Personal Tax Changes and Financial Well-Being: Evidence from the Tax Cuts and Jobs Act

Christine L. Dobridge* [1], Joanne Hsu†[2], and Mike Zabek‡ [1]

[1]Board of Governors of the Federal Reserve System
[2]University of Michigan

December 12, 2023

Abstract

We estimate the effects of personal income tax decreases on subjective financial well-being and consumer credit outcomes. A plausibly causal design shows that tax decreases in the Tax Cuts and Jobs Act made survey respondents more likely to say they were "living comfortably" financially, with null effects at lower levels of subjective financial well-being. Estimates from a similar design using credit bureau data show that people who had larger tax decreases were modestly more likely to open new accounts, more likely to have higher consumer credit balances, and less likely to have a delinquency. Tax decreases had precisely estimated effects on credit scores that are indistinguishable from zero. Results suggest that tax decreases improve financial well-being in ways not fully proxied by expenditures.

Keywords: Taxes; subjective well-being; household finances; credit; financial well-being
JEL Numbers: H24, G50, I31

---

*20th Street and Constitution Avenue N.W. Washington, DC 20551; Christine.L.Dobridge@frb.gov, Board of Governors of the Federal Reserve System, 20th Street and Constitution Avenue, NW, Washington, DC 20551

†426 Thompson St, Ann Arbor, MI 48104; jwhsu@umich.edu.

‡Corresponding author: 20th Street and Constitution Avenue N.W. Washington, DC 20551; Mike.Zabek@frb.gov.
We thank Preston Harry, Zofsha Merchant, and Shooka Saket for excellent research assistance. The project benefited from useful comments by Elliot Anenberg, Neil Bhutta, Breno Braga, Lisa Dettling, Jeff Larrimore, Alicia Lloro, Andrew Whitten, David Splinter, and seminar participants at the Board of Governors of the Federal Reserve System, the Federal Reserve Bank of Richmond, the National Tax Association Spring Symposium, and the National Tax Association Annual Meetings. The analysis and conclusions set forth are those of the authors and do not indicate concurrence by other members of the Federal Reserve research staff, the Board of Governors, the Federal Reserve System, or the University of Michigan. Any remaining errors or omissions are the authors' responsibility. Declarations of interest: none.

I Introduction

The U.S. Congress has enacted changes to the personal income tax rate schedule and related tax provisions about 20 times in the post-war period, under almost every Presidential administration (Tax Foundation, 2023). With the most recent set of tax reductions——enacted in 2017 as part of legislation commonly known as the Tax Cuts and Jobs Act (TCJA)—set to expire in 2025, the policy debate regarding extending these reductions is heating up in Washington.[1] A sizeable body of work has studied the effects of tax policy changes involving lump sum payments, such as the tax rebates in 2001 or the stimulus payments during the Great Recession, on household financial positions (Agarwal, Liu and Souleles, 2007; Sahm, Shapiro and Slemrod, 2010; Gross, Notowidigdo and Wang, 2014). However, we know of little direct, causal evidence about the effects of more gradual, long-lived changes in personal income tax rates on household financial well-being. In this paper, therefore, we study the effects of tax reductions on financial well-being using a panel survey measuring subjective well-being alongside objective household finance outcomes drawn from a detailed panel of consumer credit data.

Our analysis focuses on personal income tax changes enacted as part of the TCJA. To identify the effects of this policy, we use a generalized difference-in-differences empirical strategy that leverages the substantial geographic and individual-level variation in the size of personal tax decreases. While tax rate changes may be a commonly used policy tool, it is unclear whether they cause meaningful changes in financial well-being, particularly if the changes in take-home income are gradual and not necessarily salient. At the same time, subjective assessments of financial well-being may not necessarily line up with objective measures of a family's financial characteristics.

We expect personal tax decreases to have weakly positive effects on financial well-being overall. If the tax decreases are not salient, we would expect little effect. But if they are salient, we would expect delinquencies to fall, while the a priori effects of personal tax changes on other credit outcomes are unclear. Delinquencies should either stay stable or fall because lower income taxes increase consumers' disposable incomes and, hence, their ability to pay their bills on time. Since missing payments yields costly fees, additional interest accrual, and higher interest rates on loans (both for current loans, in the case of penalty interest rates for credit cards, and for any new loans via lower credit scores), individuals face a strong incentive to use the income to help keep their loans current and stay out of delinquency. Effects on other outcomes are unclear, however, because consumers may increase their spending by more or less than the size of the tax cuts. One example of consumers increasing their spending by more than the tax cuts is a consumer who purchases a durable good like an appliance or a car using credit that they intend to pay off over time. Consumers might also choose to accumulate savings or lower their credit balances by paying down existing

[1] Public Law 115-97. The beginning of the TCJA extensions debate is summarized in Buhl (2022).

debts. As such, the theoretical prediction on the effect of tax cuts on consumer debt usage is ambiguous.

Using rich panel survey data from the Federal Reserve Board's Survey of Household Economics and Decisionmaking (SHED), we show that households that received larger tax cuts subsequently reported larger improvements in subjective financial well-being. An event-study analysis shows null effects and no pre-trend leading up to 2017 and a roughly stable positive effect in 2018 and 2019. In addition to living more comfortably from a financial perspective, households that received larger tax cuts were also less likely to have student debt and more likely to be homeowners after the tax cut. However, we find little effect on emergency savings or liquidity.

To determine if these improvements are limited to subjective measures, we draw on large-scale administrative data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) dataset and investigate more objective financial well-being outcomes (Bhutta, Skiba and Tobacman, 2015; Hu et al., 2018; Argys et al., 2020; Black et al., 2020). Our results show that consumers with larger tax cuts were more likely to open new accounts, were less likely to be delinquent on an account, and had higher credit balances following the enactment of the TCJA, though the magnitudes of the effects are modest. Increases in credit usage alongside decreases in delinquencies indicate greater spending power and better debt management, both of which are consistent with improvements in subjective financial well-being. However, we also find a precisely estimated zero effect on credit scores overall. The lack of credit score increases alongside other outcomes suggests that tax cuts improve subjective well-being without changing the overall credit risk profile from a lender's perspective.

Our most direct contributions are to literatures on the effects of tax changes, as well as income shocks more broadly, on household finances. A number of papers analyzing the effects of income shocks arising from tax or other fiscal policy changes have shown that consumers react by changing consumption, which might be financed by additional debt, as well as by paying down existing debt. In a study of Singaporeans, consumers reacted to the announcement of an exogenous, fiscal-policy-related income shock by increasing their spending, primarily using credit cards (Agarwal and Qian, 2014). Agarwal, Liu and Souleles (2007) and Sahm, Shapiro and Slemrod (2010) find evidence that borrowers use tax rebates to pay down debts; Skiba (2014) shows that receiving a tax rebate reduces the short-term likelihood of taking out a payday loan. Several related studies focus on changes to Medicaid enrollments induced by fiscal policy, with Hu et al. (2018) and Miller et al. (2021) showing that Medicaid expansions had notable effects on financial well-being, including reducing unpaid credit balances in collection and over-limit credit card spending, though they had little effect on credit scores or other credit delinquency measures. Argys et al. (2020) find that sudden disenrollment from Medicaid resulted in a notable decline in well-being as measured by credit scores.

Effects of the TCJA on financial well-being are of particular interest because tax cuts from the TCJA are more representative of other policy-relevant income fluctuations than other commonly studied tax changes like lumpy rebates or stimulus checks. Under the TCJA, households quickly saw small incremental changes in their disposable incomes as the Internal Revenue Service (IRS) updated withholding tables after the TCJA was enacted.[2] The changes were so small that multiple public opinion polls showed that few people believed they owed less in taxes under the TCJA (Tax Policy Center, 2019). Still, Scharlemann and van Straelen (2022) show the TCJA had meaningful household financial effects, finding the legislation increased a household's probability of mortgage refinancing conditional on refinancing incentives. The small and long-lived changes in disposable incomes brought by the TCJA most resemble the incremental increases to income driven by many other policy-relevant phenomena. In term of one of those policies, minimum wage hikes, Aaronson, Agarwal and French (2012) find greater spending on durables, often debt financed. Dettling and Hsu (2021) also show that higher minimum wages are associated with greater access to low-cost credit, as well as reduced payment delinquency.

Another contribution is showing effects on subjective financial well-being alongside more objective outcomes from survey and administrative data sources.[3] Our analysis validates that increases in subjective financial well-being were accompanied by increases in the numbers of credit accounts, increased credit balances, decreased delinquencies, a lower share of people holding student loans, and higher homeownership rates among people exposed to larger tax decreases. These other measures validate that subjective financial well-being can be useful in cases where more detailed data are not available. Our results also illustrate how subjective financial well-being can provide additional information that can help categorize the extent that consumers experience changes positively, much like subjective well-being more generally (Lachowska, 2017). In our case, the increase in subjective financial well-being alongside a precisely estimated zero result for credit scores shows how focusing only on credit scores can miss a measurable improvement in how people experience financial well-being. Using subjective financial well-being as an outcome of interest is particularly useful in cases where it is difficult to determine the extent that consumers experience a particular financial development as being harmful, such as the extent that having uncollected medical debt affects financial well-being (Brevoort, Grodzicki and Hackmann, 2020) or the extent that banning payday loans helps or harms consumers' financial well-being (Bhutta, Skiba and Tobacman,

[2]See the updated withholding tables, which are available on the IRS's website at: https://www.irs.gov/pub/irs-prior/p15–2018.pdf

[3]Lachowska (2017) shows that 2008 tax rebates had substantial effects on high-frequency measures of subjective well-being, and van Praag, Frijters and Ferrer-i-Carbonell (2003) relate financial well-being to broader measures of subjective well-being. Diener, Oishi and Tay (2018) provide a recent summary of the extensive literature in psychology on subjective well-being, including a cross-country examination of associations between progressive taxation and subjective well-being by Oishi, Schimmack and Diener (2012).

2015).[4]

This work also informs policymakers considering potential effects of tax policy or other related fiscal policy changes on household financial well-being. Given that most of the TCJA personal tax rate reductions are slated to expire at the end of 2025, our analysis has a few specific implications for the extension debate—that a personal tax increase with distributional properties similar to those of the TCJA tax decreases would likely have modest effects on consumer credit utilization and performance and meaningful negative effects on subjective well-being.

The remainder of the paper proceeds as follows. We present background information on the TCJA in Section II, describe the datasets and the empirical strategy in Section III, discuss the results in Section IV and conclude in Section V.

II Policy Background

The TCJA made considerable changes to the U.S. federal income tax code, including both personal and business tax provisions. The law was enacted rapidly—it was introduced on November 2, 2017, and became public law on December 22, 2017—and the personal income tax provisions applied to the tax year beginning January 1, 2018. Among the key changes to the personal tax code were (1) reducing income tax brackets (see Table A.1 for pre- and post-TCJA tax brackets), (2) increasing the standard deduction for taxpayers, (3) reducing the deductibility of mortgage interest from $1,000,000 to $750,000 of mortgage debt, (4) limiting the deductibility of state and local income and property taxes to $10,000, (5) raising the threshold for the alternative minimum tax for households, (6) raising and expanding the child tax credit, and (7) allowing for the deductibility of qualified business income for pass-through corporations. The Joint Committee on Taxation (JCT) projected that these personal tax provisions would substantially reduce tax revenue—by $1,127 billion from 2018 to 2027 (Joint Committee on Taxation, 2017*b*). The JCT also projected the bulk of the total reduction in tax revenue in the $100,000 to $200,000 and the $200,000 to $500,000 taxpayer income categories—$51 billion in 2019 (approximately $1,700 per taxpayer unit) and $47 billion in 2019 (approximately $5,100 per taxpayer unit), in these income categories, respectively. For taxpayers with income less than $50,000, the TCJA was projected to reduce tax revenue by about $14 billion in 2019, or about $150 per taxpayer unit (Joint Committee on Taxation, 2017*a*).

[4] Nanda and Banerjee (2021) review the studies of subjective financial well-being and document a significant uptick in recent studies, primarily within marketing.

III Data and Empirical Specification

III.I Data sources

To study how financial well-being changed following passage of the TCJA, we use two main data sources that include individual-level financial outcomes as well as information that facilitates the use of a detailed microsimulation model of the U.S. tax code provided by the National Bureau of Economic Research (NBER)—the TAXSIM model—to compute an estimated tax change for each household.

First, to understand subjective financial well-being, we turn to data from the SHED, conducted by the Board of Governors of the Federal Reserve System each year since 2013. Depending on the year, 6,000 to 12,000 individuals answer a broad range of questions related to household financial positions and well-being, including a rating of their overall financial well-being, as well as detailed questions about banking and credit, ability to cope with unexpected expenses, housing positions, and retirement preparation.

