online transactions, Finkelstein (2009), who studies salience of electronically debited toll charges, and Choi, Laibson, and Madrian (2010), who examine salience of fees on index funds. Another strand of literature investigates strategic use of such "price obfuscation" in product markets. See, for example, Gabaix and Laibson (2006); Ellison

In our setting, the complexity of income tax policy can lead to confusion over the slope and level of the tax schedule. Just as our results suggest that a lump-sum change in tax liability is misinterpreted as partly due to an increase in the slope of the tax schedule, we may similarly anticipate that policies that change MTRs are partially misinterpreted as a level shift in the tax schedule. Generally, tax policy changes that are misunderstood may lead to unintended behavioral consequences, with implications for welfare. For example, Liebman and Zeckhauser (2004) show that under "ironing," some of the deadweight loss that arises from high MTRs is eliminated because individuals are less sensitive to the true marginal rate.[8]

Our empirical framework could be adapted for other settings. For example, one could use other child age-relevant discontinuities, such as the EITC or child deduction. These experiments, however, are inferior to the CTC that we use in this study. In both cases, a child aging out of eligibility could trigger an actual change in the MTR. While such changes may be subject to much confusion, separating responses to actual changes in the MTR from perceived changes would be empirically challenging.[9] In addition, the discontinuity for eligibility for the EITC and child deduction is not sharp at a single age: eligibility ends at age 19 unless the child is a full-time student, in which case eligibility ends at age 24. The CTC has the strong advantage of having a sharp discontinuity at age 17 for all families, and this credit loss occurs at an age when the child is more likely to remain co-resident with the family. The age-discontinuity for CTC eligibility provides a clean, natural experiment for testing taxpayer perceptions. The lessons from this setting provide insights into the effect of confusion over myriad tax provisions, including the loss of the EITC, health care subsidies, and other provisions of the tax system.

I. Model

We formalize a model of the evolution of taxpayer beliefs over the parameters of the tax schedule in the presence of potential misperceptions. Suppose that a long-lived household faces a linear tax schedule in every period s with MTR given by τ_s and demogrant (a lump-sum deduction in tax liability) given by D_s. A household with taxable income y_s thus faces a tax liability of $T_s(y_s) = -D_s + \tau_s y_s$. In practice, income tax schedules are piecewise linear, so the proposed schedule can be thought of as a local approximation of a more complicated tax schedule in the relevant range. The two parameters typically change from one period to another. We assume that they are in principle knowable for the current as well as all future and past time periods. To achieve this knowledge, however, households must expend cognitive, monetary, or time resources. Given that such resources are costly, households may opt for a less-than-full knowledge of their tax schedules. This setup is motivated by

(2006); Spiegler (2006); Carlin (2009); and Heidhues, Kőszegi, and Murooka (2014) for theoretical approaches; and Ellison and Ellison (2009) for an empirical investigation.

[8] In addition, several recent papers examine the welfare implications of consumption tax salience, including Chetty, Looney, and Kroft (2009); Finkelstein (2009); Goldin (2015); and Reck (2013).

[9] A focus on the EITC would likely also include single head of households whose labor supply decisions are more complicated. In addition, the impact on the MTR would depend on the number of children. One may argue that an EITC household on the flat portion of the EITC may face a lump sum change upon a child aging out of eligibility but this range is small and would also rely upon lining up the plateau regions of the EITC schedules for one, two, or three children.

the observation that there are many perfectly predictable changes to household tax schedules. For example, some provisions are tied to a dependent's age, and some tax policies come with sunset provisions. Moreover, there could be tax consequences of planned actions, such as mortgage interest payments. Such changes are, under a stable tax system, perfectly predictable in advance.[10] Households file their tax return for year s at the beginning of year $s + 1$. At this point, households are aware of $T_s(y_s)$, though not necessarily of the underlying tax parameters that determined it.

We define three types of households: *fully informed*, *ex post informed*, and *ex post confused*. Fully informed households know perfectly their current tax schedule. Moreover, these households correctly perceive their past tax schedules and correctly predict their future tax schedules. In contrast, ex post informed and ex post confused households do not necessarily know their current tax schedule. We assume that, in period $t + 1$, both types use their beliefs about their most recent tax parameters, τ_t and D_t, as their expectation for their current and all future tax schedules.[11] Thus, both types fail to anticipate future tax schedule changes.

We distinguish the latter two types by their ex post learning when their tax schedule changes. Ex post informed households perfectly learn about their period t tax schedule upon filing their tax return in period $t + 1$. This learning could be rationalized by households paying attention to their tax parameters when calculating their tax liability. As a result, in period $t + 1$, ex post informed households have perfect knowledge of all previous tax schedules. Given our previous assumption on expectation formation, these households use this new tax schedule as their expectation of future tax schedules.

In contrast, when filing their period t tax return, ex post confused households note only that their tax liability differs from their expectations, but do not fully learn their new tax schedule. We assume that these households use a simple updating rule in which some fraction α of the surprise is attributed to an MTR innovation and the remaining fraction $1 - \alpha$ is attributed to a demogrant innovation. Denoting τ_s^t, D_s^t, and T_s^t to be period t's expectations of τ_s, D_s, and T_s, for all $s \geq t$,

(1)
$$\tau_s^{t+1} = \tau_t^t + \alpha \frac{T_t(y_t) - T_t^t(y_t)}{y_t}$$

$$D_s^{t+1} = D_t^t - (1 - \alpha)\{T_t(y_t) - T_t^t(y_t)\}.$$

Importantly, these households typically misunderstand the tax schedule changes they experience.[12]

[10] This setup assumes away future tax schedule innovations due to less-than-perfectly predictable outcomes of tax policy changes or surprises in income or other variables affecting the effective MTR and demogrant. At the cost of a more complicated exposition, the analysis can be extended to account for such shocks. See Feldman and Katuščák (2006) for more details.

[11] This simple adaptive process of expectation formation satisfies the Law of Iterated Expectations and hence is internally consistent in a minimal way. For the purpose of the empirical prediction, this assumption can be relaxed somewhat as long as a higher realization of $T_t(y_t)$ increases the expectation of future MTRs and reduces the expectation of future demogrants among the ex post confused households, each by the same amount in the current and all future time periods.

[12] If we assume that the household is only confused about the MTR, but not about the demogrant, and the demogrant does not change in time, an unexpected tax liability increase will increase the expected future MTRs by the magnitude of the surprise in the realized ATR (this corresponds to $\alpha = 1$ in equation (1)). The special case

To motivate our empirical strategy, consider the effect of a predictable permanent decrease in the demogrant in period t on household labor supply choices.[13] Fully informed households anticipate this change and have incorporated it into their long-term optimization problem. In the absence of credit constraints, there is no impact on household labor supply at the time the credit is lost.[14] Ex post informed households are surprised by a higher than expected period t tax liability upon filing their tax return in period $t + 1$, but they perfectly understand that the increase was due to a decrease in the demogrant. They also expect that the demogrant has decreased from that period onward. As a result, there is a negative income effect that (weakly) increases labor supply. In contrast, ex post confused households are surprised by a higher than expected period t tax liability but, in line with (1), form a higher expectation of $\boldsymbol{\tau}_{s=t}^{\infty}$ and a lower expectation of $\mathbf{D}_{s=t}^{\infty}$. The perceived increase in MTR generates both a substitution effect and an analogous income effect, while the perceived decrease in the demogrant generates an analogous income effect. The substitution effect reduces labor supply, and the income effect increases labor supply (assuming leisure is a normal good); the overall impact on labor supply depends on the relative magnitudes of these effects. Put together, our model predicts that these agent types respond differently to an unexpected decrease in the demogrant. In particular, households who reduce their labor supply in response must be ex post confused and have a dominant substitution effect.

II. Identification Strategy and Empirical Implementation

To test how taxpayers interpret changes in their tax liability, we identify a source of predictable, lump-sum variation in after-tax income. This variation comes from an age-based discontinuity generated by the eligibility rules for the Child Tax Credit (CTC). The CTC is a nonrefundable tax credit available for any eligible child below 17 years of age as of December 31 of the tax year.[15] The CTC was introduced in 1998, and the credit amount was initially set at \$400 per eligible child. The credit amount has increased over time, and we focus on the 2004–2011 period when the CTC was \$1,000 per eligible child.[16] In addition to the CTC, the Additional Child Tax Credit (ACTC) was introduced, which provides limited refundability of the nonrefundable part of the CTC for families with three or more qualifying children.[17] In 2001, the ACTC was expanded to allow any family to claim the nonrefundable

of zero demogrant corresponds to the "ironing" hypothesis of Liebman and Zeckhauser (2004). Under ironing, a household believes that its current MTR is equal to its previous period's ATR.

[13] Similar thought experiments for changes to the MTR can easily be conducted.