Since respondents to the SHED are drawn from a broader online panel survey in each year we can observe them across multiple years.[5] We focus our analysis on the period from 2015 to 2019. We begin the sample in 2015 (three years before the TCJA enactment) to provide for an ample pre-period and to focus on a period when SHED questionnaires are relatively stable. We end the sample in 2019 (two years after the enactment) so as not to capture effects of the COVID-19 pandemic in 2020.[6] To keep our focus on tax decreases, we restrict our sample to people whom we observe in 2017, which is the year we use to calculate the size of people's TCJA-induced tax decreases based on pre-legislation characteristics, as well as at least one year post-TCJA. Having pre- and post-treatment observations allows us to control for time-invariant individual characteristics in our regression specifications as well.

To gain further insight into how objective financial well-being of U.S. households changed following the TCJA, we use information reported in the CCP. The CCP dataset is an individual-level, anonymized panel of consumer credit records, drawn at the end of each quarter from Equifax——one of the three major credit bureaus in the United States. The data include detailed information drawn from credit reports, including loan balances, credit limits, and payment status. Aside from variables on age and geographic location, the dataset is generally limited to information about credit status. The CCP does not contain information, for example, about household income, employment status or demographic characteristics like race and education level.

We use a 10 percent sample of the overall CCP and create a panel with characteristics simlar

[5]The sample in each year is drawn from continuing members of the Ipsos KnowledgePanel—an online panel of individuals originally recruited via random-digit dialing and address-based sampling.

[6]A robustness appendix shows that we have similar results when we include a wider array of years.

to those of the SHED panel: limiting the period to 2015 to 2019, requiring an individual to be in the dataset in 2017 as well as at least one year following, and using end-of-year observations to smooth through fluctuations in financial positions over the course of the year. We also require an individual to have a reported Equifax Risk Score (a type of credit score) and to have a non-missing value for the number of new accounts established in a year, in order to ensure a person's credit records are sufficiently populated to make inferences across time. More details on construction of both the SHED and CCP data samples are presented in the Data Appendix.

III.II Computing estimated tax changes

Since the TCJA changed many aspects of the tax code simultaneously, the goal of this paper is to study how the overall changes in an individual's tax liability affected financial well-being. Our primary variable of interest, therefore, is the change in an individual's estimated average tax rate under the pre-TCJA tax code in 2017 compared with the post-TCJA tax code in 2018. For both the SHED and the CCP analyses, we estimate pre- and post-TCJA tax rates using characteristics measured in 2017, before the tax law change, inputted into the TAXSIM model (Feenberg and Coutts, 1993). This procedure gives us measures of tax changes that are exogenous with respect to changes in economic conditions due to the TCJA itself. Given the substantial interactions between the federal and state tax codes, we calculate the change in the total household average federal plus state tax rate—the federal-plus-state personal income tax liability divided by total income—to capture the overall change in tax burden for households in each of these groups. A positive value of the tax change variable reflects a tax rate reduction.

For the analysis using individual-level survey data from the SHED, the estimated tax rates are primarily based on pre-TCJA individual characteristics—household income, marital status, number of children, whether the person reports that they care for an adult, monthly mortgage payments, and state of residence. In addition to the individual-level variables, we include some limited census-tract-level statistics on mortgage interest payments derived from the Equifax Credit Risk Insight Servicing and Black Knight McDash (CRISM) dataset and property taxes from the U.S. Census Bureau's American Community Survey.

In the CCP, we do not observe information on individuals' incomes or other household-level tax inputs. Instead, we use TAXSIM to calculate hyperlocal, representative tax rates at the census tract level, separately for mortgage holders and non-holders and for single and joint filing status.[7] We input pre-TCJA data on census-tract-level median household or worker incomes and property taxes from the American Community Survey and local median mortgage payments from CRISM.

[7] Census tracts are small, sub-county geographic areas that include between about 420 and 1,200 housing units, approximating a neighborhood; for more information, see the discription on the U.S. Census Bureau's website at https://www.census.gov/history/www/programs/geography/tracts_and_block_numbering_areas.html.

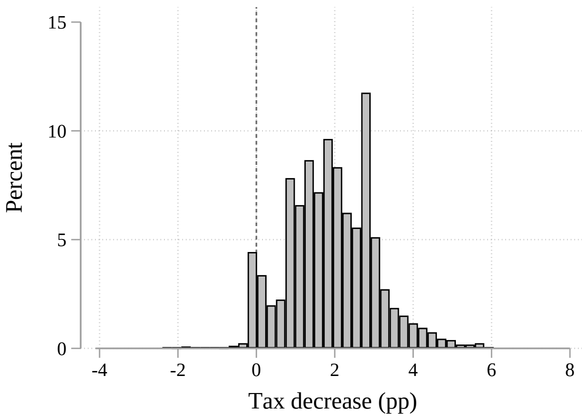
Specifically, we assign different representative tax rate changes to individuals in the CCP based on (1) the individual's census tract of residence in 2017, (2) whether we observe a mortgage for an individual in 2017, and (3) household size in 2017 (used to assign single or joint filing status). In robustness checks, we also present results using alternative inputs for computing representative tax changes, including estimates of dependents and business income. We provide a detailed description of the tax change calculations and data sources for the SHED and CCP analyses in the Data Appendix.

Figure 1, Panel A, shows the distribution of tax rate decreases in the SHED sample, and Panel B shows the same distribution for the CCP sample. Each distribution is centered around a 2 percentage point decrease and exhibits considerable variation. Because the tax decreases in the SHED are based on individual-level responses, we capture a higher variance in tax decreases using the SHED, an advantage of having individual-level data on incomes. The SHED data, however, also show some signs of binning due to income categories being collected as a categorical variable.[8] Estimated tax decreases in the CCP data also show considerable variation, reflective of substantial dispersion in hyperlocal measures of income, property taxes, and mortgage payments, in particular. In the CCP sample we observe a mean tax decrease of 1.8 percentage points, a median of 1.9, and a range from 0.93 at the 10th percentile to 2.7 at the 90th percentile.

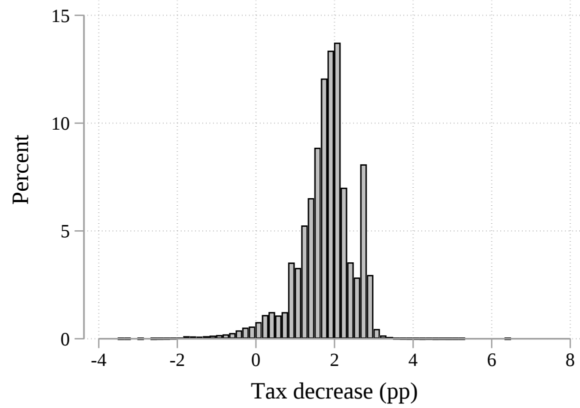
Panels C and D of Figure 1 present box plots of tax rate changes within the income bins of individuals recorded in the SHED—for the SHED and CCP samples, respectively. For both samples, the tax rate reduction tends to rise with income, and there is substantial variation in the tax reduction within each decile.

[8]The variable had 21 distinct categories in 2017, so it does provide significant variation, despite being binned. For simplicity, we use the midpoint of each bin in the tax decrease calculation, omitting observations in the top-most bin, which is unbounded.

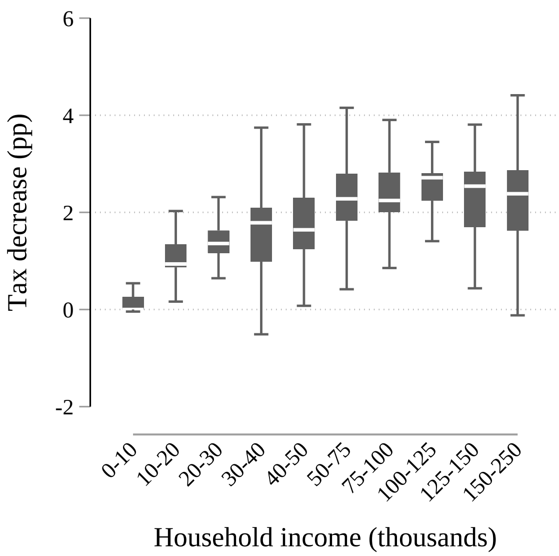
Figure 1: Distribution of TCJA personal tax reductions and relationship between tax reductions and income levels in the SHED and CCP analysis samples



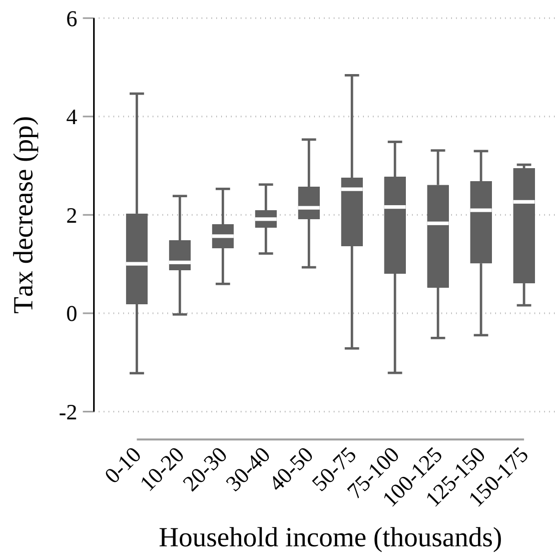
Panel A: SHED tax decreases



Panel B: CCP tax decreases



Panel C: SHED tax decreases and incomes



Panel D: CCP tax decreases and incomes

This figure presents histograms of the distribution of the TCJA personal tax reductions in percentage points (pp) for the SHED analysis sample (Panel A) and for the CCP analysis sample (Panel B), as well as box plots of the relationship between the tax reductions and sample income for the SHED and CCP samples (Panel C and Panel D). The box plots for the SHED analysis sample show data for the income bins reported to the survey in 2017. The box plots for the CCP analysis show data for the corresponding bins of census-tract median income (drawn from the U.S. Census Bureau's American Community Survey) assigned to each individual in the sample in 2017. Sample selection is described in Section III.I, and a detailed data description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Board, SHED; Federal Reserve Bank of New York/Equifax Consumer Credit Panel; U.S. Census Bureau, American Community Survey; CRISM; NBER, TAXSIM; and authors' calculations.

III.III Outcomes of interest

The main outcome variables we study from the SHED are individuals' perceptions of financial well-being. In the survey, respondents are asked "Overall, which one of the following best describes how well you are managing financially these days: (1) Finding it difficult to get by, (2) Just getting by, (3) Doing okay, or (4) Living comfortably." We create dummy variables for reporting a specific category or higher of "just getting by", "doing okay" and "living comfortably." We also study other financial outcomes that likely affect a person's sense of financial well-being, such as homeownership—an important vehicle for wealth accumulation—and the presence of student loans, which has been cited as a barrier to wealth accumulation. We also include several measures that are often important for those with low levels of financial well-being, such as having an emergency fund and the ability to pay a small, unexpected expense. Table A.3 reports summary statistics for the sample of SHED respondents, just before the TCJA was enacted in late 2017. Most people report relatively high levels of financial well-being, with 33 percent giving the highest category, living comfortably, and 41 percent saying that they are doing okay. Additionally, 68 percent of adults are homeowners and 14 percent have student loans.[9]

To understand whether the effect of larger tax changes on subjective financial well-being translates to meaningful differences in objective financial measures, we turn to the CCP data and analyze outcomes related to household credit utilization and performance. First, as a summary measure of household credit positions and credit risk from a lender's perspective, we study the Equifax Risk Score, a type of credit score (similarly to Bhutta, Skiba and Tobacman (2015)). As measures of utilization, we use the number of new accounts opened over the past 12 months and the natural log of total outstanding consumer credit balances as well as the natural log of total consumer credit plus mortgage balances. Finally, as a measure of credit performance, we use the number of delinquent accounts over a time horizon from 60 days delinquent to in severely derogatory status. Table A.4 reports summary statistics in 2017 for all variables used in the main analysis and in robustness checks with the CCP data. We observe that the mean credit score in the dataset is 703, with a slightly higher median level of 726. The mean number of new accounts is about 0.9 in 2017, with a median of zero, and the mean number of delinquencies is 0.3, also with a median of zero.

III.IV Empirical specification

To study how personal income tax changes affect subjective and objective financial well-being, we use a generalized difference-in-differences regression specification. This strategy allows us to

[9] The statistics are generally similar to those for the weighted cross-sectional survey presented in Federal Reserve Board (2018). However, there does appear to be a small amount of positive selection that could be due to the lack of weights (which we include as a robustness check) and the requirement that people are randomly selected into the sample and then agree to be interviewed in multiple waves.

exploit heterogeneity in the size of tax decreases from 2017 to 2018 to see if individuals who had larger tax decreases also experience larger changes in a financial well-being outcome. Because the tax changes all occurred in 2017, this strategy is not affected by econometric issues with comparing people who received treatments at different times (Roth et al., 2023).

For the SHED, we use individual panel survey data to estimate linear probability models of a series of dichotomous outcomes (Y_{it}) for an individual i in year t. Our coefficient of interest, β, is interpreted as the effect of a 1 percentage point tax rate reduction, calculated using changes in the tax code from 2017 to 2018 using pre-determined 2017 data in TAXSIM. Because the tax decreases went into effect in early 2018, we estimate effects by comparing outcomes in 2018 and 2019 with the same outcomes before and during 2017. In terms of controls, α_i represents an individual fixed effect that captures all time-invariant differences between individuals. Additionally, we also include α_{st}, a state-by-year fixed effect that controls for time-varying differences across states—including the local economic cycle—and a rich series of individual-level controls X_{it}, including contemporaneous household incomes, a cubic in age, rural status, and current employment status. Results are unweighted, and standard errors are clustered by state using the respondent's state when we first observe them. The regression specification is as follows:

$$Y_{it} = \beta \text{Tax decrease}_i \mathbf{1}(t > 2017) + \alpha_i + \alpha_{st} + \gamma X_{it} + \varepsilon_{it}. \tag{1}$$

For the CCP analysis, we use a similar specification with some adaptation for the different strengths of the administrative credit bureau data. As before, our coefficient of interest β measures the effect of a 1 percentage point decrease in a household's average tax rate, beginning in early 2018, on an outcome Y_{it}. Also as before, we use individual fixed effects (α_i) to control for time-invariant individual-level differences like education level, gender, race, or ethnicity. In contrast to the SHED specification, the high number of observations in the CCP dataset allows us to include much more detailed county-by-year fixed effects α_{ct} in this specification. These fixed effects control for highly localized, time-varying shocks to household financial conditions, such as the potential for changes in local-area hiring conditions due to the TCJA corporate tax rate changes. Since the CCP data do not have as many individual-level characteristics as the SHED data, we use fewer of them in this specification, controlling only for the age bin of the individual. Standard errors are clustered at the county level to allow for arbitrary correlation of errors within a local geography. The regression specification is as follows:

$$Y_{it} = \beta \text{Tax decrease}_i \mathbf{1}(t > 2017) + \alpha_i + \alpha_{ct} + \gamma_1 \text{Age bin}_i + \varepsilon_{it}. \tag{2}$$

In both specifications, variables for the treatment group and a post-period dummy are subsumed by the individual and the state-by-year or county-by-year fixed effects (compared with a standard difference-in-differences specification as in Bertrand, Duflo and Mullainathan (2004)). We discuss robustness to the specifications in Section IV.

IV Results

IV.I SHED results

In our analysis of the SHED data, we find evidence that tax reductions led to increases in the share of people who were living comfortably financially. As seen in column (1) of Table 1, a 1 percentage point larger tax reduction increases the likelihood that someone will say they are living comfortably by 1.5 percentage points. Since the average tax decrease was 2.0 percentage points, the coefficient estimate of 1.5 implies that the TCJA increased the overall share of people living comfortably by about 3.0 percentage points, ignoring general equilibrium effects. This outcome translates to slightly less than a 10 percent increase in the share of individuals living comfortably from the average level in 2017. Columns (2) and (3) show insignificant, though somewhat noisy, estimates for the likelihood that someone was more likely to be getting by or better, or doing okay or better, as a result of the TCJA tax decreases. Together, these results indicate that larger tax reductions improve subjective financial well-being at the top of the well-being distribution.

Putting the coefficient estimates in the context of year-to-year changes in overall well-being also shows that the effects are meaningful. The rise of 3.0 percentage points was about triple the 1 percentage point increase in the share of people reporting they were living comfortably from 2017 to 2018 (from 0.33 to 0.34) and matched the entire increase from 2017 to 2019 (from 0.33 to 0.36). We also cannot rule out the possibility that the TCJA had meaningful effects at lower thresholds, at least in terms of year-over-year aggregate changes.[10]

[10]Cross-sectional differences in the fraction of survey respondents reporting living comfortably are much larger than year-to-year aggregate differences, so the effects of the tax reduction are much smaller in comparison. For example, the share living comfortably in 2017 goes from 0.15 for families earning less than $40,000 per year to 0.62 for families earning more than $100,000.

Table 1: Effects of tax reductions on SHED variables

	Living comfortably	Doing okay	Getting by	Student loans	Homeowner	Would handle $400	Emergency fund
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable mean (2017)	*0.33*	*0.73*	*0.93*	*0.14*	*0.68*	*0.61*	*0.53*
Tax reduction (percentage points)	0.015***	-0.0016	-0.0025	-0.0062***	0.0059**	-0.0025	0.0030
	(0.0051)	(0.0048)	(0.0041)	(0.0020)	(0.0029)	(0.0054)	(0.0041)
Observations	13,056	13,056	13,056	13,056	13,056	13,056	13,056
Number of people	4,890	4,890	4,890	4,890	4,890	4,890	4,890
Individual fixed effects	X	X	X	X	X	X	X
Year-by-state fixed effects	X	X	X	X	X	X	X
Individual level controls	X	X	X	X	X	X	X

This table presents estimates of the effects of a 1 percentage point reduction in an individual's tax rate due to the TCJA on various measures of subjective financial well-being (columns 1 to 3) as well as other financial outcomes, including having a student loan outstanding (column 4), homeownership status (column 5), ability to handle an unexpected $400 expense with cash or its equivalent (column 6), and having a 3-month emergency fund (column 7). The results show that tax reductions increased the likelihood that someone said they were living comfortably financially and decreased the likelihood people had student loans. Results are estimated from Equation (1). This specification includes individual fixed effects, state-by-year fixed effects, and controls for a cubic in age, rural status, employment status, and categories of household income. Standard errors are clustered by state and are reported in parentheses. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. The regressions exclude people in the top bin of SHED household incomes where it is not possible to compute a midpoint of income for the purposes of the TAXSIM calculation. Sample selection is described in Section III.I, outcome variables are described in Section III.III, and a detailed data description for all variables is provided in the Data Appendix. Data sources are the Federal Reserve Board SHED; CRISM; NBER, TAXSIM; and authors' calculations.

We now turn to factors that may affect perceptions of financial well-being. First, we analyze student loan holdings. In the ongoing debate on student loan forgiveness, proponents argue that student loan payment burdens serve as a barrier to wealth accumulation in spite of the human capital investments they facilitate (Mezza et al., 2020). We find significant effects on holding student loans. Column (4) in Table 1 shows that a 1 percentage point decline in taxes decreases the likelihood that someone has student loans by 0.6 percentage points, suggesting that some people used their reduced tax liability to pay off student loans. Note that the panel nature of the data allows us to identify within-person changes in loan holding over time, so the results likely reflect paydown of debt, consistent with the results from Sahm, Shapiro and Slemrod (2010), which showed survey evidence that half of consumers used the 2008 tax rebates to pay down debt.

We also find some evidence of increased homeownership, an important vehicle for wealth accumulation—particularly since, for many families, a home is their largest asset. Column (5) shows that a 1 percentage point tax rate reduction led to a 0.6 percentage point increase in the likelihood that someone owns their home.[11]

We find insignificant and small effects for measures of savings and liquidity. In column (6), we report statistically insignificant and economically modest negative effects on the likelihood that someone would handle an unexpected $400 expense with cash or its equivalent in column (6).[12] We similarly find, as shown in column (7), insignificantly positive effects on the likelihood that someone has an emergency fund of three months of expenses.

Event studies in Panel A of Figure 2 show no evidence of a pre-trend in the share of people living comfortably before the tax changes and a stable effect after the tax change. The dark line in Panel A of Figure 2 is roughly flat from 2015 to 2017 and close to zero. The estimated effect jumps up to around 1.5 percentage points in 2018 and stays there in 2019. The other two levels of financial well-being, shown in Panels B and C, are also undetectably different from zero before the tax cut. However, all of these effects are imprecisely measured before 2017, as shown by the relatively wide 95 percent confidence interval indicated by the gray lines. As with the regression results, we see no detectable effect of the tax decreases at these other two, lower levels of financial well-being after the implementation of the TCJA.[13]

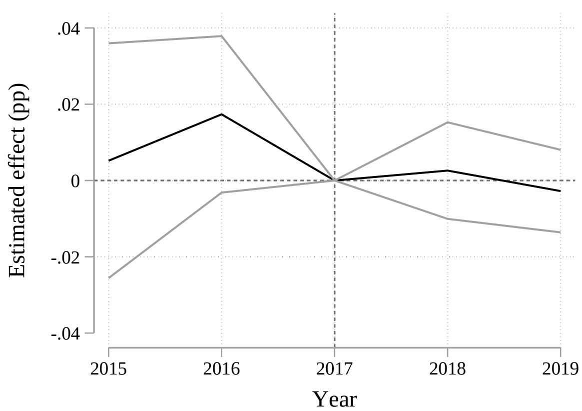
[11] The relationship between larger tax cuts and increasing homeownership may seem puzzling, since the TCJA removed some of the incentives for home-ownership by raising the standard deduction and imposing caps on both mortgage interest and state-level taxes, including property taxes. Our results are consistent with the channel of increased after-tax incomes offsetting some of these effects, similar to findings in analyses from the Tax Policy Center (McClelland, Mucciolo and Sayed, 2022).

[12] This measure is based on asking people to give the potentially multiple ways that they could pay for "an emergency expense that costs $400" based on their current financial situation. The variable is one if the person would cover the expense exclusively using cash, savings, or a credit card paid off at the next statement. The variable is zero if the person said they would pay for at least part of the $400 expense by borrowing or selling something, or if they said they would not have been able to cover the expense.

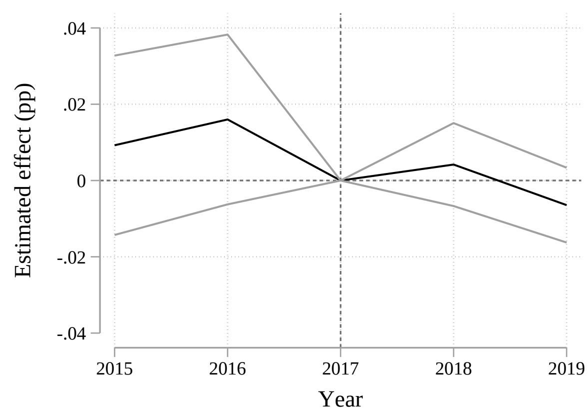
[13] The figure plots the β_k coefficient estimates and 95 percent confidence intervals from the following event-study

Figure 2: Event studies of effects on financial well-being

Panel A: Living comfortably



Panel B: Doing okay



Panel C: Getting by

This figure presents event-study effects of the TCJA tax decrease on various categories of households' subjective financial well-being: whether a household reports they are "Living comfortably" (Panel A), "Doing okay" or better (Panel B), or "Getting by" or better (Panel C). The black lines in the figures indicate coefficient estimates, and the gray lines indicate 95 percent confidence intervals obtained by estimating a specification similar to Equation (1), but interacting the TCJA tax decrease per individual with dummy variables for the years 2015, 2016, 2018, and 2019 (as described in footnote 13). In the years before the tax change, people who would experience larger tax reductions following the TCJA had no significant differences in their probability of saying they were living comfortably. In the years following the tax change, the reported likelihood of living comfortably increased. Sample selection is described in Section III.I, outcome variables are described in Section III.III, and a detailed data description for all variables is provided in the Data Appendix. Data sources are the Federal Reserve Board, SHED; CRISM; NBER, TAXSIM; and authors' calculations.

Robustness exercises also show that the effects on financial well-being are of similar magnitudes with different controls and sample restrictions. We discuss these results in detail in Online Appendix Section V. Appendix Table A.5 shows that the estimated effect of a 1 percentage point tax decrease on the likelihood that someone is living comfortably is quite stable across specifications and samples. An exception is that effect sizes are larger when we omit the individual fixed effect and when controls are lagged twice, so they are pre-determined with respect to the effect of the tax change. The larger effect in the twice-lagged control sample appears due to the smaller sample size caused by this restriction, however, and not the inclusion of the twice-lagged controls themselves. Results are similar for a specification that uses contemporaneous controls, but restricts the sample to individuals for whom twice-lagged controls are available.