[14] For all types, a binding credit constraint results in households consuming less and working more because of inability to smooth consumption and leisure using future income.

[15] There are several other provisions in the tax code that make the tax schedule a function of a dependent child's age, such as the loss in the eligibility for the personal exemption and the EITC for a dependent child who turns 19 (or 24, if a full-time student). This provision has been exploited by Looney and Singhal (2005) and Dokko (2008) in order to estimate the effect of marginal tax rates on labor supply.

[16] The CTC was increased to \$500 for the 1999 and 2000 tax years, to \$600 for the 2001 and 2002 tax years, and then to \$1,000 for the 2004 tax year, where it has remained since then. 2003 was a slightly unusual year where the credit transitioned from \$600 to \$1,000. Upon filing taxes, the credit was \$600, but then an additional \$400 lump sum was provided retroactively. In addition, as part of stimulus payments in 2008, eligible households received an additional \$300 on top of the \$1,000 credit.

[17] These families could claim the nonrefundable part of the CTC up to the amount of employee contributed Social Security and Medicare taxes less any EITC they received.

part of the CTC up to one-tenth of the excess of their earned income over \$10,000.[18] The CTC is phased out with adjusted gross income (AGI) at a 5 percent rate above \$110,000 for married couples filing a joint tax return.[19]

Four features of the CTC make it a good natural experiment for our analysis. First, to be eligible for the credit, the dependent child must not have reached 17 years of age by December 31 of the tax year. Because the timing of a child's 17th birthday is perfectly predictable, so should be the associated net income loss. Second, over the period we consider, virtually any household with AGI between \$30,000 and \$100,000 can take advantage of the full \$1,000 of the CTC because of the ACTC. As a result, the loss of the CTC constitutes a pure lump-sum change in both tax liability and after-tax income, conditional upon claiming the dependent, for households in this income range. Third, it is difficult to plan the exact timing of birth. Thus, among families whose children turn 17 just before the end of year t or at the very beginning of year $t + 1$, eligibility for the CTC is virtually exogenous.[20] Fourth, there is no other feature of the tax code that changes upon turning 17. Hence, focusing on households whose children turn 17 around the turn of a year, losing the CTC generates predictable, lump-sum, and exogenous variation in tax liability that is uncorrelated with other changes in the income tax schedule.

Because the impact of the EITC, another tax incentive targeted to low-income households, on labor supply has been extensively studied (see, for example, Eissa and Hoynes 2006 for a survey), it is useful to note where these two provisions differ. Like the EITC, the CTC is available for eligible children, but is available on a per-child basis. Both tax credits have a phase-in, plateau, and phase-out region, but unlike the EITC, the plateau range for the CTC is very large.[21] In comparison, in 2004, the first year of our study, the EITC reaches its plateau value of \$4,300 at \$11,000 of earned income for married couples filing jointly and begins to phase out after \$15,000. By 2011, the last year of our study, this plateau value increases to \$5,112 and its range spans from \$13,000 to \$21,000, after which phase out begins. The EITC is fully phased out for married couples in 2004 by \$35,000 and in 2011 by \$45,000 of earned income. Lastly, the age-discontinuity for EITC-eligibility is not sharp like CTC: a dependent child is generally eligible for the EITC until age 19, and through age 24 if he or she is a full-time student.

The loss of the \$1,000 CTC per child is at least as large as other recently analyzed tax-related income shocks. In what follows, we focus our attention on married couples filling a joint return. Shapiro and Slemrod (1995) analyze a change in income tax withholding instituted in 1992 and accounting for \$691 per year.

[18] The \$10,000 threshold has been indexed to inflation over time. In addition, starting in 2004, the ACTC limit was increased to 15 percent of earned income in excess of the threshold. Families with three or more eligible children could still claim the nonrefundable part of the CTC up to the amount of employee contributed Social Security and Medicare taxes less any EITC they received if this limit had turned out to be higher.

[19] The thresholds are \$75,000 and \$55,000 for single/head of household taxpayers and married taxpayers filing separately, respectively. None of these thresholds are indexed for inflation.

[20] One concern is that families may manipulate the timing of births around the end of the calendar year for tax purposes. Dickert-Conlin and Chandra (1999) provide evidence that shifting of births from January to December is (at least partly) motivated by tax reasons. In recent analyses, LaLumia, Sallee, and Turner (2015) use administrative US tax return data and find that the size of such birth timing manipulation is quite small. Schulkind and Shapiro (2014), using publicly available birth data from the US Vital Statistics, similarly find a relatively small amount of shifting of C-section births from January to December for tax reasons. We return to this concern in Section IVC.

[21] We provide a graph of the CTC schedule in Figure A1 in the Appendix.

Using Consumer Expenditure Survey data, Souleles (1999) analyzes the impact of tax refunds averaging \$874. Shapiro and Slemrod (2003) and Johnson, Parker, and Souleles (2006) analyze the income tax rebates of 2001 amounting to \$600. Shapiro and Slemrod (2009); Parker et al. (2013); and Broda and Parker (2014) analyze analogous rebates of 2008 that ranged between \$600 and \$1,200. Accounting for inflation would make the loss of the CTC and these rebates fairly comparable. When households in our sample are still CTC-eligible, the average refund amount is nearly \$2,800. This would make the CTC worth over one-third of the average refund. The loss of the EITC, as compared to the CTC, is less straightforward as the EITC applies to a different population than the one studied here and is more highly dependent upon marital status and number of children. Nonetheless, for a married couple earning the maximum amount under the EITC, the impact of a child aging out of EITC-eligibility (say, going from two children to one) is, in our most generous calculation, slightly less than twice as large as the impact of losing the CTC during the 2004–2011 time frame.

To test whether households are ex post confused by the loss of the CTC, we examine the effect of realizing this loss on household labor supply choices. The age-eligibility rule for the CTC creates a discontinuity in CTC-eligibility as a function of a child's date of birth that allows us to use a regression discontinuity (RD) methodology. We define the running variable, d, to be the number of days after January 1 of year $t + 1$. A household is assigned to the treatment group when $d < 0$, that is

$$T_i = \begin{cases} 1 \text{ if child turns 17 in year } t \\ 0 \text{ if child turns 17 in year } t + 1. \end{cases} \tag{2}$$

Treated households have a child that becomes ineligible for the CTC in year t. If unaware that their child will lose CTC-eligibility, these households are unlikely to notice the CTC loss until the early months of year $t + 1$ when they file their tax returns. Control households have a child who retains CTC-eligibility in year t and instead becomes ineligible in year $t + 1$. These control households are similarly unlikely to notice the credit loss until year $t + 2$ if they are unaware of the CTC-eligibility rules. Our identification strategy thus relies on the comparability of households who just lose the credit at the end of year t to households who just retain the credit for that year.

The baseline equation that we estimate is given by

$$\ln Y_{i,t+1} = \beta T_{i,t} + \pi' \mathbf{X}_{i,t} + f(d_{i,t+1}) + \gamma_{t+1} + u_{i,t+1}, \tag{3}$$

where Y is household wage income, $\mathbf{X}$ is a vector of household characteristics in year t, and γ_{t+1} are year fixed effects. The function $f(\cdot)$ is a polynomial expansion of the running variable, and we allow the parameters of $f(\cdot)$ to vary on either side of the discontinuity. In the most basic specification, we do not control for $\mathbf{X}$. In our preferred specification, the vector $\mathbf{X}$ contains the age of the primary filer (the level and its square), the number of children in the household (the level and its square), state fixed effects, and $\ln Y_{it}$ to reduce the residual variation of our dependent variable. All standard errors are clustered at the household level.

We use a one-month bandwidth on either side of the discontinuity. Specifically, treated households have a child who turns 17 in December of year t, and control households have a child who turns 17 in January of year $t+1$. We estimate this model on a balanced panel of households with some wage income in each period $t-1$ through $t+1$. Our specification identifies an intent to treat parameter, rather than the treatment effect on the treated. In practice, there is very little difference between these two parameters, as take-up of the CTC is extremely high. For example, over 97 percent of households in our control group report some CTC in year t, the last year in which they are eligible to claim the credit for the child that we consider.

Under the null hypothesis that households ex post understand their tax schedule, there is a nonnegative income effect on parental labor supply. That is $\beta \geq 0$.[22] The alternative hypothesis is that households are ex post confused. If the income effect dominates, then $\beta \geq 0$ and we are unable to distinguish between the null and alternative hypotheses. However, if $\beta < 0$, it must be the case that, on average, households are ex post confused and that the substitution effect dominates the income effect.

III. Data

Our data come from administrative, population-level US federal tax filings. We first identify all individuals who turn 17 between 2004 and 2011 in either December or January.[23] We match these children to the primary filer who claims the child as a dependent on his tax return, requiring that the same primary filer claims a child across years to avoid changes in income resulting from changes to household composition.[24] We collect income variables for the relevant tax years, along with the limited demographic information available from tax returns (i.e., the number of children, age of the primary filer, and filing status).