Overall, we see evidence from survey responses that the TCJA led people to feel more comfortable financially. Some people may have also felt more comfortable because tax decreases helped them pay down student loan obligations but we see no detectable positive effects on emergency savings or liquidity. Another factor that lends credibility to the result is that it is identified off of changes in people's subjective well-being over time, not cross-sectional differences. So the result cannot be due to permanent differences in people's dispositions or similarly unchanging differences in how people rate their subjective well-being for a fixed set of circumstances. However, it is still relevant to understand if these differences are reflected in other, more objective measures, and we turn to that question using large-scale administrative credit bureau data from the CCP in the next section.

IV.II CCP results

Table 2 presents results from the CCP analysis, showing the effects of larger tax decreases on the Equifax Risk Score, the number of new accounts opened, the number of delinquencies, and total balances of consumer credit accounts as well as consumer credit plus mortgage accounts. We observe no statistically significant relationship between the size of the tax decreases and credit scores in column (1). In contrast, we observe a modest and statistically significant increase in new accounts in column (2): a 1 percentage point tax rate reduction led to an estimated increase in the number of new accounts by 0.03 across the sample. An average 1.76 percentage point tax cut implies a 0.05 increase in the number of new accounts for the average individual—an effect of small magnitude compared to about 0.9 new accounts opened on average by an individual in 2017.

We observe modest but statistically significant effects for delinquencies as well (column 3) We find that a 1 percentage point reduction in the personal tax rate results in a decline in the number of delinquencies of 0.006, with a 1.76 percentage point tax cut resulting in a 0.01 average decrease

specification: $Y_{it} = \sum_{k=2015,k\neq 2017}^{2019} \beta_k \cdot I\{k=t\} \cdot \text{Tax decrease}_i + \alpha_i + \alpha_{st} + \gamma X_{it} + \varepsilon_{it}$. Results are unweighted and standard errors are clustered by state using the respondent's state when we first observe them.

in the number of delinquencies—also small values compared to overall average delinquencies of 0.3 in 2017. Looking at total consumer credit balances (column 4), we find total balances increase following the TCJA, with the interpretation that a 1 percentage point tax rate reduction increases consumer credit balances by 0.9 percent. This result is qualitatively consistent with observing an increase in new credit accounts. We find a much larger effect when looking at consumer credit plus mortgage balances (column 5)—with a 1 percentage point tax rate reduction increasing balances by 4.1 percent—suggesting that there were also substantial effects of the tax cuts on mortgage holdings, consistent with the SHED results on homeownership. Of note, the null result on credit scores is consistent with the results on delinquencies and account balances—lower delinquencies would tend to raise credit scores, while higher account balances and new accounts would tend to depress credit scores.[14]

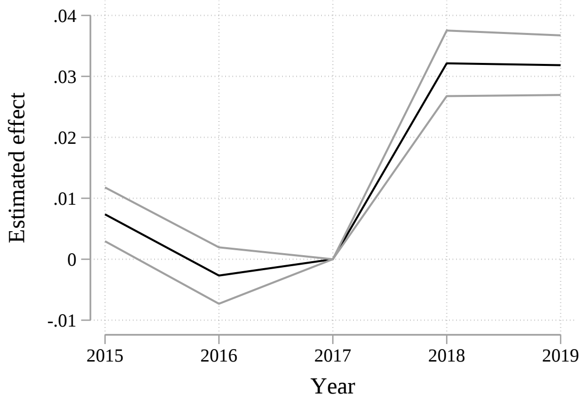
[14] We present results for the natural log of consumer credit balances and the natural log of consumer credit plus mortgage balances, which exclude accounts with zero balance. Results for the natural log of balances plus one are qualitatively similar, with very similar coefficients and statistical significance. Additionally, we find similar effects for delinquencies over other time horizons, such as 30 to 120 days past due, or 60 to 120 days past due.

Table 2: Effects of tax reductions on consumer credit outcomes

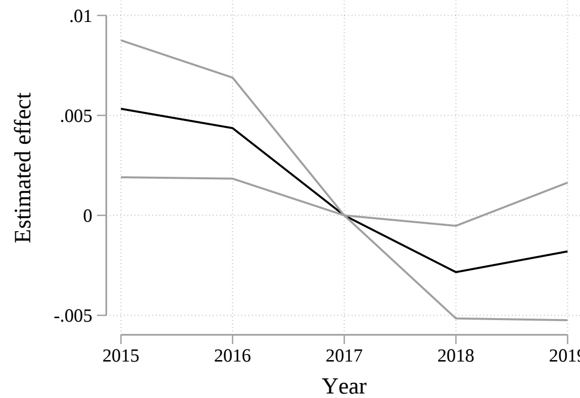
	Equifax Risk Score	# New credit accounts	# Delinquencies	ln(Consumer credit balances)	ln(Consumer credit plus mortgage balances)
	(1)	(2)	(3)	(4)	(5)
Dependent variable mean (2017)	*703*	*0.881*	*0.298*	*9.841*	*9.001*
Tax reduction (percentage points)	-0.0746	0.0305***	-0.0055***	0.0089***	0.0413***
	(0.0692)	(0.0022)	(0.0015)	(0.0034)	(0.0032)
Observations	5,513,155	5,513,155	5,513,155	4,592,701	4,704,981
Number of people	1,160,652	1,160,652	1,160,652	1,008,534	1,023,640
Individual fixed effects	X	X	X	X	X
County-by-year fixed effects	X	X	X	X	X
Age bin control	X	X	X	X	X

This table presents estimates of the effects of a 1 percentage point reduction in the average hyperlocal personal tax rate due to the TCJA on the Equifax Risk Score (column 1), the number of new credit accounts (column 2), the number of accounts that are 60 days delinquent to in severely derogatory status (column 3), the natural log of total consumer credit balances (column 4), and the natural log of total consumer credit balances plus mortgage balances (column 5). Results show the tax reductions led to an increase in the number of new credit accounts, consumer credit balances, and consumer credit plus mortgage balances, led to a decrease in delinquencies, and had no statistically significant effect on credit scores. Results are estimated using Equation (2). This specification includes county-by-year fixed effects, individual fixed effects, and a control for an individual's age bracket. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Sample selection is described in Section III.I, outcome variables are described in Section III.III, and a detailed data description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Bank of New York/Equifax Consumer Credit Panel; U.S. Census Bureau, American Community Survey; CRISM; NBER, TAXSIM; and authors' calculations.

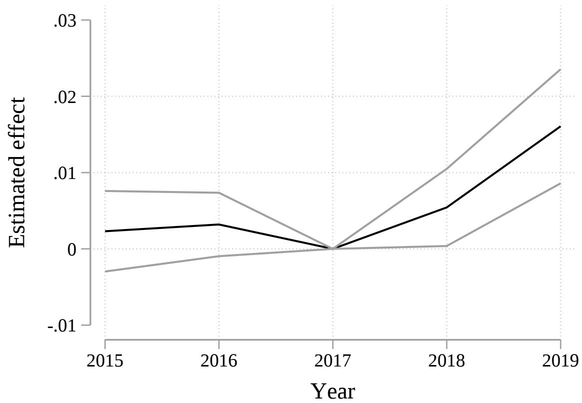
Figure 3: Event studies of effects on consumer credit outcomes



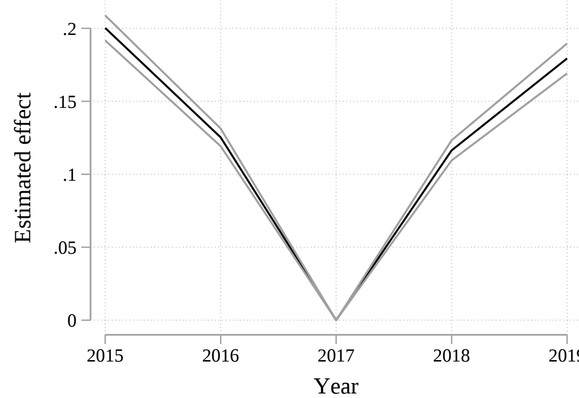
Panel A: # New accounts



Panel B: # Delinquencies



Panel C: ln(Consumer credit balances)



Panel D: ln(Consumer credit plus mortgage balances)

This figure presents event-study effects of the TCJA tax decrease on several consumer credit outcomes: the number of new consumer credit accounts (Panel A), the number of accounts 60 days delinquent to in severely derogatory status (Panel B), the natural log of consumer credit balances (Panel C), and the natural log of consumer credit plus mortgage balances (Panel D). The black lines in the figures indicate coefficient estimates and the gray lines indicate 95 percent confidence intervals obtained by estimating a specification similar to Equation (2), but interacting the TCJA tax decrease per individual with dummy variables for the years 2015, 2016, 2018, and 2019 (as described in footnote 15). We observe trends that suggest the parallel trends assumption holds particularly for the number of new accounts and for consumer credit balances. Sample selection is described in Section III.I, outcome variables are described in Section III.III, and a detailed data description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Bank of New York/Equifax Consumer Credit Panel; U.S. Census Bureau, American Community Survey; CRISM; NBER, TAXSIM; and authors' calculations.

Figure 3 presents event-study graphs showing results of the effects of the tax decrease in 2017 on outcomes presented in Table 2 with statistically significant effects: new accounts (Panel A), delinquencies (Panel B), consumer credit balances (Panel C), and consumer credit plus mortgage balances (Panel D).[15] We observe trends that suggest the parallel trends assumption holds for the number of new accounts and consumer credit balances. For delinquencies, we observe what appears to be some decline for those who eventually have larger tax decreases starting in 2017. While it is possible that individuals expecting tax reform to pass in 2017 were able to make budgetary changes to stave off delinquency, observing some delinquency decline in 2017 may also weaken the causal interpretation of this result. For consumer credit plus mortgage balances, however, we observe a very large decline in the years leading up to TCJA passage in 2017 and then an increase in 2018 and 2019, suggesting that any pre-trend would work against finding a result from the TCJA tax decreases.

We present robustness results for alternative specifications and alternative methods for calculating the tax decrease in Table A.6 and describe them in detail in Online Appendix Section V. We find that the results for our preferred specification are similar in magnitude and significance to those for alternatives such as including county and year fixed effects and including economic control variables like the unemployment rate with county and year fixed effects. We also find our results are robust to alternative ways of calculating the tax decrease, including a method for incorporating dependents into the calculation, a method for incorporating business income, and an alternative method for calculating property tax liabilities.

On the whole, we find that our evidence on the effect of personal tax decreases on objective measures of household financial well-being is consistent with the results on subjective measures. Since delinquencies are symptoms of damaging financial events, a lower probability of delinquency could lead to higher levels of financial well-being. The relationship is less clear cut for increased numbers of new accounts and larger consumer credit balances, however, since these could occur due either to a consumption response, as in Dinerstein, Yannelis and Chen (2023), or to borrowing to cover fixed expenses after a negative income shock, as in Dodini, Larrimore and Tranfaglia (2022). Looking at other outcomes as well as the context of our effect suggests that a consumption response is more natural since we observe increased borrowing after a positive income shock, accompanied by improved financial well-being in terms of other variables—delinquencies, credit scores, home ownership, student loans, and subjective financial well-being. Additionally bolstering this interpretation of increased borrowing due to a consumption response, we find that credit scores were essentially unchanged, so the tax decreases did not affect individuals' credit risk profiles from a lender's perspective on average.

[15] The figure plots the β_k coefficients and the 95 percent confidence interval from the following event-study specification: $Y_{it} = \sum_{k=2015,k\neq 2017}^{2019} \beta_k \cdot I\{k=t\} \cdot \text{Tax decrease}_i + \alpha_i + \alpha_{ct} + \gamma_1 \text{Age bin}_i + \varepsilon_{it}$. Standard errors are clustered at the county level.

V Conclusion

This paper provides plausibly causal evidence that larger personal income tax decreases after the TCJA led to greater improvements in subjective well-being in the two years after their enactment, with effects concentrated at the highest level of well-being—individuals reporting living comfortably. Improvements in subjective financial well-being are also accompanied by a decrease in the likelihood of having student loans but by little detectable effect on measures of household savings.

Larger tax decreases also have modest effects on consumer credit outcomes, which are generally consistent with the results on subjective well-being. TCJA tax decreases are associated with a small decline in the number of delinquent accounts and a small increase in the number of new accounts. On net, we observe no change in individuals' credit scores.