We make several additional sample restrictions. First, we restrict our analysis to married couples filing joint returns so we do not conflate changes to wage and salary income due to the change in the CTC with those due to changes in marital status.[25] Second, we exclude households where the primary filer is less than 35 years old (which would put the primary filer under 18 years of age at the time of birth of the child) or older than 60 to remove the potential influence of completing education or

[22] Perhaps a more natural way to formulate the null hypothesis is that taxpayers are *fully informed*. Then, there would be two alternative hypotheses to test: (i) that taxpayers are ex ante uninformed; and (ii) that taxpayers are both ex ante uninformed and ex post confused about the source of the unanticipated change in their tax liability. Our formulation of the null hypothesis is driven by the observation that, in the presence of liquidity constraints, ex post informed taxpayers are empirically indistinguishable from *fully informed* taxpayers.

[23] These individuals are identified using date of birth information. There are roughly 400,000 children born in each month over our sample period. We begin our analysis in 2004 to consider the period when the CTC is $1,000 per eligible child, rather than have household responses differ over time due to different credit amounts.

[24] Family relationships cannot be determined from tax return data, and so we consider these matched primary filers to be a parent. We drop the few instances where the dependent is claimed as a spouse. Note that children in treated households turn 17 and 18 in years t and $t+1$, whereas children in control households turn 16 and 17 in these years. We restrict our analysis to households who consistently claim these dependents in the years that they turn 16, 17, and 18 years old. There are several reasons why kids may not be claimed consistently over time. Some parents will become non-filers and there may be changes in who claims a child. Some fraction of kids die before age 18 and some may become primary filers.

[25] In general, IRS statistics suggest that over 95 percent of married households file jointly.

retirement decisions on labor supply choices. Third, we exclude households outside of the 50 states or Washington, DC. We also exclude households in states where it is likely that treatment and control children are in different academic grades.[26] We also exclude roughly 1 percent of households with more than one child turning 17 in a particular month-year. Finally, we focus on households who plausibly face a pure lump-sum tax liability change due to their child aging out of CTC eligibility. We accomplish this by excluding households with AGI less than \$30,000 and greater than \$100,000 in year $t - 1$ (i.e., the year prior to treated households losing CTC-eligibility) in order to remove households who likely fall in the phase-in or phase-out ranges of the CTC schedule (see Figure A1). All dollar values are adjusted to real 2008 dollars using the CPI, and our dependent variables are winsorized at the 5 percent and 95 percent levels to mitigate the influence of outliers.

Our final sample includes roughly 850,000 observations. We construct seven pairwise cohorts of treated households (i.e., those whose kids turn 17 in December of year t) and control households (i.e., those whose kids turn 17 in January of $t + 1$). Figure 1, which plots the density of households by day of birth of the child under consideration, shows that the distribution of households is fairly smooth, with expected dips in births around Christmas and New Year's.

Our identification strategy relies on the comparability of households with a child turning 17 at the end of one calendar year and households with a child turning 17 at the beginning of the next calendar year, except for the loss of the CTC. Table 1 shows that treated and control households appear very similar in both years t and $t + 1$ for variables that are not affected by the loss of the CTC. Treated and control households are balanced on observable demographic characteristics (i.e., number of children, age of the primary filer), however due to our large sample sizes, each is statistically different at the 1 percent level. In Figure A2 in the Appendix, we provide complementary graphical evidence that the covariates included in our preferred specification are smooth through the discontinuity in year t. We estimate equation (3) with each demographic characteristic as the dependent variable, excluding any of the control variables in $\mathbf{X}$. The estimated difference at the discontinuity for the number of children is statistically significant at the 5 percent level, but is very small at -0.018 (that is, less than 2/100th of a child). The estimated difference at the discontinuity is also quite small for the age of the primary filer, -0.047, and is not statistically different from zero.

Importantly, the variables in Table 1 with meaningful differences between treated and control households in year t can be explained by the loss of the CTC. Treated households have \$923 less in CTC amounts on average in year t, the year that their child becomes ineligible for the credit. This treatment is reflected in average tax liabilities: tax liabilities for the treated group are roughly \$760 higher than those in the control group in year t, but this difference practically disappears the following year.[27] The difference in tax liabilities does not appear to be driven by a difference

[26] We exclude households from five states that had a December 31 school entry cutoff date in the year in which the child was born: Delaware, Hawaii, Louisiana, Maryland, and Rhode Island. In these states, there is likely a one academic year difference between the treatment and control groups. Note, however, that, due to data availability, we base this exclusion on the state in which the household filed, which is not necessarily the state in which the child lived when he or she first entered school.

[27] Tax liabilities are computed as the total tax owed, less EITC and ACTC payments.

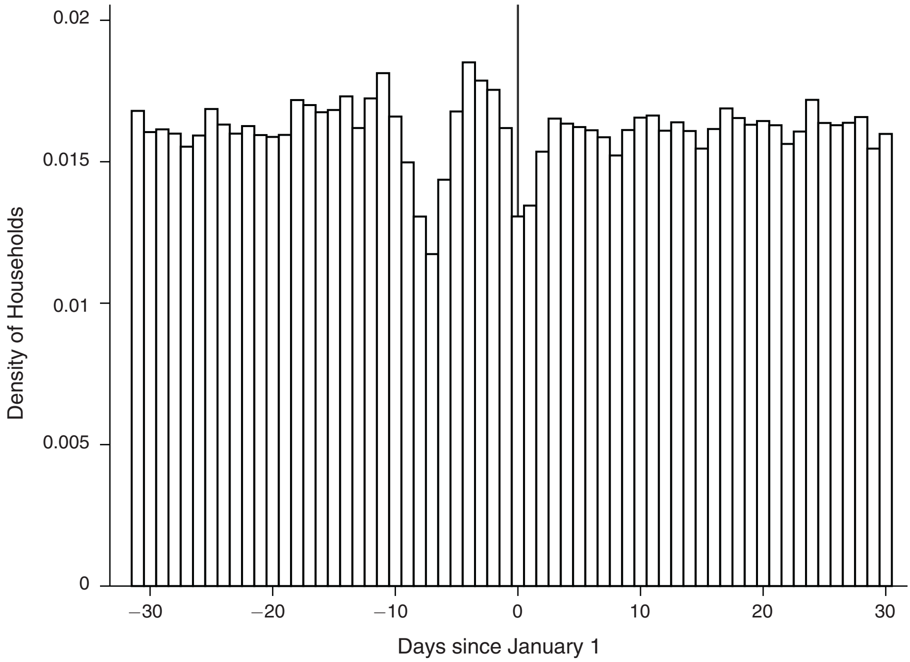

FIGURE 1. DENSITY OF HOUSEHOLDS, BY DAY OF BIRTH

Notes: The figure plots the density of households in our treatment and control groups, by day of birth. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

TABLE 1—SUMMARY STATISTICS

	Year of treatment (year t)		Year after treatment (year $t+1$)	
	December birth ($T=1$)	January birth ($T=0$)	December birth ($T=1$)	January birth ($T=0$)
Age of primary filer	46.4	46.5	47.4	47.5
	(5.59)	(5.53)	(5.59)	(5.53)
Number of children	2.26	2.25	2.18	2.18
	(1.06)	(1.06)	(1.06)	(1.05)
Child Tax Credit (CTC + ACTC)	873	1796	856	842
	(980)	(1,010)	(983)	(980)
Tax liability	4,812	4,054	5,212	5,137
	(4,179)	(4,249)	(4,688)	(4,810)
EITC amount	129	117	168	159
	(568)	(539)	(663)	(647)
EITC > 0	1,737	1,699	1,884	1,882
	(1,239)	(1,230)	(1,306)	(1,302)
Proportion with a refund	0.79	0.87	0.79	0.79
	(0.40)	(0.33)	(0.40)	(0.41)
Refund amount	1,970	2,796	2,064	2,014
	(3,238)	(3,205)	(3,226)	(3,295)
Average tax rate	5.82	4.97	6.00	6.09
	(4.00)	(3.97)	(4.08)	(4.08)
Observations	422,817	429,073	422,817	429,073

Notes: This table provides means and standard deviations of several demographic characteristics and income measures available in administrative tax data. Treatment households are those with a child who turns 17 in December of year t; control households are those with a child who turns 17 in January of year $t+1$.

in EITC amounts. There is also evidence that treated households likely would have noticed the loss of the CTC in the early months of year $t + 1$ when they filed their taxes: for tax year t, treated households are, on average, 8 percentage points less likely to receive a refund and have \$826 less in refunds than those in the control group.

IV. Results

A. *Main Result*

Before turning to our main results, Figure 2 depicts the quasi-experimental variation that we exploit. In this figure, the circles plot day-of-birth cell means of CTC amounts claimed on a tax return, and the solid lines plot fitted values from a regression of the dependent variable on a cubic polynomial of the running variable on each side of the discontinuity. As expected, CTC amounts are roughly the same in year $t - 1$, when children in both the treated and control groups are eligible for the CTC. In year t, there is clear evidence of a sharp discontinuity in the amount of CTC claimed, with treated households claiming roughly \$1,000 less in CTC than control households. In year $t + 1$, children in the control group have now also become ineligible for the CTC, and there is again no discernible difference in the CTC amounts claimed by the treated group and control group.[28]

Our main result is presented graphically in Figure 3, with corresponding parameter estimates from estimating equation (3) presented in Table 2. As before, the circles in Figure 3 plot day-of-birth cell means of log wages, and the solid lines plot fitted values from a regression of the dependent variable on a cubic polynomial of the running variable on each side of the discontinuity. First, consider results for year $t + 1$, presented in panel C of Figure 3. Consistent with ex post confusion, we see a discontinuous break in household wage income in this year, and wage income is lower for treated households relative to control households. This evidence suggests that treated households, on average, fail to anticipate the loss of the CTC in year t. They notice an increase in their tax liability in the early months of year $t + 1$, and reduce their labor supply in response to a (mis)perceived increase in their MTR.

Columns 1–4 of Table 2 provide corresponding estimates of the treatment effect that is depicted graphically. Moving across columns incrementally increases the covariates that are included in the vector $\mathbf{X}$: column 1 presents results with only year fixed effects; column 2 adds demographic characteristics to specification (1); column 3 adds lagged wage income to specification (1); and column 4 includes all available covariates. We consistently estimate a negative effect on wage and salary income upon observing the loss of the CTC, the magnitude of which is robust, though the statistical significance of our estimates is sensitive to the included covariates. In particular, lagged income appears to be important for reducing the residual variation of our outcome of interest. Taking column 4 as our preferred specification, treated households reduce their wage earnings by 0.5 percent on average in the year that

[28] Even if ineligible for the CTC, children can still be claimed as dependents for purposes of the EITC, dependent exemptions, and head of household status. The left panel of Figure A2 in the Appendix provides evidence that this first-stage variation is not due to a differential change in the number of children claimed as dependents between the treatment group and control group. That is, changes in CTC amounts are not driven by other children being disproportionately removed from treated households.

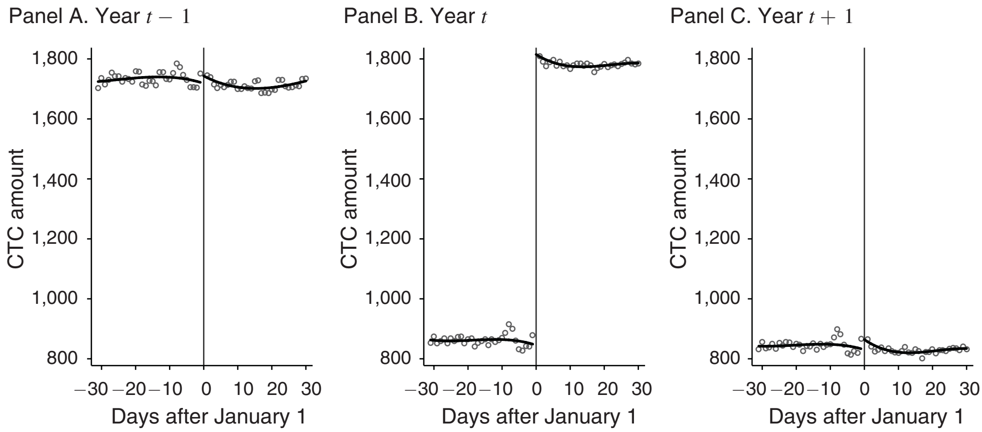

Figure 2. CTC Amounts Claimed, by Day of Birth

Notes: The figure presents the average amount of the CTC claimed by calendar day. Circles plot day-of-birth cell means of CTC amounts, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

Table 2—Impact of CTC Loss on Wage and Salary Income

	$\log(wage\ income)_{t+1}$				$\log(wage\ income)_t$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.006	−0.006	−0.005***	−0.005***	−0.001	−0.001	−0.001	−0.001
	(0.004)	(0.004)	(0.0020)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
Demographic characteristics?	No	Yes	No	Yes	No	Yes	No	Yes
Lagged dependent variable?	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.0058	0.0185	0.7576	0.7583	0.0113	0.0226	0.7465	0.7472

Notes: Demographic characteristics are age and age squared of the primary tax filer, total number of dependent children in the household and its squared term, and state fixed effects. All specifications include year fixed effects. Each regression includes 812,426 observations. A third order polynomial of the running variable is used. Robust standard errors, clustered by household, are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

they observe a $1,000 increase in their tax liability, relative to otherwise similar households who retained the CTC for an additional year. We provide some interpretations of the magnitude of this estimated effect in the next subsection.

We may be concerned that the estimated treatment effect reflects preexisting differences between treatment and control households. In Section III, we showed that these households look quite similar in their demographic characteristics, but there were dips in the density of households across the treatment discontinuity. To further address this concern, we show that our estimated treatment effect is not due to preexisting differences in wage income across treatment and control households. If the assumptions of our underlying model hold, then we should find no impact of the treatment of losing CTC-eligibility in the actual year in which the treatment group turns 17.

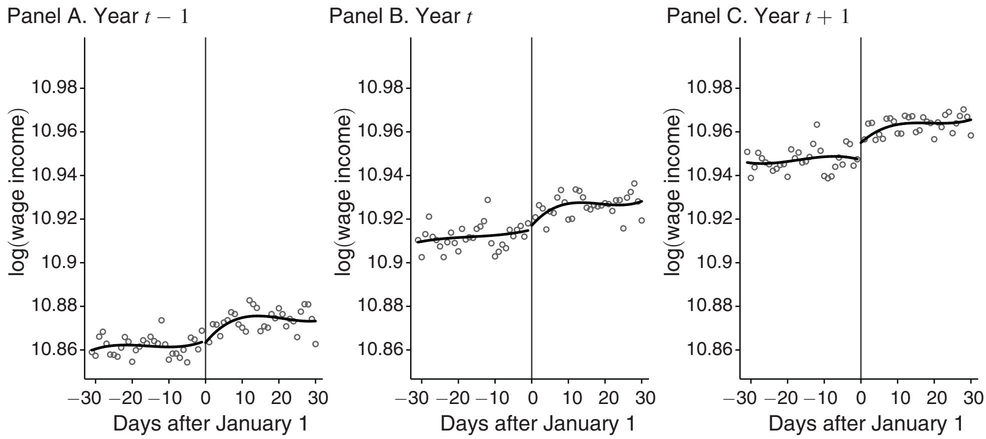

Figure 3. Average log Wage and Salary Income, by Day of Birth

Notes: The figure presents the average log wage and salary income by calendar day. Circles plot day-of-birth cell means of log wage income, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

Panel B of Figure 3 shows log wage income for treated and control households in year t. The difference between treated and control households is much smaller than in year $t+1$. Regression results from estimating equation (3) for year t, shown in columns 4–8 of Table 2, show that the difference in log wages at the discontinuity is small in magnitude and not statistically significant at conventional levels. Thus, we cannot reject the null hypothesis that the treatment and control groups are similar in year t. As further validation of our estimation strategy, panel A of Figure 3 shows average log wage income for treatment and control households in year $t-1$, the year before a child in the treated household turns 17. Under the assumptions of our model, these households should be similar, on average, even in terms of claiming the CTC. There is no discernible difference in log wage income between treatment and control households at the discontinuity. In regressions not shown (available upon request), we find that the estimated "treatment" effect from this placebo test is 0.001, with a standard error of 0.004.[29]

In sum, we reject the null hypothesis that households, on average, ex post understand the loss of their child's CTC-eligibility in year t. Our results support the alternative hypothesis that households instead misinterpret the lump-sum loss of the CTC as at least partially due to an increase in their MTR.

B. *Implied Elasticities*

The estimated treatment effects indicate that, upon losing the CTC, some households interpret their tax liability increase as at least partially due to an increase in their MTR. A natural next question is whether the magnitudes of the estimated effects are plausible. However, interpreting these magnitudes requires additional

[29] We estimate these regressions for the simple model with only year fixed effects, and for the model including demographic characteristics.

assumptions on the perceived change in MTR to which households react. In this subsection, we consider a number of scenarios that might describe a household's misinterpretation of the CTC loss and compute the implied elasticity based on the perceived change in MTR and observed change in income. Overall, we find that under a broad range of assumptions, the implied elasticities fall within the range of elasticities in the literature that are estimated based off of real changes in MTRs.