Overall, the results are consistent with tax decreases improving people's overall perceptions of their finances whether or not they notably improve less subjective measures like cash on hand or credit delinquency. Since the tax decreases in the TCJA were concentrated among households earning higher incomes, however, our results leave open the possibility that tax decreases that affect lower income households could have different effects. Households living on lower incomes may use their tax decreases differently—for example, in ways that may leave their levels of financial well-being unchanged, or move them along different margins in terms of borrowing and saving. Additional research could shed light on these differences and further clarify the mechanisms behind our results.

References

Aaronson, Daniel, Sumit Agarwal, and Eric French. 2012. "The spending and debt response to minimum wage hikes." *American Economic Review*, 102(7): 3111–39.

Agarwal, Sumit, and Wenlan Qian. 2014. "Consumption and debt response to unanticipated income shocks: Evidence from a natural experiment in Singapore." *American Economic Review*, 104(12): 4205–30.

Agarwal, Sumit, Chunlin Liu, and Nicholas S. Souleles. 2007. "The reaction of consumer spending and debt to tax rebates—evidence from consumer credit data." *Journal of Political Economy*, 115(6): 986–1019.

Argys, Laura M., Andrew I. Friedson, M. Melinda Pitts, and D. Sebastian Tello-Trillo. 2020. "Losing public health insurance: TennCare reform and personal financial distress." *Journal of Public Economics*, 187: 104202.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *The Quarterly Journal of Economics*, 119(1): 249–275.

Bhutta, Neil, and Lisa Dettling. 2018. "Money in the bank? Assessing families' liquid savings using the Survey of Consumer Finances." Board of Governors of the Federal Reserve System.

Bhutta, Neil, Paige Marta Skiba, and Jeremy Tobacman. 2015. "Payday loan choices and consequences." *Journal of Money, Credit and Banking*, 47(2-3): 223–260.

Black, Sandra E., Jeffrey T. Denning, Lisa J. Dettling, Sarena Goodman, and Lesley J. Turner. 2020. "Taking it to the limit: Effects of increased student loan availability on attainment, earnings, and financial well-being." National Bureau of Economic Research.

Brevoort, Kenneth, Daniel Grodzicki, and Martin B. Hackmann. 2020. "The credit consequences of unpaid medical bills." *Journal of Public Economics*, 187: 104203.

Buhl, John. 2022. "The tax debate is just beginning." *TaxVox: Federal Budget and Economy*.

Consumer Financial Protection Bureau. 2017. "CFPB financial well-being scale: Scale development technical report." Consumer Financial Protection Bureau.

Deaton, Angus. 2008. "Income, health, and well-being around the world: Evidence from the Gallup World Poll." *Journal of Economic Perspectives*, 22(2): 53–72.

Dettling, Lisa J., and Joanne W. Hsu. 2021. "Minimum wages and consumer credit: Effects on access and borrowing." *The Review of Financial Studies*, 34(5): 2549–2579.

Diener, Ed, Shigehiro Oishi, and Louis Tay. 2018. "Advances in subjective well-being research." *Nature Human Behaviour*, 2(4): 253–260.

Dinerstein, Michael, Constantine Yannelis, and Ching-Tse Chen. 2023. "Debt moratoria: Evidence from student loan forbearance." National Bureau of Economic Research Working Paper 31247.

Dodini, Samuel, Jeff Larrimore, and Anna Tranfaglia. 2022. "Financial repercussions of SNAP work requirements." Board of Governors of the Federal Reserve System.

Federal Reserve Board. 2016. "Report on the economic well-being of U.S. households in 2015." Board of Governors of the Federal Reserve System.

Federal Reserve Board. 2018. "Report on the economic well-being of U.S. households in 2017." Board of Governors of the Federal Reserve System.

Federal Reserve Board. 2019. "Report on the economic well-being of U.S. households in 2018." Board of Governors of the Federal Reserve System.

Federal Reserve Board. 2020. "Report on the economic well-being of U.S. Households in 2019, featuring supplemental data from April 2020." Board of Governors of the Federal Reserve System.

Federal Reserve Board. 2021. "Economic well-being of U.S. households in 2020." Board of Governors of the Federal Reserve System.

Feenberg, Daniel, and Elisabeth Coutts. 1993. "An introduction to the TAXSIM model." *Journal of Policy Analysis and Management*, 12(1): 189–194.

Goodman, Lucas, Katherine Lim, Bruce Sacerdote, and Andrew Whitten. 2021. "How do business owners respond to a tax cut? Examining the 199A deduction for pass-through firms." National Bureau of Economic Research.

Gross, Tal, Matthew J. Notowidigdo, and Jialan Wang. 2014. "Liquidity constraints and consumer bankruptcy: Evidence from tax rebates." *Review of Economics and Statistics*, 96(3): 431–443.

Hu, Luojia, Robert Kaestner, Bhashkar Mazumder, Sarah Miller, and Ashley Wong. 2018. "The effect of the Affordable Care Act Medicaid expansions on financial wellbeing." *Journal of Public Economics*, 163: 99–112.

Ipsos Group S.A. n.d.. "KnowledgePanel: A methodological overview." Ipsos Group S.A. Available at https://www.ipsos.com/sites/default/files/ipsosknowledgepanelmethodology.pdf.

Joint Committee on Taxation. 2017*a*. "Distributional effects of the conference agreement for H.R.1, the 'Tax Cuts and Jobs Act'."

Joint Committee on Taxation. 2017*b*. "Estimated budget effects of the conference agreement for H.R. 1, the 'Tax Cuts and Jobs Act'."

Lachowska, Marta. 2017. "The effect of income on subjective well-being." *Journal of Human Resources*, 52(2): 374–417.

Larrimore, Jeff, Maximilian Schmeiser, and Sebastian Devlin-Foltz. 2015. "Should You Trust Things You Hear Online? Comparing SHED and Census Bureau Survey Results." Board of Governors of the Federal Reserve System.

Lee, Donghoon, and Wilbert Van der Klaauw. 2010. "An introduction to the FRBNY Consumer Credit Panel." *FRB of New York Staff Report*, 1(479).

McClelland, Robert, Livia Mucciolo, and Safia Sayed. 2022. "New evidence on the effect of the TCJA on the housing market." Tax Policy Center research report. Available at https://www.taxpolicycenter.org/publications/new-evidence-effect-tcja-housing-market.

Mezza, Alvaro, Daniel Ringo, Shane Sherlund, and Kamila Sommer. 2020. "Student loans and homeownership." *Journal of Labor Economics*, 38(1): 215–260.

Miller, Sarah, Luojia Hu, Robert Kaestner, Bhashkar Mazumder, and Ashley Wong. 2021. "The ACA Medicaid expansion in Michigan and financial health." *Journal of Policy Analysis and Management*, 40(2): 348–375.

Nanda, Ambika Prasad, and Ranjan Banerjee. 2021. "Consumer's subjective financial well-being: A systematic review and research agenda." *International Journal of Consumer Studies*, 45(4): 750–776.

Oishi, Shigehiro, Ulrich Schimmack, and Ed Diener. 2012. "Progressive taxation and the subjective well-being of nations." *Psychological Science*, 23(1): 86–92. PMID: 22157676.

Roth, Jonathan, Pedro H.C. Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's trending in difference-in-differences? A synthesis of the recent econometrics literature." *Journal of Econometrics*.

Sahm, Claudia R., Matthew D. Shapiro, and Joel Slemrod. 2010. "Household response to the 2008 tax rebate: Survey evidence and aggregate implications." *Tax Policy and the Economy*, 24(1): 69–110.

Scharlemann, Therese C., and Eileen van Straelen. 2022. "Fiscal policy and geographic variation in the refinancing channel." *Available at SSRN*.

Sherter, Alain. 2019. "Nearly 40% of Americans can't cover a surprise $400 expense." *CBS News*.

Skiba, Paige Marta. 2014. "Tax rebates and the cycle of payday borrowing." *American Law and Economics Review*, 16(2): 550–576.

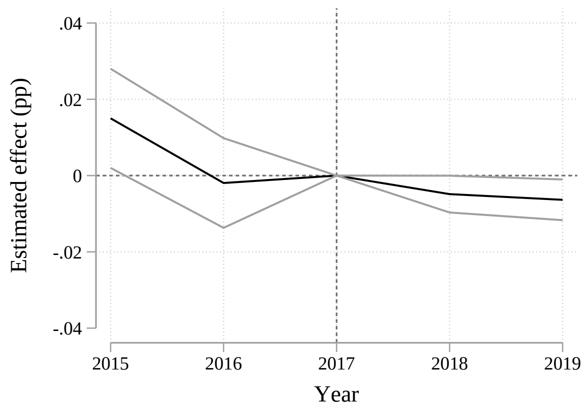
Tax Foundation. 2023. "Historical U.S. federal individual income tax rates & brackets, 1862-2021." Available at https://taxfoundation.org/historical-income-tax-rates-brackets/.

Tax Policy Center. 2019. "A last look at the 2019 filing season."

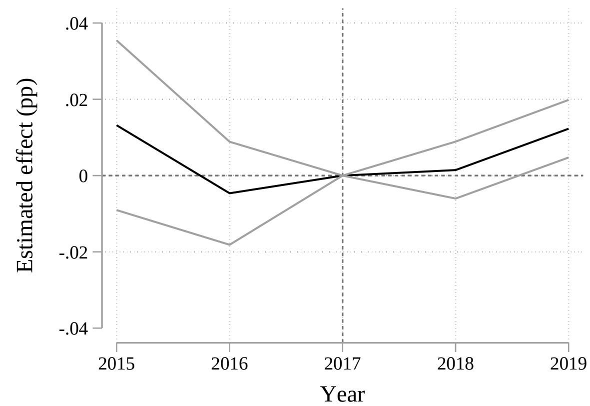
van Praag, B.M.S, P. Frijters, and A. Ferrer-i-Carbonell. 2003. "The anatomy of subjective well-being." *Journal of Economic Behavior and Organization*, 51(1): 29–49.

Appendix figures and tables

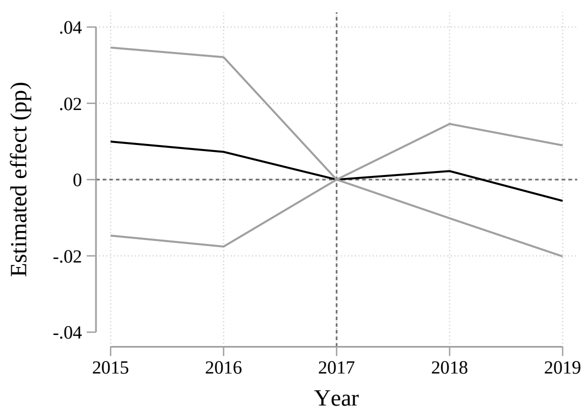
Figure A.1: Event studies of effects on other SHED variables



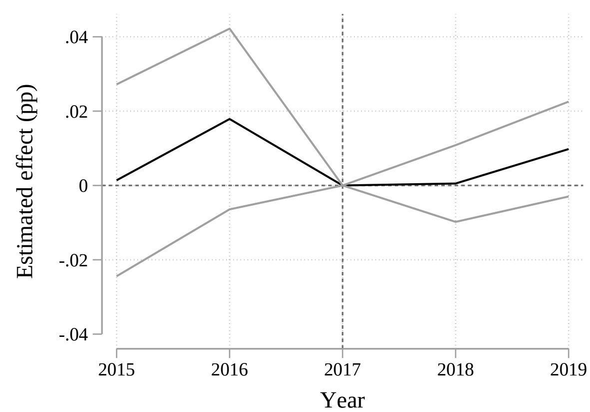
Panel A: Student loans



Panel B: Homeowner



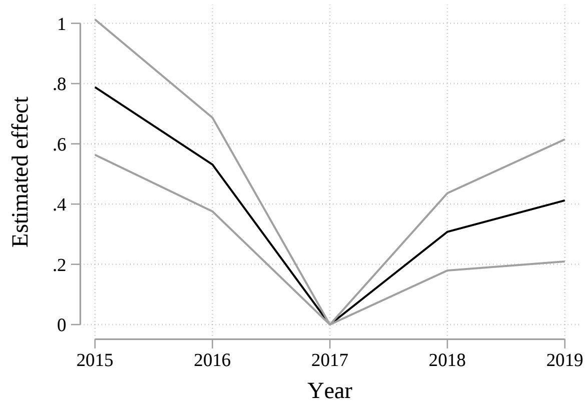
Panel C: Would pay a $400 expense solely with cash or an equivalent



Panel D: Has a three-month emergency fund

This figure presents event-study effects of the TCJA tax decrease on various SHED outcome variables: whether a household reports having a student loan outstanding (Panel A), owning a home (Panel B), if they would pay a $400 expense with cash or a cash equivalent (Panel C), and having a three-month emergency fund (Panel D). The black lines in the figures indicate coefficient estimates, and the gray lines indicate 95 percent confidence intervals obtained by estimating a specification similar to Equation (1), but interacting the TCJA tax decrease with dummy variables for the years 2015, 2016, 2018, and 2019 (as detailed in footnote 13). Sample selection is described in Section III.I, outcome variables are described in Section III.III, and a detailed data description for all variables is provided in the Data Appendix. Data sources Federal Reserve Board, SHED; CRISM; NBER, TAXSIM; and authors' calculations.

Figure A.2: Event study of effects on Equifax Risk Score



This figure presents event-study effects of the TCJA tax decrease on the Equifax Risk Score. The black line in the figure indicates coefficient estimates, and the gray lines indicate 95 percent confidence intervals obtained by estimating a specification similar to Equation (2), but interacting the TCJA tax decrease with dummy variables for the years 2015, 2016, 2018, and 2019 (as detailed in footnote 15). Sample selection is described in Section III.I, outcome variables are described in Section III.III, and a detailed data description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Bank of New York/Equifax Consumer Credit Panel; U.S. Census Bureau, American Community Survey; CRISM; NBER, TAXSIM; and authors' calculations.

Table A.1: Personal income tax brackets pre- and post-TCJA

Pre-TCJA			Post-TCJA		
	Income threshold			Income threshold	
Marginal tax rate	Single filers	Joint filers	Marginal tax rate	Single filers	Joint filers
10%	\$0	\$0	10%	\$0	\$0
15%	\$9,325	\$18,650	12%	\$9,525	\$19,050
25%	\$37,950	\$75,900	22%	\$38,700	\$77,400
28%	\$91,900	\$153,100	24%	\$82,500	\$165,000
33%	\$191,650	\$233,350	32%	\$157,000	\$315,000
35%	\$416,700	\$416,700	35%	\$200,000	\$400,000
39.60%	\$418,400	\$470,700	37%	\$500,000	\$600,000

This table shows personal income marginal tax rate brackets for single filers and joint filers before and after the enactment of the TCJA. The rate shown is applied to taxable income above the amounts given in the following column up to the income level for the next highest tax bracket. Data source is the Internal Revenue Service.

Table A.2: SHED analysis sample construction

	2015	2016	2017	2018	2019	Total
Cross sectional observations	5,642	6,610	12,447	11,316	12,173	48,188
Reason excluded						
Not in panel	2,695	3,885	6,024	4,371	4,041	21,016
Not observed pre and post	2,127	1,553	1,123	3,217	4,780	12,800
Insufficient tax info	30	40	196	123	117	506
Item nonresponse	6	43	111	57	32	249
Household income topcoded	21	31	207	150	152	561
Baseline sample	763	1,058	4,786	3,398	3,051	13,056

This table shows the number of observations in the overall, cross-sectional SHED in each year along with the number that are excluded from the analysis sample for various reasons broken out by each year. The first row gives the total number of observations in the dataset for that year. The next rows give reasons why an observation is not in the analysis sample. "Not in panel" means that people are not observed in any of the other years analyzed—the most common reason that people are not included in the analysis sample. "Not observed pre and post" means the person is observed only before or after 2017. "Insufficient tax info" means that we did not have enough information to calculate tax rate changes for that person either because they were not interviewed in 2017 or because of item nonresponse in 2017. "Item nonresponse" means that the observation was excluded because an outcome or control variable was refused or otherwise unavailable due to item nonresponse. "Household income topcoded" means that the observation was removed because it included a household income in the topmost, unbounded bin. The last row gives the baseline sample. Note that the size of the SHED was nearly doubled from 2017 forward. Data sources are the Federal Reserve Board, SHED; CRISM; NBER, TAXSIM; and authors' calculations.

Table A.3: Summary statistics: SHED analysis

	Number of observations	Mean	Median	Standard deviation
Variable of interest				
Tax reduction (percent)	4,786	1.95	1.93	1.13
Outcome variables				
Living comfortably	4,786	0.33	0.00	0.47
Doing okay	4,786	0.41	0.00	0.49
Just getting by	4,786	0.19	0.00	0.40
Struggling to get by	4,786	0.07	0.00	0.26
Student loans	4,786	0.14	0.00	0.35
Home owner	4,786	0.68	1.00	0.47
Would cover $400 with cash or equivalent	4,786	0.61	1.00	0.49
Has emergency fund	4,786	0.53	1.00	0.50
Individual control variables				
Household income less than $25,000	4,786	0.20	0.00	0.40
Household income $25,000 to $49,999	4,786	0.23	0.00	0.42
Household income $50,000 to $99,999	4,786	0.27	0.00	0.44
Household income $100,000 to $199,999	4,786	0.26	0.00	0.44
Household income $200,000 to $249,999	4,786	0.04	0.00	0.19
Less than high school	4,786	0.03	0.00	0.17
High school or GED	4,786	0.26	0.00	0.44
Some college	4,786	0.34	0.00	0.47
College or more	4,786	0.37	0.00	0.48
Working	4,786	0.57	1.00	0.50
Nonmetropolitan	4,786	0.14	0.00	0.35
White	4,786	0.72	1.00	0.45
Black	4,786	0.11	0.00	0.31
Latino or Latina	4,786	0.12	0.00	0.32
Woman	4,786	0.46	0.00	0.50
Age	4,786	52.44	55	16.98

This table presents summary statistics for variables used in the analysis of the effect of the TCJA tax decrease on variables from the SHED. Column (1) presents the number of observations, column (2) presents the mean value, column (3) presents the median, and column (4) presents the standard deviation. The sample shown in the table consists of values from the 2017 survey for people included in the baseline SHED analysis data sample (as included in the specification shown in Table 1). Sample selection is described in Section III.I, and a detailed description for all variables is provided in the Data Appendix. Data sources are the Federal Reserve Board, SHED; and authors' calculations.

Table A.4: Summary statistics: CCP analysis

	Number of observations	Mean	Median	Standard deviation
Variable of interest				
Tax reduction (percent)	1,128,407	1.76	1.85	0.73
Outcome variables				
Equifax Risk Score	1,128,407	703	726	105
# New accounts	1,128,407	0.88	0.00	1.27
# Delinquencies	1,128,407	0.30	0.00	1.10
ln(Consumer credit balances)	943,664	9.00	9.44	1.90
ln(Consumer credit plus mortgage balances)	965,064	9.84	10.10	2.28
Control variables				
State level:				
Real GDP growth (percent)	1,128,407	2.60	2.34	1.62
County level:				
Unemployment rate (percent)	1,128,407	3.98	3.80	1.21
Average weekly wage growth (percent)	1,128,407	3.50	3.16	2.70
Employment growth (percent)	1,128,407	1.43	1.32	1.63
Zip code level:				
Ordinary dividends per return (thousands)	1,128,407	1.67	0.75	4.01
Net capital gains per return (thousands)	1,128,407	4.81	1.69	16.15
Individual level:				
Age	1,126,782	50	50	19

This table presents summary statistics for variables used in the analysis of the effect of the TCJA tax decrease on consumer credit outcomes. Column (1) presents the number of observations, column (2) presents the mean value, column (3) presents the median, and column (4) presents the standard deviation. The sample shown in the table consists of values from 2017 for all individuals included in the baseline CCP data sample (as included in the specification shown in Table 2). Age is tabulated for individuals with a non-missing age value in the sample. Individuals with a missing age are included with a separate dummy variable in the baseline specifications. Other variables are tabulated for all individuals included in the sample. Sample selection is described in Section III.I, and a detailed description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Bank of New York/Equifax Consumer Credit Panel; U.S. Census Bureau, American Community Survey; CRISM; Bureau of Labor Statistics, Local Area Unemployment Statistics; Bureau of Economic Analysis, Gross Domestic Product by State; IRS Individual Income Tax Statistics; NBER, TAXSIM; and authors' calculations.

SHED robustness appendix

Table A.5 presents a robustness analysis of how the estimated effect of TCJA tax reductions on the likelihood someone reports that they are living comfortably financially varies as we include various controls and sample restrictions. The first row reports the estimated coefficient and clustered standard error for the specification and sample indicated. The first five columns show coefficients as we progressively add more fine-grained controls, leading up to our preferred specification in column (5). Columns (6) and onward involve changing the sample by re-weighting, adding additional observations, or restricting to a sub-sample, including the sub-samples necessary to include different sets of controls.

The first five columns of Table A.5 show that the effect size decreases by more than half when we include individual fixed effects but is relatively stable as we include other, more fine-grained controls. Column (1) shows the estimated effect including only the tax reduction variable without any additional controls. When we do not include any controls, our estimate is that a 1 percentage point tax reduction leads to a 3.0 percentage points higher likelihood, which is double the magnitude of our preferred estimate in column (5). However, the coefficient decreases to 1.1 percentage points in column (2) when we include both individual and year fixed effects. The coefficient remains stable when we include state-by-year fixed effects in column (3) as well as an age cubic, an indicator for living in a metropolitan area, and an indicator for whether the person was working in column (4). The coefficient is slightly larger, but not detectably so, when we move to our preferred specification including a number of bins for household incomes in column (5).

Columns (6) through (9) of Table A.5 show that changes in the weighting and sample of observations included have small effects on the point estimates and do not change the qualitative results. Column (6) shows estimates using the baseline specification (Equation 1) in column (5) when we include the yearly cross-sectional survey weights. The weights are not included in the baseline specification because they are calculated for the entire cross-section in each year, whereas we use a sub-sample of the cross-section for this analysis. It is reassuring, however, that our results are similar when we include them, though with larger standard errors. Column (7) shows that we also find similar results when we include people in the top-most bin, where we cannot determine a midpoint. We also find very similar results in column (8), where we exclude people in the bottom-most income bin, where the percentage point tax reduction may be particularly sensitive to mis-measurement of income, since income is in the denominator of the tax reduction variable.[16] Column (9) shows that the results are also similar when we include more years of SHED data, covering from 2013 to 2020.

[16] We code household income at $500,000 when the top-most bin indicates a household income of more than $250,000. For the bottom-most bin, we take the midpoint of $2,500 between zero and the lowest bin of $5,000.

Column (10) shows results when we include twice-lagged controls as opposed to the contemporaneous controls in our specification, and column (11) shows results using contemporaneous controls, but restricting to the sample with data available to include twice-lagged controls in the specification. The estimated effects are very similar across columns (10) and (11). In our preferred specification, we include contemporaneous controls because the sparse nature of the SHED's panel component severely restricts our sample when we restrict to people who were also interviewed two years previously. We include twice-lagged controls to check that our main results still hold when we condition only on variables that are pre-determined before the tax cut, thus alleviating concerns that the so-called "bad controls" problem is biasing our results (i.e., that the TCJA tax reduction may be affecting the control variables directly, leading to bias). Column (10) shows that when we include these twice-lagged controls, our sample is less than one-third the size of the baseline sample and our estimated coefficient is much larger—4.1 percentage points as opposed to the baseline value of 1.5 percentage points. The coefficient in column (10) is also much less precisely estimated, given the much smaller sample—the standard error is roughly three times larger than for our preferred specification. To check whether the sample change is causing the differences or the controls change is causing the differences, column (11) estimates our preferred specification with the contemporaneous controls using the sub-sample of observations for which we have twice-lagged controls available. Estimating the preferred specification on this sub-sample results in a very similar estimate of the tax reduction effect to when we use twice-lagged controls, showing that the differences in the estimate are driven by the sample restriction and not the pre-treatment control variables.

Overall, Table A.5 shows that the estimated effect of tax reductions on the likelihood that someone reports living comfortably financially is positive and significant across a range of specifications and sub-samples. The effect size is much larger when we omit individual fixed effects. When we restrict to the roughly one-third of respondents for which we have enough observations to use (twice) lagged controls, the point estimate is larger, but also with a loss of precision. The larger point estimates appear to have more to do with the sample restriction imposed by requiring twice-lagged data, however, because coefficients are quite similar when we use the standard set of contemporaneous controls with this smaller sample.