First, we consider the special case when households disregard the demogrant when thinking about the tax schedule. Under this scenario, households "iron" out the tax schedule and confuse their ATR with their MTR (Liebman and Zeckhauser 2004). Our estimated treatment effect would then reflect responses to the change in ATRs due to the loss of the CTC. To compute the implied elasticity under "ironing," we calculate the change in the log of the net-of-ATR that treated households face when they lose the CTC.[30] We calculate the uncompensated elasticities based on the percent change in the net-of-average tax rate and the estimated treatment effect. The implied elasticity of wage earnings is 0.30. If we consider our wage income elasticity as a proxy for the household labor supply elasticity, our estimate is comparable to previous studies. For example, the elasticity for prime-age single workers ranges roughly between 0.1 and 0.3 (see, for example, Altonji 1986; McClelland and Mok 2012; and Peterman forthcoming), whereas the elasticity for married women is around 0.8 (Eissa 1995).[31]

Next, we interpret our estimated treatment effect in our more general model of household confusion. As discussed in Section I, when households experience a change in tax liability, they may attribute some fraction α as being due to a change in the MTR and $1 - \alpha$ as being due to a change in the demogrant. Without further assumptions, we are unable to pin down α. Moreover, calculating the log change in net-of-MTR necessitates taking a stand on base MTRs that households believe they are facing. This is particularly challenging within the context of taxpayer confusion.

To remain agnostic on the degree of baseline knowledge of the tax system and the degree of ex post confusion, we present a range of possibilities in Figure 4. The figure shows four lines that differ by the baseline MTR that the household may believe it faces. The horizontal axis measures the amount of the \$1,000 tax liability increase that is attributed to a change in MTR. The far right value of \$1,000 represents a household that believes that all of the increase is due to a change in the MTR (as assumed under "ironing") and \$0 represents a household that attributes the entire tax liability increase to a change in the demogrant (a fully informed or ex post informed household). The lines thus depict the implied elasticity based upon the perceived change in the MTR evaluated at the average sample wage income of \$63,000 and our estimated treatment effect. For example, if a household believes that it faced a 30 percent MTR in year t and attributes \$500 of the increase in tax liability it realizes in year $t + 1$ as due to an increase in its MTR, the implied wage elasticity is roughly 0.44.

[30] We first compute the ATR for treated households as the ratio of tax liability and AGI in year $t - 1$, the year before they lose the credit. We then compute the ATR implied after the \$1,000 loss based on information in year $t - 1$. This is the ATR that treated households would face upon losing the CTC prior to any other behavioral response.

[31] In one sense, it is difficult to compare our elasticity calculation to those in the previous literature because we work from the assumption that the changes in marginal tax rates that were used to generate the elasticity estimates were perfectly understood. If not, then those elasticities may be miscalculated as well.

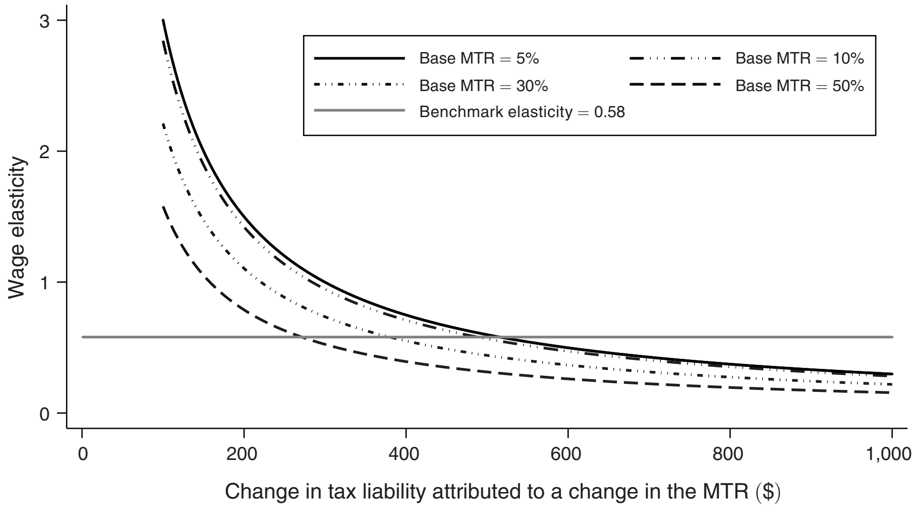

Figure 4. Wage and Salary Income Elasticity

Notes: The figure presents the wage and salary income elasticities that result from varying the amount of the increase in tax liability (T) that is due to a perceived increase in the MTR where $T = -D + \tau Y$. \$0 reflects that none of the change is due to the MTR whereas \$1,000 reflects that all of the change is due to the MTR. Each of the lines represents a different assumption on baseline household knowledge regarding their MTR. For example, households that believe they face a 5 percent MTR before realizing the increase in tax liability are represented by the solid black line.

There are a number of major points to take away from this figure. First, as the weight placed on the MTR (α) increases, the perceived baseline MTR is of less importance. In the extreme, if we assume that all households attribute the entire \$1,000 tax liability increase to an increase in the MTR, then the perceived baseline MTR does not matter much. Whether households assume that the baseline MTR is 5 percent or 50 percent, the difference in the implied elasticity between these two extremes is approximately 0.15. In other words, as more of the CTC loss is attributed to a change in the MTR, household confusion over complex tax liability changes overrides knowledge of its true baseline MTR. Second, if α is low, say, 0.1, where only \$100 of the \$1,000 tax liability increase is attributed to an increase in the MTR on average, we obtain elasticities that are, in most cases, not well supported in the current literature. In the case where $\alpha = 0.1$, we obtain wage elasticities that are near or above 2. This suggests that this relatively low level of confusion is not empirically supported given the generally accepted range of estimated elasticities and implies that some other higher level of confusion is more appropriate. Finally, abstracting from income effects, if we take the Hicksian aggregate hours elasticity from Chetty (2012) of 0.58 as the "true" elasticity, our calculations imply that households, on average, attribute between one-quarter and one-half of the tax liability increase as due to an increase in their MTR.[32] Or, an alternative interpretation is to consider our sample as comprised of "ironers" and otherwise

[32] If we assume a 50 percent base MTR, an elasticity of 0.58 implies that about \$250 out of the \$1,000 is attributed to a change in the MTR. Analogously, assuming a base MTR of 5 percent implies that about \$500 is attributed to a change in the MTR.

fully informed households. Our results under this interpretation imply that roughly between one-quarter and one-half of all households are confused as to the source of tax liability changes that result from losing the CTC.

C. *Robustness Tests*

In Table 3, we present estimates from a number of robustness tests to our baseline model in Section IVA. The objective of these tests is to examine the extent to which our baseline results may be driven by a spurious correlation between the treatment status and underlying unobservable characteristics that determine income.

One concern is that parents may manipulate the timing of a child's birth for tax reasons. Similarly, parents or doctors may try to minimize the number of births during holidays such as Christmas or New Year's. Although recent empirical evidence suggests that there is little birth timing manipulation for tax purposes in the United States (see footnote 20), Figure 1 reveals that there are dips in the density of households whose children are born around Christmas and New Year's. Importantly, such potential manipulation presents a challenge to our identification strategy only if it is systematically correlated with wage income. The strongest counter to this concern is that, as previously shown, wage income is essentially smooth through the discontinuity in years prior to the loss of the CTC. In addition, Figure A2 in the Appendix shows the same is true also of the two demographic variables that we do observe, namely the number of dependent children and the age of the primary filer. Nevertheless, we conduct several additional robustness tests to examine whether our results might be driven by such manipulation.

First, we nest the RD framework within a difference-in-differences (DID) specification. The DID estimator controls for *any* time-invariant unobserved heterogeneity in income, hence solving the problem with a potential correlation between income and birth shifting. We provide DID estimates for year $t + 1$ and year t in columns 1 and 2, respectively. In column 1, we recover the estimated treatment effect from our level specifications in Table 2. Moreover, in column 2, we recover a placebo estimate that is numerically and statistically equivalent to our level-based placebo estimates in Table 2. Therefore, spurious correlation between the treatment status and unobserved, time-invariant heterogeneity in wage income, such as a correlation of birth shifting with income, does not appear to drive our baseline results.

Next, to address the potential holiday-driven birth date shifting, we reestimate our baseline specification excluding specific dates. In column 3, we drop households whose children are born during Christmas and New Year's from the estimation sample. In column 4, we additionally drop the day before and the day after these holidays.[33] In both specifications, the size of the estimated treatment effect is, if anything, larger in absolute value than our baseline estimate and remains statistically significant.

Another potential source of spurious correlation is that there may be a direct effect of child aging on parental labor supply. Households in the treatment group have, on average, slightly older children than households in the control group. If

[33] In results not shown, but available upon request, we repeat this analysis for year t. The estimated placebo effect remains small at -0.001 and is not statistically significant.

Table 3—Robustness Tests

	DID, year $t+1$ (1)	DID, year t (2)	Exclude 12/25, 1/1 (3)	Column [3], +/− 1 day (4)	Monthly placebo (5)
T	−0.005***	−0.001	−0.006***	−0.007**	−0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Observations	812,426	812,426	792,422	743,838	1,000,776
R^2	0.055	0.047	0.758	0.758	0.773

Notes: All specifications include a third order polynomial of the running variable. Control variables (estimates not displayed) are age and age squared of the primary tax filer, total number of dependents in the household and its squared term, and state fixed effects; columns 3–5 also include the lagged dependent variable as an additional control. Columns 1 and 2 provide difference-in-differences estimates for years $t+1$ and t, respectively; column 3 excludes December 25 and January 1 from the estimation sample; column 4 excludes December 25, January 1, and the day preceding and following those days; column 5 is a placebo test using matched month pairs within the same year (e.g., January (treated) versus February (control), March (treated) versus April (control), etc.). Robust standard errors, clustered by household, are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

parental wage and salary income depends on the age of their children, then our estimated treatment effect may confound the effect of losing the CTC with a direct effect of the child's age. We test for a direct effect of child aging by comparing parental wage and salary income for households with children turning 17 in consecutive birth months within a tax year.[34] For example, we compare January (treated) and February (control) births within the same year. Households in the "treatment" and "control" groups have children with similar differences in age as our baseline specification, but there should be no difference in the tax consequences between the two groups because they all turn 17 within the same year. We run this placebo test on six cohorts within the year, where each cohort is a set of adjoining months (January–February, March–April, etc.). The results, reported in column 5, show no statistical or economic difference between "treatment" and "control" groups. Thus, our baseline results do not appear to be driven by a direct effect of child aging on parental labor supply either.

D. *Alternative Dependent Variables*

We examine two alternative dependent variables that differ in their tax reporting requirements and in their available margins for response: (i) a measure of labor income that includes self-employment earnings; and (ii) a measure of broad income. Evaluating changes to these alternative forms of income may provide insight into the extent to which households adjust their behavior in response to a perceived change in MTRs.

We construct a broader "labor income" measure that is the sum of wage and salary income and self-employment income (reported on Schedule C). Relative to

[34] Because of the intensity of the data collection, for this exercise, we collect data only for all individuals who turned 17 in 2006 (chosen randomly).

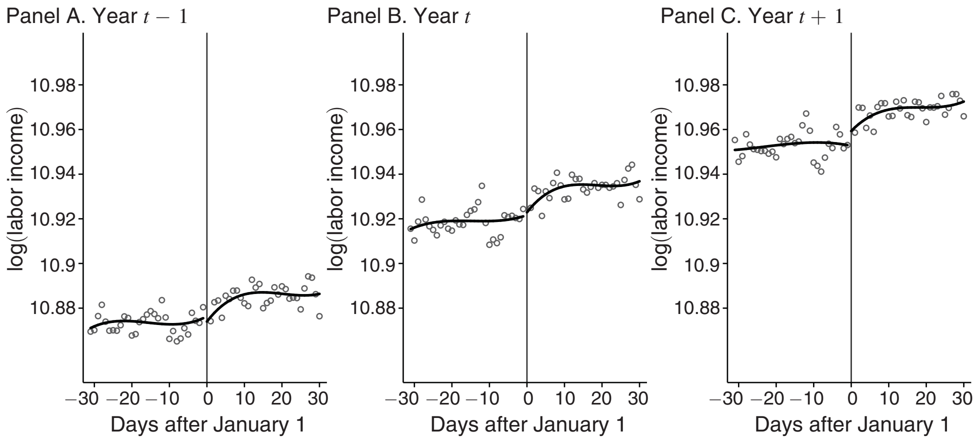

FIGURE 5. AVERAGE LOG LABOR INCOME, BY DAY OF BIRTH

Notes: The figure presents the average log labor income by calendar day. Circles plot day-of-birth cell means of log labor income, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

wage and salary income, which is verified by W-2s filed by employers with the IRS, reported self-employment income is easier to manipulate (Slemrod 2007). Thus, changes in this broader labor income measure may be influenced by the income reporting decisions of the household and we might expect an even larger treatment effect when we include it in our labor income measure.[35] Results for labor income are presented graphically in Figure 5, with corresponding regression results presented in panel A of Table 4.[36] In panel C of Figure 5, we observe a discrete jump in labor income in year $t + 1$. The magnitude of the estimated treatment effect, shown in columns 1–4 of the top panel of Table 4 is very consistent with our wage income results, and provides further support for the ex post confusion hypothesis. The analysis of labor income in years t and $t - 1$ provides additional support that treatment and control households are similar in their labor supply outcomes prior to the treatment year. While there is a small difference in labor supply between treatment and control households in year t, the estimated treatment effects are relatively small, at a 0.2 percent reduction in labor income, and are never statistically significant. In results not shown, the estimated treatment effect in year $t - 1$ is quite small, at 0.002, and not statistically significant at conventional levels. Overall, the estimated treatment effect in year $t + 1$ of the CTC loss on labor income are very consistent with those on wage and salary income alone.

Broad income is typically defined as gross income (i.e., AGI) excluding capital gains, and differs from taxable income largely because it excludes itemized deductions. Under certain conditions, the responses of broad income to taxes is

[35] Self-employment income alone may be an interesting dependent variable. In unreported specifications, we examine self-employment income and find no evidence of a statistically significant treatment effect as self-employment income tends to be much more volatile than wage and salary income.

[36] Note that self-employment income can be negative, so labor income can be less than wage and salary income.

Table 4—Alternative Dependent Variables

	log($income_{t+1}$)				log($income_t$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Labor income								
T	−0.005	−0.005	−0.005**	−0.005**	−0.0000	−0.0004	−0.002	−0.002
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
R^2	0.005	0.021	0.760	0.761	0.010	0.025	0.744	0.745
Panel B. Modified broad income								
T	−0.006	−0.006	−0.006*	−0.006*	−0.002	−0.003	−0.004	−0.004
	(0.005)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
R^2	0.006	0.022	0.479	0.483	0.009	0.024	0.437	0.442
Demographic characteristics?	No	Yes	No	Yes	No	Yes	No	Yes
Lagged dependent variable?	No	No	Yes	Yes	No	No	Yes	Yes

Notes: Demographic characteristics are age and age squared of the primary tax filer, total number of dependent children in the household and its squared term, and state fixed effects. All specifications include year fixed effects. Each regression in panel A includes 834,414 observations. Each regression in panel B includes 840,136 observations. A third order polynomial of the running variable is used. Robust standard errors, clustered by household, are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

what matters for welfare analysis (Chetty 2009). Behavioral adjustments to broad income may come in many forms, such as labor supply (or effort), the timing of income, savings decisions, or tax evasion and avoidance decisions. In the Appendix, Figure A3 plots average log broad income between treatment and control groups in years $t-1$, t, and $t+1$. Broad income is quite similar across treatment and control households in pre-treatment years; in year $t+1$, however, we see little evidence of a treatment effect.[37] We explore several components of broad income to better understand this nonresponse and find that it is driven by two sources: income from retirement distributions and unemployment insurance compensation (UI). Figure A4 in the Appendix plots the sum of these two income components. While there is little evidence of a difference between treatment and control households in years t and $t-1$, there is evidence of a sharp increase in retirement distributions and UI in year $t+1$.[38] The changes in both of these income sources could plausibly represent other forms of a labor supply decrease that is predicted by our model of tax confusion, either through retirement or job separation. In addition, retirement distributions have been found to be an income source used for consumption smoothing following negative income shocks.[39]

We examine an adjusted version of broad income that excludes income from retirement distributions and unemployment compensation. Results from these

[37] In regressions not shown, we find that the estimated treatment effect is small, at roughly −0.002, and not statistically significant.

[38] In regressions not reported, we find no evidence of a change in this composite income measure in years $t-1$ and t, but a 6 percent increase in this income measure in year $t+1$ that is statistically significant at the 5 percent level. In year $t-1$, the estimated treatment effect is −0.017 and in year t the estimated treatment effect is −0.0002.

[39] For example, Kawano and LaLumia (forthcoming) find increases in retirement distributions during unemployment spells, and Deryugina, Kawano, and Levitt (2014) document increases in retirement distributions among those impacted by Hurricane Katrina.