Table A.5: Robustness of the effect on living comfortably

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Tax reduction (percentage points)	0.030***	0.011**	0.011**	0.012**	0.015***	0.014*	0.015***	0.015**	0.012***	0.041***	0.040***
	(0.0037)	(0.0042)	(0.0049)	(0.0050)	(0.0051)	(0.0076)	(0.0050)	(0.0057)	(0.0044)	(0.013)	(0.013)
Observations	13,056	13,056	13,056	13,056	13,056	13,056	13,858	12,654	15,290	4,461	4,461
Number of people		4,890	4,890	4,890	4,890	4,890	5,279	4,729	4,890	3,369	3,369
Individual fixed effects		X	X	X	X	X	X	X	X	X	X
Year fixed effects		X									
State-by-year fixed effects			X	X	X	X	X	X	X	X	X
Age cubic, metro, and working				X	X	X	X	X	X	X	X
Household income bins					X	X	X	X	X	X	X
Cross-sectional yearly weights						X					
Includes top-coded incomes							X				
Excludes bottom incomes								X			
Data from 2013 to 2020									X		
Twice-lagged controls										X	
Contemporaneous controls with twice-lagged control sample											X

This table presents a robustness analysis of the effect of the TCJA tax reduction on the likelihood of a household reporting they were living comfortably financially across a number of alternative specifications, compared with the baseline specification (Equation 1). Baseline results are presented in Table 1 and shown in column (5). Alternative specifications are described in detail in the SHED robustness appendix and include the controls listed in rows of the table. Unless otherwise noted, controls are from the same year as the observation and the regressions are unweighted. Unless otherwise specified, the regressions exclude both people who report having no family incomes and the top bin of family incomes where it is impossible to compute a midpoint. Across specifications, tax reductions increased the likelihood that someone said they were living comfortably. Effects are larger without individual fixed effects and within the sample of observations where we have twice-lagged controls available, both when using the twice-lagged controls and when using contemporaneous controls. Standard errors are clustered by state and are reported in parentheses. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Sample selection is described in Section III.I, and a detailed description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Board, SHED; CRISM; NBER, TAXSIM; and authors' calculations.

CCP robustness appendix

Table A.6 presents a robustness analysis of how the estimated effect of TCJA tax reductions on consumer credit-related outcomes varies under alternative specifications and under alternative methods for calculating the TCJA tax reduction.

We examine robustness for the three primary consumer credit outcomes of interest shown in rows of the table: the Equifax Risk Score, the number of new credit accounts, and the number of delinquencies reported that are 60 days past due to in severely derogatory status. The baseline specification results from Table 2 (estimated using Equation (2)) are presented in column (1) for comparison. The detailed construction of all variables is described in the Data Appendix.

In the baseline specification, we include a control for an individual's age bin as well as county-by-year and individual fixed effects. Columns (2) to (4) show that the results are generally similar using different variations of fixed effects and control variables. In the specification presented in column (2), which includes county-by-year and individual fixed effects, we also include zip-code-level deciles of total realized capital gains and ordinary dividends received in 2017 as additional control variables. These variables control for wealth levels at a hyperlocal level, which helps alleviate concerns that wealth effects could be driving our results. For example, one may have the concern that unobserved increases in equity market wealth related to the TCJA tax cuts could be causing a downtrend in delinquencies if households use wealth gains to stay current on debt service payments. In the specifications shown in columns (3) and (4), we present results for including less granular fixed effects. Column (3) presents results that include county and year fixed effects, and column (4) presents results including county and year fixed effects as well as a number of county-level economic controls (end-of-year unemployment rate, employment growth, and total wage growth), the state-level real GDP growth of the previous four quarters, and zip-code-level deciles of realized gains and ordinary dividends per return received in 2017. We observe that the results are little changed across these alternative specifications, suggesting that neither localized annual shocks nor differences in localized economic conditions are a key driver of our results.

Columns (5) to (7) show that the results are generally similar when employing alternative inputs for calculating the hyperlocal, representative TCJA tax reduction using the TAXSIM model, suggesting that our results are not especially sensitive to measurement of these inputs. All regressions in these columns are estimated using the baseline specification, Equation (2). Column (5) shows results using average county-level house prices in 2017 and state property tax rates to estimate property taxes paid, instead of the Census Bureau's estimates of median census-tract-level property taxes, and results are similar to those in the baseline. Column (6) shows results including an estimate of business income for individuals in the median income group at the zip-code level, as derived from the IRS Individual Income Tax Statistics report in 2017. The TCJA also made substantial changes to the treatment of business tax income for pass-through companies like S corporations and partnerships, in which business income is taxed under the personal tax code (Goodman et al., 2021). Including the business income estimates—which are most often zero for the median income group—we observe essentially identical results as in the baseline. This outcome suggests that the TCJA's changes to the tax treatment of business income for pass-through companies is not driving our results. Finally, one of the drawbacks of the CCP dataset is that several relevant inputs into an estimated tax rate calculation are unobservable, like the number of dependents. Therefore, in column (7), we present results for including a probability-weighted estimate of the tax cut incorporating dependents in a household based on an individual's age and assumed tax filing status, as described in detail in the Data Appendix. Though we observe some statistically significant, negative results for the Equifax Risk Score in this specification, the effect is economically modest. In addition, the results for the number of new accounts and for the number of delinquencies are little changed compared with the baseline, suggesting measurement error due to the inability to observe dependents in the CCP dataset is not meaningfully affecting these results.

Table A.6: Robustness of the effect on consumer credit outcomes

		Alternative specification			Alternative tax cut estimate		
	Baseline	County-by-year fixed effects and controls	County and year fixed effects	County and year fixed effects and controls	House-price-based property tax estimate	Including estimate of business income	Including estimate of dependents
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome Variables							
Equifax Risk Score	-0.0746	-0.0762	0.0259	0.0401	0.0077	-0.0746	-0.2804***
	[0.0692]	[0.0692]	[0.0835]	[0.0801]	[0.0710]	[0.0692]	[0.0728]
# New accounts	0.0305***	0.0305***	0.0285***	0.0285***	0.0284***	0.0305***	0.0321***
	[0.0022]	[0.0019]	[0.0022]	[0.0019]	[0.0021]	[0.0022]	[0.0023]
# Delinquencies	-0.0055***	-0.0055***	-0.0053***	-0.0048***	-0.0064***	-0.0055***	-0.0059***
	[0.0015]	[0.0015]	[0.0016]	[0.0016]	[0.0016]	[0.0015]	[0.0017]
County-by-year fixed effects	X	X			X	X	X
Age control	X	X	X	X	X	X	X
Geographic controls		X		X			
Year fixed effects			X	X			
County fixed effects			X	X			

This table presents a robustness analysis of the effect of the TCJA tax reduction on consumer credit variables across a number of alternative specifications, as compared with the baseline specification (Equation 2), and alternative methods for calculating the representative TCJA tax reduction. Results are presented for the three dependent variables shown in rows: the Equifax Risk Score, the number of new accounts and the number of accounts that are 60 days delinquent to in severely derogatory status. Baseline results are presented in Table 2 and in column (1). Alternative specifications and methods for calculating the tax reduction are described in detail in the CCP robustness appendix. We find that the results for our preferred specification are similar in magnitude and significance to those for almost all of the alternatives shown. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Sample selection is described in Section III.I, and a detailed description for all variables is provided in the Data Appendix. Data sources are Federal Reserve Bank of New York/Equifax Consumer Credit Panel; U.S. Census Bureau, American Community Survey; CRISM; Bureau of Labor Statistics, Local Area Unemployment Statistics; Bureau of Economic Analysis, Gross Domestic Product by State ; IRS Individual Income Tax Statistics; NBER, TAXSIM; and authors' calculations.

Data Appendix

Survey of Household Economics and Decisionmaking

The Survey of Household Economics and Decisionmaking (SHED) has been conducted annually in the fourth quarter of the year since 2013. The survey was designed by Federal Reserve Board staff and fielded by Ipsos, a private firm focused on consumer research. Survey respondents are members of Ipsos's KnowledgePanel, and are selected into that panel using address-based sampling. The surveys are administered online, and potential respondents who do not have internet access are provided with a device with an internet connection that will allow them to take surveys. Ipsos Group S.A. (n.d.) provides more information on the KnowledgePanel's construction and methodology.

Response rates for the SHED are relatively high for an online panel. The lowest response rate is after initial contact, which Ipsos estimates at 13 percent in 2018. From here, 64 percent complete an initial survey and 54 percent of those respondents completed the SHED. Since drop-offs in each stage compound, that fact implies a relatively low response rate moving through each stage—4.3 percent. While the specific figures here apply to 2018 and come from Federal Reserve Board (2019), they are similar in other years. Previous analyses of statistics drawn from the SHED have shown that they often correspond to statistics from comparable questions drawn from surveys with substantially higher response rates from first contact. Larrimore, Schmeiser and Devlin-Foltz (2015) and Federal Reserve Board (2016) shows that several statistics from the SHED match those in the Census Bureau's Current Population Survey, American Community Survey (ACS), and Survey of Income and Program Participation. This result suggests that the various measures used to retain a representative online panel sample limit the level of bias due to unobservable differences.

Ipsos and the Federal Reserve take several steps to improve the representativeness of the online panel. From 2014 to 2017, the survey explicitly included an oversample of people with household incomes below $40,000 per year alongside an additional sample of people who had completed the survey in the previous year. From 2018 onward, Ipsos gave larger incentive payments as well as more follow up emails to people who belonged to target groups with lower response rates—adults aged 18 to 20, adults with less than a high school degree, and adults who are either Hispanic or non-White.[17] In addition to benefiting from efforts to improve response rates, the survey includes post-stratification weights designed to make the samples in various years nationally representative. We include these weights as a robustness check and find similar results. We do not include these weights in our baseline analysis, however, since they do not apply to our analysis sample of people

[17] More details on the specific response rates and survey frames in each year are available in the survey reports for each year on the Federal Reserve Board's website. See, for example, Federal Reserve Board (2016, 2018, 2019, 2020, 2021).

who are interviewed in multiple years.

Sample construction

Our SHED analysis sample is a panel of respondents who are interviewed across multiple years. We can include people across multiple years because of two features of the sample design. The first reason we can observe people after the tax decrease is that the main sample in each year is drawn from continuing respondents in the KnowledgePanel. Since the SHED has a relatively large sample that includes a substantial share of the KnowledgePanel in each year, a large number of respondents who are randomly drawn from the KnowledgePanel in a current year were also randomly drawn in one or more previous years. The second reason is that the sampling frame included an explicit sample of people who responded in the previous years in 2014 through 2017.[18]

We can link 56 percent of observations from the SHED waves of 2015 to 2019 to the same person in another year of the SHED. However the number decreases to 30 percent when we restrict to observing them both before and after the tax change. Table A.2 gives the number of observations in each year's survey along with a breakdown of categories of responses we need to exclude from the analysis. Linking is done based on a respondent's being a member of the repeat panel in 2016 or 2017, which implies that the unique identifier of the person in that previous year was the same as in that year. Datasets in 2018 and 2019 also include a variable for the unique identifier for the person in previous years if they were a respondent. It may be possible to link more observations with additional data. Note that most observations are not linked because they are not sampled, not because of non-response.

Table A.2 also presents the other reasons why people are excluded from the analysis sample, including survey respondents providing insufficient information to calculate tax changes, top-coded household incomes, and item non-response in variables we use in the regression but not the tax calculation. The final data sample includes 13,056 observations, or 27 percent of the cross-sectional observations from 2015 to 2019.

SHED outcomes

Financial well-being. Financial well-being is coded as responses to the following question (number B2): "Overall, which one of the following best describes how well you are managing financially these days?" Respondents can answer "living comfortably," "doing okay," "just getting by," or "finding it difficult to get by." Since the responses are ordinal, we present regression results in terms of the share of people who are doing at least as well as a given category. For example, in the

[18]Note that while each feature makes it more likely that people will be interviewed in consecutive years, we do not impose this restriction. So some people are re-interviewed after several years.

variable for doing okay or better, we include people who say they are living comfortably alongside people who say they are doing okay.

The financial well-being question is a major focus of the SHED release each year (Federal Reserve Board, 2020) because it gives insights into how people subjectively feel about their finances. Methodologically it resembles attempts to examine overall well-being—for example overall life satisfaction in the Gallup World Poll (Deaton, 2008)—in that it asks people to say where they stand in a hierarchy of categories of financial well-being. The question also gives similar results to more involved measures. In 2017 and 2020, the SHED also included a financial well-being scale developed by the Consumer Financial Protection Bureau (CFPB; Consumer Financial Protection Bureau, 2017), and answers to each measure showed similar trends (Federal Reserve Board, 2018, 2021).

Other measures of personal finances. In addition to overall financial well-being, the SHED asks extensive questions about specific parts of people's finances. In keeping with the more subjective measures of overall financial well-being, we also include several somewhat subjective concepts that are similar to questions used in the CFPB's financial well-being scale. These items include the following:

- *Would handle $400*: Would cover a $400 expense with cash or a near equivalent.(Sherter, 2019) This measure is commonly employed to assess the financial security of families.[19]

We also include arguably less subjective measures about the respondent:

- *Emergency fund*: Had an emergency fund that would cover their expenses for three months
- *Student loans*: Had a student loan
- *Homeowner*: Owned their home

Administrative measures from consumer credit reports in the CCP analysis also provide less subjective measures of people's finances.