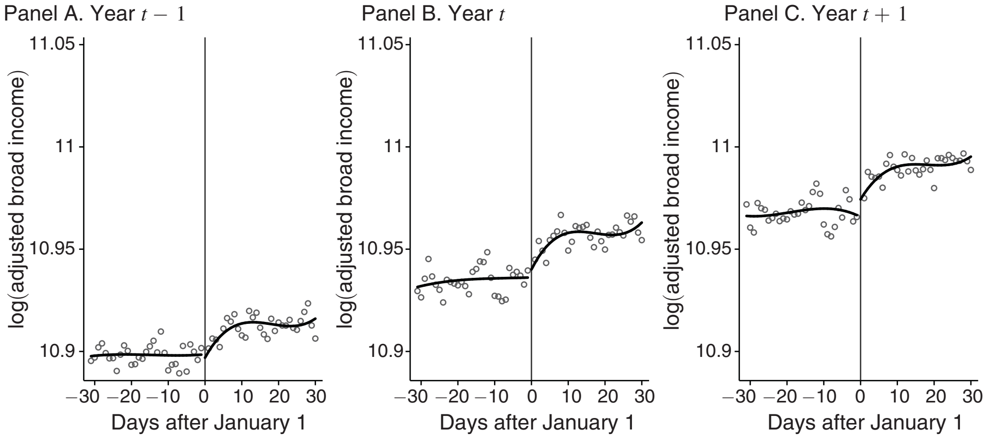

FIGURE 6. AVERAGE ADJUSTED BROAD INCOME, BY DAY OF BIRTH

Notes: The figure presents the average log adjusted broad income by calendar day. Circles plot day-of-birth cell means of log adjusted broad income, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

specifications are presented in Figure 6 and panel B of Table 4. As expected, panels A and B of Figure 6 show little visual difference in modified broad income between treatment and control households in year $t - 1$ or year t. Columns 5–8 of the bottom panel of Table 4 confirm that the small gap in year t is not statistically significant. In panel C of Figure 6, there is now a distinct jump at the CTC-eligibility discontinuity. Columns 1–4 of the bottom panel of Table 4 provide a robust treatment effect, a 0.6 percent decline in modified broad income, that is statistically significant at the 10 percent level when lagged income is included as a covariate.

In sum, while both labor income and broad income offer more potential margins of response, we find that the estimated impact of losing the CTC is fairly consistent across these alternative earnings variables. Thus, our estimated treatment effect likely reflects real labor supply responses, rather than a retiming of income or tax avoidance or evasions responses.

E. *Heterogeneity*

We first estimate equation (3) separately for three income groups: \$30–55K, \$55–80K, and \$80–100K.[40] We present the estimated treatment effects across the income distribution in Figure 7. The vertical lines depict 95 percent confidence intervals around the estimated treatment effects. The treatment effect is strongest for the lowest income category, and it becomes smaller in absolute value with income. The estimated effect is -0.010 for the lowest income group. The estimated effect for the middle income group is roughly equal to the average treatment effect, -0.005, and is statistically significant at the 10 percent level. For the highest income group,

[40] Income groups are defined by AIG in year $t - 1$.

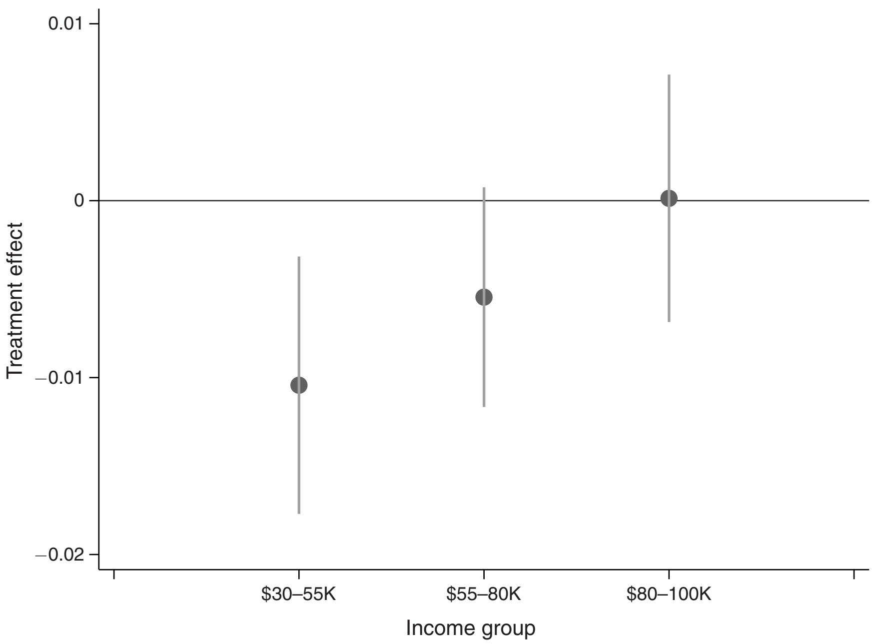

Figure 7. Heterogeneity in Treatment Effect, by Income Group

Notes: The figure plots estimated treatment effects and 95 percent confidence intervals for different income ranges: \$30–55K, \$55–80K, and \$80–100K. These income groups are defined using AGI in year $t - 1$.

the estimated treatment effect is close to zero, 0.0001, and is not statistically significantly different from zero at conventional levels.

We compute the wage income elasticities implied under "ironing" for each of these income groups, evaluated at the mean change in the log net-of-ATR in each income range. The implied elasticities are 0.42 for the lowest income group and 0.34 for the middle income group. The implied elasticity for the highest income group is nearly zero, at −0.01. This heterogeneity could reflect several factors. First, because the CTC is a fixed amount per child, the effect of the credit loss as a percentage of income decreases in income and, as a result, has a less noticeable impact for higher income households. Second, lower income households may be more able to adjust their wage and salary income if they are paid hourly or have some part-time work. Third, this heterogeneity in responses may also reflect that the highest income households in our income range have a better understanding of the CTC eligibility rules and their effect on MTRs.

Next, we divide our sample based upon three different variables: (i) households where the primary filer is below or above the median age of our sample; (ii) households with and without self-employment income; and (iii) households with and without capital gains realizations. Older households and households with self-employment or realized capital gains income are likely to be more financially sophisticated than their counterparts, and so they may also have a better understanding of the tax system. Figure 8 presents the estimated treatment effect for each of these subsamples along with 95 percent confidence intervals. In the leftmost panel, we find that younger households are more likely to be ex post confused compared

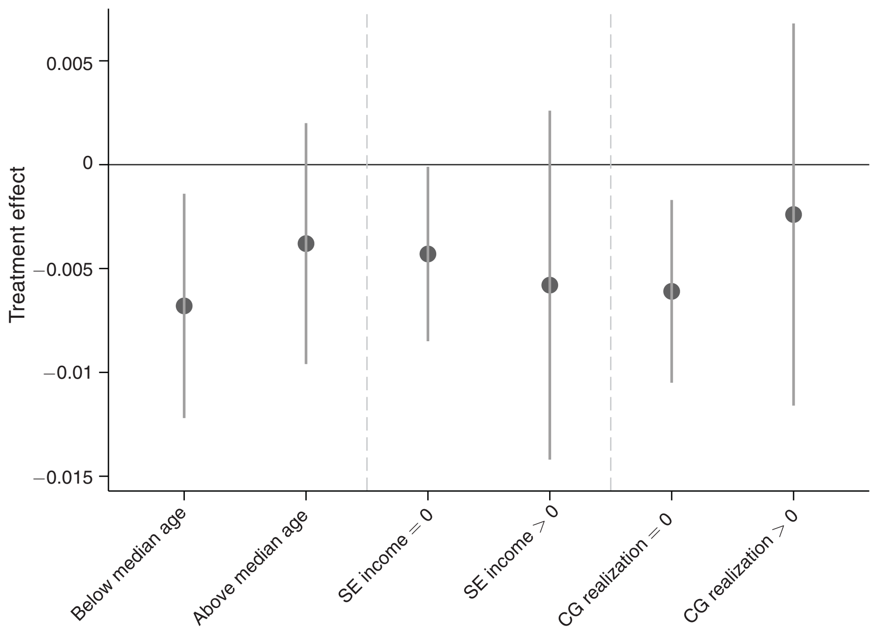

Figure 8. Heterogeneity in Treatment Effect, by Age, Self-Employment, and Capital Gains Realizations

Notes: The figure plots estimated treatment effects and 95 percent confidence intervals across different subpopulations: (i) those below and above the median primary filer age; (ii) those without and with self-employment income; and (iii) those without and with capital gains realizations.

to older households. The point estimate for younger households is equal to −0.007 and is statistically significant at the 5 percent level, whereas that for older households is smaller (in absolute value) and not statistically different from zero. In the middle panel and the rightmost panel, we find statistically significant evidence that households without self-employment or capital gains realizations are also likely to exhibit ex post confusion upon losing the CTC when compared to those households with self-employment income or capital gains realizations, respectively.

Interestingly, these results suggest that experience and perhaps exposure to more complicated aspects of the income tax system—as proxied by older or higher income households or those with more complex tax situations—are less likely to be ex post confused. As such, there appears nothing particularly hard-wired in household behavior that would imply that confusion cannot be overcome by learning and interacting with the income tax system over time.

V. Conclusion

The complexity of the income tax system can make it difficult for taxpayers to understand the tax schedule that they face. We present a model in which households may misperceive the nature of tax policy changes that they experience: fully informed households anticipate predictable tax schedule changes; ex post informed households fail to anticipate tax schedule changes but correctly perceive them once they are experienced; and ex post confused households both fail to anticipate tax schedule changes and remain confused about the schedule ex post. This latter case generally leads to ex post misperceptions over changes in the MTR the household faces. We examine the misperception hypothesis by measuring taxpayer wage and salary income responses to a source of exogenous, lump-sum and predictable variation in tax liability due to losing eligibility for the Child Tax Credit when a child turns 17.

We find that, on average, households who lose the credit due to having their child turn 17 at the end of a calendar year report about 0.5 percent lower household wage and salary income in the subsequent year compared to households that have their child turn 17 at the beginning of the following calendar year. This result is inconsistent with all households being fully or ex post informed about predictable changes in their tax schedule. We argue that at least some households misinterpret the increase in their tax bill as (at least partly) due to an increase in their MTR, leading to a reduction in labor income due to the conventional substitution effect. This finding is robust to a variety of tests that include placebo effects at various other age and calendar cutoffs.

Our results suggest that tax policy changes that are not well-understood by the affected population may have unintended behavioral and welfare consequences. In particular, changes that affect the level but not the slope of the tax schedule may result in unanticipated (by policymakers) substitution effects, hence increasing or reducing the deadweight loss relative to the full-information case. On the other hand, changes that mostly affect the MTR may be partly interpreted as changes in the level of the tax schedule, with analogous implications for deadweight loss. The complexity of the tax system may therefore interact with tax changes to create departures from conventionally understood welfare effects.

Appendix

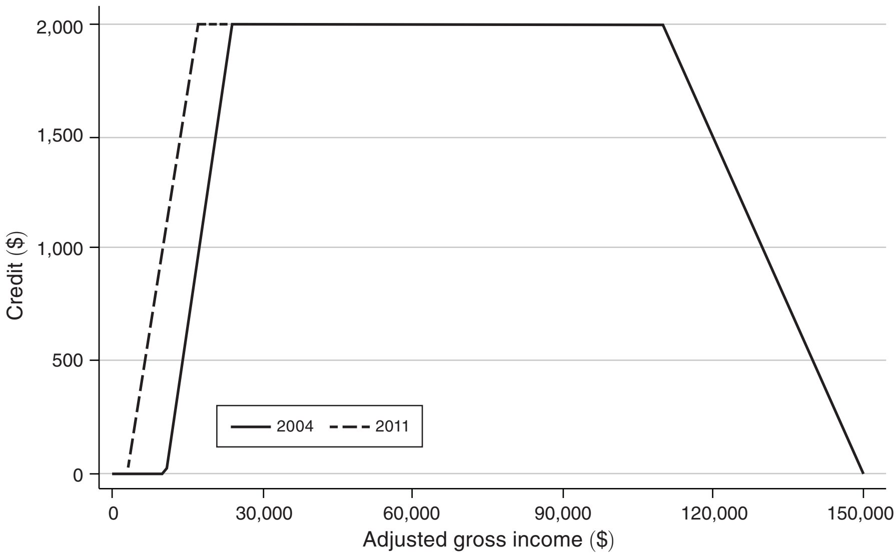

Figure A1. CTC (+ACTC) for Two Qualifying Children

Notes: The figure presents the CTC (including the ACTC) schedules for 2004 and 2011. These schedules are based off of calculations using TAXSIM (Feenberg and Coutts 1993).

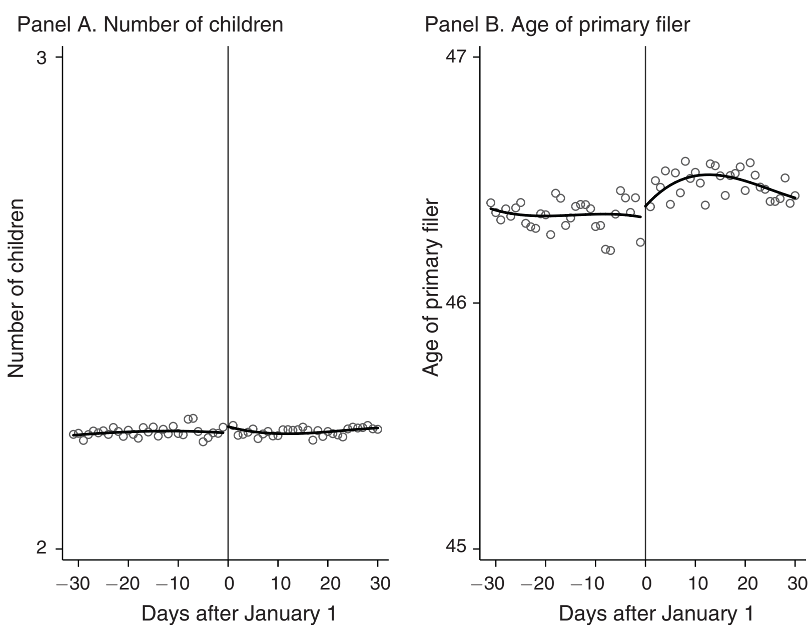

Figure A2. Average of Control Variables, by Day of Birth

Notes: The figure presents means by calendar day. Circles plot day-of-birth cell means of the number of children in the household or the age of the primary filer, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

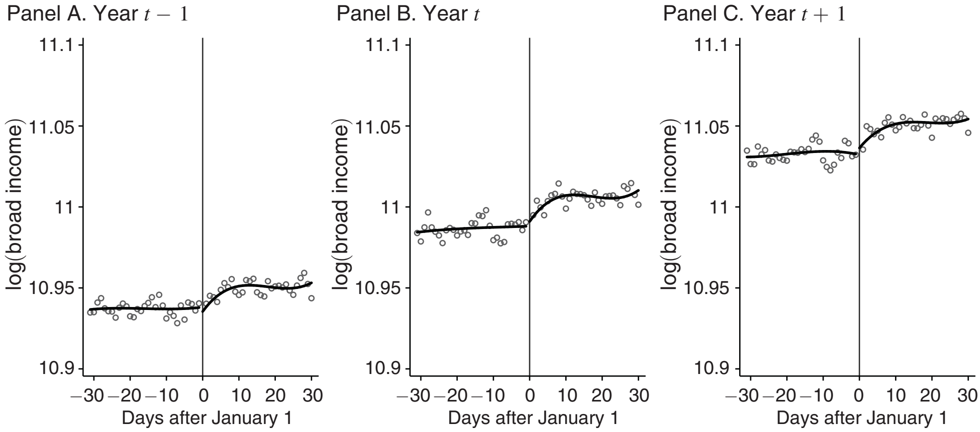

Figure A3. Average log Broad Income, by Day of Birth

Notes: The figure presents the average log broad income by calendar day. Circles plot day-of-birth cell means of log broad income, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

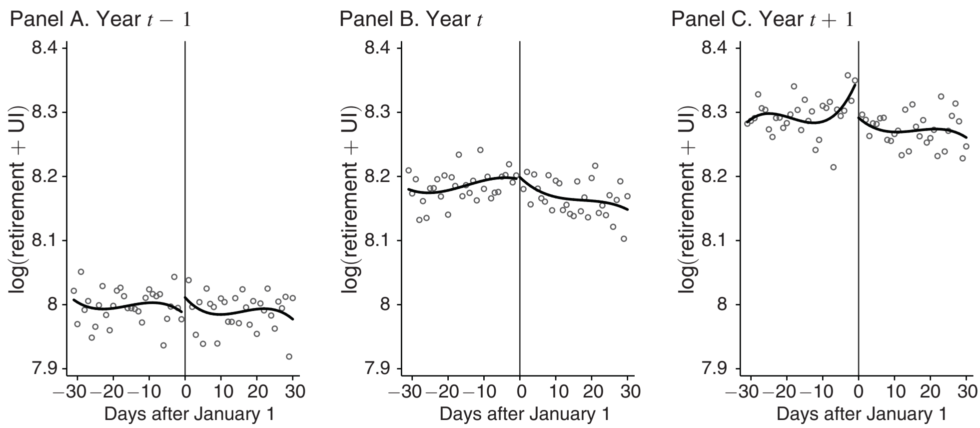

Figure A4. Retirement Income and Unemployment Insurance, by Day of Birth

Notes: The figure presents the average retirement and unemployment insurance income by calendar day. Circles plot day-of-birth cell means of log retirement and unemployment insurance income, and the solid lines plot cubic polynomials of the running variable, defined as the number of days after January 1, on each side of the discontinuity. The vertical line represents January 1, the date of birth that separates the treatment and control groups.

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