Computing tax changes

The SHED has the advantage of providing many of the main inputs necessary to identify average tax rates, and we use them to compute implied changes in taxes using the NBER TAXSIM tax microsimulation model (Feenberg and Coutts, 1993). Specifically, we compute tax rates based on information obtained in the latest year available before the tax law was implemented so as to avoid

[19] A comparison of the implications of this question with measures obtained from the Survey of Consumer Finances is in Bhutta and Dettling (2018) and Box 3 of Federal Reserve Board (2020).

any possible behavioral effects of the tax law changes. For our main specification, we compare average tax rates in 2017, before the law was implemented, with average tax rates in 2018, after the law was implemented. Since we are comparing someone with the same characteristics before and after, the differences are due to changes in the tax law from 2017 to 2018, which we interpret as the effect of the TCJA.[20]

The SHED itself, as well as the other surveys given to the Ipsos KnowledgePanel, provide the bulk of our inputs into TAXSIM that identify the size of tax decreases. They include the following:

- *Total family income, TAXSIM variable* `pwages`: We use income as reported in a "panel variable" that asked all members of the Ipsos KnowledgePanel to report their household income. The variable is named `ppincimp` in the dataset. We use the midpoint of each income category. We generally exclude the top category, which is "$250,000 or higher" in 2017. Where it is included we arbitrarily use $500,000. Since we are unable to identify different types of income, we assume that all of the family's income is wage income.

- *Filing status, TAXSIM variable* `mstat`: We use an Ipsos KnowledgePanel variable giving marital status, `ppmarit`.

- *Number of dependents, TAXSIM variables* `depx`, `dep13`*,* `dep17`*, and* `dep18`: We use the number of individuals reported as living in the household of various ages under 18. The number of people in the household of various ages is drawn from "panel variables" asked of all members of the Ipsos KnowledgePanel, not the equivalent questions in the SHED itself. We include an additional adult dependent if the person reports living with extended family, parents, or a friend and report that they do so to care for that individual or group of individuals in the main survey.

- *Mortgage interest payments, TAXSIM variable* `mortgage`: To calculate an estimate of annual mortgage interest payments, we use question M4, which asks people who own their homes with a mortgage to report the range of their total monthly mortgage payment. To calculate the interest share of the total mortgage payment, we use the Equifax Credit Risk Insight Servicing and Black Knight McDash (CRISM) dataset for 2017:Q4 to calculate the median interest payment share of mortgage payments by census tract. For census tracts with fewer than 10 observations, we use the county median interest payment share.

- *State of residence, TAXSIM variable* `state`: We use the individual's reported state of residence according to the Ipsos KnowledgePanel.

[20]Coding was done through a submission to TAXSIM version 32, available at: https://users.nber.org/ taxsim/taxsim32/.

- *Property tax value estimation, TAXSIM variable* `proptax`: For property tax estimates, we use census-tract-level data from the U.S. Census Bureau's American Community Survey in 2017 on median real estate taxes paid with a mortgage (variable name `HD01_VD03`) and median real estate taxes paid without a mortgage (variable name `HD01_VD04`). For missing tracts, we use median county-level data, and we use median state-level data if county-level data are missing.

From TAXSIM output, we calculate an individual's average tax rate under 2017 tax law as the sum of federal and state income tax liability divided by the individual's income (using the TAXSIM variable names: (fiitax + siitax)/pwages). We calculate an individual's average tax rate under the 2018 tax code in the equivalent way. Then, for each individual, we define the TCJA tax rate reduction as -1*(2018 tax rate – 2017 tax rate).

Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP)

The CCP is an individual-level, anonymized panel dataset of consumer credit records, drawn at the end of each quarter from Equifax—one of the three major credit bureaus in the United States. The primary CCP data sample consists of a 5 percent random sample of all U.S. individuals with Social Security numbers and credit records, and each quarter, the panel is updated as new individuals establish credit records. Once an individual establishes a credit history and enters the sample, they remain in the sample continuously, whether or not they have credit activity in a particular quarter, until death. The data include detailed information drawn from credit reports, including loan balances, credit limits, and payment status. Aside from variables on age and geographic location, the dataset is generally limited to information about credit status. The CCP does not contain information, for example, about household income, employment status, or demographic characteristics like race and education level. Additional information about the dataset, including sampling and methodology, is available in Lee and Van der Klaauw (2010).

Sample construction

To draw our sample of individuals' credit histories, we use a 10 percent sample of the overall CCP dataset and include the years 2015 to 2019—three years before, to two years after, the TCJA enactment. We use end-of-year observations to smooth through fluctuations in financial positions over the course of the year and for consistency with the annual nature and timing of the SHED, which results in a total of 6,598,370 observations in the dataset. To be included in the sample, an individual is required to be in the dataset in 2017—the year before the TCJA implementation—as well as in 2018 or 2019, to have a reported Equifax Risk Score (a type of credit score), and to have a reported value for the number of new accounts established in a year. These requirements limit the sample to 5,513,155 observations.

CCP outcomes

We study five outcomes from the CCP:

- *Equifax Risk Score*: An individual's credit score
- *# New accounts*: The number of accounts opened within 12 months
- *# Delinquencies*: The sum of the number of accounts that have been reported 60, 90, and 120 days past due plus the number of accounts that have been reported in severely derogatory status
- *ln(Consumer credit balances)*: The natural log of total outstanding account balances minus mortgage account balances
- *ln(Consumer credit plus mortgage balances)*: The natural log of total outstanding account balances

Computing tax changes

The CCP dataset does not include information on household income, tax filing status, or other data necessary to identify an individual's average tax rate change following the TCJA. Therefore, we calculate hyperlocal (i.e., census-tract-level), representative changes in effective personal tax rates following the TCJA to study how the tax changes affected household finances. Using 2017 data as inputs into the NBER's TAXSIM model of tax liabilities, we calculate the representative average tax rate (federal plus state taxes) under 2017 tax law and under 2018 tax law. As in the SHED analysis, we define the TCJA tax rate reduction as -1*(2018 tax rate – 2017 tax rate).

For each census tract observed in the CCP dataset in 2017, we calculate four representative tax rate changes: for single filers with a mortgage, for single filers without a mortgage, for joint filers with a mortgage, and for joint filers without a mortgage. We then assign the relevant tax rate change to an individual in the CCP based on the census tract in which they lived in 2017:Q4, whether we attribute joint or single filing status to them in 2017:Q4, and whether we observe them holding an outstanding mortgage loan in 2017:Q4. All calculations were done using TAXSIM version 32.

Using the TAXSIM inputs described in the next paragraph, we calculate the representative average federal plus state tax rates under 2017 tax law and under 2018 tax law. We calculate the total average tax rate change given the substantial interactions between federal and state tax codes.

The tax rate for each year is calculated as (Federal individual income tax liability (fiitax) + State individual income tax liability (siitax))/(Wage and salary income of primary taxpayer (pwages)).

We describe the inputs into the TAXSIM model in detail:

- *Filing status, TAXSIM variable* `mstat`: To assign single or joint tax filing status to individuals in the CCP, we use information from a supplemental CCP data sample of individuals who live at the same address as people included in the primary data sample. For households of more than one individual with a credit record in the CCP but fewer than five individuals, we assign a person "married filing jointly" filing status if the age difference between any two members of the household is smaller than 15 years. For households of more than four individuals with a credit record, we assign "single" filing status, as these individuals are more likely to live in multi-unit buildings.

- *Income measures, TAXSIM variable* `pwages`: As measures of income by census tract, we use data from the U.S. Census Bureau's American Community Survey from 2017. As our measure of income for joint filers, we use married-couple family median income (variable name `HC03_EST_VC13`). As our measure of income for single filers, we use median earnings for workers (variable name `HC01_VC124`).

- *Mortgage interest calculation, TAXSIM variable* `mortgage`: Using the CRISM dataset for 2017:Q4, we calculate an estimate of annual mortgage interest payments for each individual by multiplying the interest rate and mortgage balance, taking the median by zip code, and mapping zip codes to census tracts using a crosswalk provided by the Department of Housing and Urban Development.[21] We exclude zip codes with fewer than 10 observations. For missing tracts, we use median county-level data, and we use median state-level data if county-level data are missing.

- *Property tax value estimation, TAXSIM variable* `proptax`: For property tax estimates, we use census-tract-level data from the U.S. Census Bureau's American Community Survey in 2017 on median real estate taxes paid with a mortgage (variable name `HD01_VD03`) and median real estate taxes paid without a mortgage (variable name `HD01_VD04`). For missing tracts, we use median county-level data, and we use median state-level data if county-level data are missing.

- *State of residence, TAXSIM variable* `state`: We use the individual's state of residence reported in the CCP as of 2017:Q4.

[21] The crosswalks are available on the Department of Housing and Urban Development's website at https://www.huduser.gov/portal/datasets/usps_crosswalk.html.

Computing tax changes (alternative methods for robustness)

We also use several alternative methods to compute the hyperlocal, representative TCJA income tax changes to assess the robustness of our primary measure:

- *Alternative property tax calculation*: To calculate an alternative estimate of the census-tract median property tax, we multiply the state property tax rate sourced from Tax-Rates.org by the census-tract home value median from the "Selected Housing Characteristics" table in the U.S. Census Bureau's American Community Survey. All other TAXSIM inputs remain the same.

- *Incorporating business income data into the tax change estimate, TAXSIM variable* `pbusinc`: To incorporate an estimate of business income into the TCJA tax rate change calculation, we calculate median business income per return in a zip code for an individual in the median income group in the zip code using data from the IRS Individual Income Tax Statistics in 2017. All other TAXSIM inputs remain the same.

- *Incorporating dependents into the tax change estimate*: We calculate an estimate of the TCJA tax rate change including dependents using data on the number of children and the fraction of households with children from the Census Bureau's 2017 report "America's Families and Living Arrangements." While households may have non-child dependents, we focus on measuring the number of children as dependents for this analysis.

  Since we cannot observe whether someone has dependents, we calculate an expected tax decrease by computing a weighted average of expected tax reductions conditional on having and not having dependents. To construct this variable, denoted $\text{Tax decrease}^{d}_{jafm}$, we begin by computing an expected tax decrease conditional on having dependents, $\text{Tax decrease}^{D=2}_{jfm}$. The relevant "with dependents" tax decrease is based on the census tract in which the person lived in 2017:Q4 (j), whether we attribute joint or single filing status to them in 2017:Q4 (f), and whether we observe them holding an outstanding mortgage loan in 2017:Q4 (m). Since the average number of children per parent is the same for married and single filers as well as for individuals with and without a mortgage, we uniformly assign the 2017 national average of two children as dependents ($D = 2$; TAXSIM variable `depx`). The weights applied to this variable are denoted by κ_{af} and are based on the fraction of individuals with children by age, for single households and married households from the Census Bureau. The other component, $\text{Tax decrease}^{D=0}_{jfm}$, is the estimate of the tax decrease conditional on not having dependents ($D = 0$), based on the person's census tract, filing status, and mortgage status.[22]

[22]Note that this latter tax decrease, $\text{Tax decrease}^{D=0}_{jfm}$, is the same as the tax decrease used in the main text, Tax decrease_i.

In equation form:

$$\text{Tax decrease}^{d}_{jafm} = \kappa_{af} * \text{Tax decrease}^{D=2}_{jfm} + (1 - \kappa_{af}) * \text{Tax decrease}^{D=0}_{jfm}$$

Data sources and definitions for control variables

- *State GDP*: Four-quarter percent change in state gross domestic product, from the Bureau of Economic Analysis
- *Unemployment rate*: County-level unemployment rate for December from the Bureau of Labor Statistics Local Area Unemployment Statistics report
- *Employment growth*: Twelve-month percent change in total county-level employment from the Bureau of Labor Statistics Local Area Unemployment Statistics report
- *Wage growth*: Four-quarter percent change in the county-level average weekly wage from the Bureau of Labor Statistics Quarterly Census of Employment and Wages report
- *Ordinary dividends*: Total ordinary dividends per zip code in 2017, from the IRS Individual Income Tax Statistics
- *Realized capital gains*: oh tht
- *Age*: We calculate age by subtracting birth year in the CCP dataset from sample year, and we generate 13 age bins for the following age groups: 18 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44, 45 to 49, 50 to 54, 55 to 59, 60 to 64, 65 to 69, 70 to 74, 75 to 79, and 80 or older. We also include an age bin for observations without a birth year