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ABSTRACT

The Tax Cuts and Jobs Act eliminated federal charitable giving incentives for roughly 20 percent of US income-tax payers. We study the impact of this on giving. Basic theory and our empirical results suggest heterogeneous effects for taxpayers with different amounts of itemizable expenses. Overall, the reform decreased charitable giving by about $20 billion annually. Using a new method to adjust estimates for retimed giving, we find evidence of moderate intertemporal shifts from pre-announcement of the law. The permanent price elasticity of giving estimates range from .6 for the average donor to over 2 for those predicted to be most responsive to the reform.

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1. Introduction

Governments use tax policy not only to directly finance collective goods, but also to incentivize their provision through voluntary contributions. Policies include allowing taxpayers to deduct charitable giving from their taxable income, providing tax credits for donations, and offering a third-party match for gifts (Charities Aid Foundation, 2016; OECD, 2020). In the United States, where giving represents about 2% of GDP, the charitable deduction involves hundreds of billions of dollars in donations and tens of billions of dollars in tax revenue each year.

The efficacy of these policies depends upon how responsive donors are to financial incentives, which ultimately is an empirical question. But despite more than 100 papers over the past half century, there is nothing resembling consensus on this question. The literature routinely produces highly divergent estimates of donor responsiveness to incentives to give.[1]

This literature faces several problems. First, in papers using the charitable deduction to estimate donor responsiveness, the source of identification is variation in marginal tax rates. Policy-induced variation in marginal tax rates, however, is usually small. Equally problematic, taxpayer-specific variation in income also induces variation in marginal tax rates, confounding attempts to identify policy effects on giving. To circumvent these problems, research over the past twenty years has largely abandoned investigation of US tax policy, and instead turned to alternative approaches to identification by using tax credits and matches to estimate donor responsiveness; an implicit assumption in this literature is that estimates of responsiveness based on credits and matches apply to responsiveness to a charitable deduction.

Tax credit papers offer a source of identification not confounded with income variation, but have had to focus on a small fraction of taxpayers (Fack and Landais, 2010), relatively

[1] For example a reader of the papers investigating tax policies and giving could conclude that giving is price elastic (Auten, Seig, and Clotfelter, 2002; Bakija and Heim, 2011; Duquette, 2016; Hickey, Minaker, Payne, Roberts, and Smith, 2023), or inelastic (Randolph, 1995; Barrett, McGuirk, and Steinberg, 1997; Almunia, Guceri, Lockwood, and Scharf, 2020; Fack and Landais, 2010; Hungerman and Ottoni-Wilhelm, 2021).

small policy variation (Hickey, Minaker, Payne, Roberts, and Smith, 2023), or a particular type of giving (Hungerman and Ottoni-Wilhelm, 2021). Matching papers, by using lab and field experiments, offer strong identification, but Hungerman and Ottoni-Wilhelm (2021) prove that matches and tax-based rebate incentives (such as from a deduction) in theory have different effects on giving. Furthermore, a seminal paper in the matching literature (Karlan and List, 2007) demonstrates that responses to incentives can depend greatly on context, so that the applicability of results from small-scale studies for the context created by an economy-wide tax policy change are unclear.

A final problem is that large tax policy changes are announced and debated prior to enactment. A pre-announced tax policy change will give taxpayers an incentive to intertemporally shift their giving. Intertemporal shifting confounds estimation of permanent policy effects, a problem first pointed out by Clotfelter (1990) and Auten, Cilke, and Randolph (1992), and well-known since Randolph (1995). There is no accepted solution to this problem.[2]

In this paper we present estimates of donor responsiveness to a charitable deduction-based incentive that address these problems. The estimates are based on changes effected by the Tax Cut and Jobs Act of 2017. TCJA was a three trillion dollar policy, and created the largest change in US giving incentives in a generation. Starting in 2018, TCJA induced an extremely large group of taxpayers—roughly 20% of all households—to use the standard deduction instead of itemizing. These taxpayers thus lost their US-tax-code incentive to give. Our first contribution is to exploit the fact that this reform created large changes in incentives even for taxpayers who experienced little change in income and marginal tax rates. This allows a study of a shock in the US tax code that addresses both identification problems inherent in studying deductibility.

[2]Tax policy work on intertemporal shifting has reached contrasting conclusions—–remarkably so, given that papers have studied similar data and policy shocks. Auten, Seig, and Clotfelter (1999, 2002) find only weak evidence of temporarily increased giving around the Economic Recovery Tax Act of 1981, but no evidence around the Tax Reform Act of 1986. Explicitly modeling giving as a function of future prices (Bakija and Heim, 2011) produces permanent price elasticity estimates close to those in Auten, Seig, and Clotfelter (2002). In contrast, Randolph (1995) finds large shifting effects. Other work investigating a non-tax context finds clear evidence of intertemporal shifting (Scharf, Smith, and Ottoni-Wilhelm, 2022).

Second, we demonstrate theoretically that the effects of TCJA on giving are heterogeneous across taxpayers, determined by their pre-TCJA level of itemized deductions. For some taxpayers the predominate TCJA effect is a large increase in post-tax income, which could increase giving. But taxpayers whose pre-TCJA deductions were close to the new TCJA standard deduction experienced TCJA as a compensated price increase. For these taxpayers we can estimate a compensated price elasticity. Knowing the compensated price elasticity obviously is important for optimal tax policy and welfare analysis. This estimate is the first of a compensated elasticity for total charitable giving.[3]

Third, we introduce a new approach, based on the models in Auten, Seig, and Clotfelter (2002) and Bakija and Heim (2011), to address the intertemporal shifting confound to estimates of permanent price elasticities. The central intuition is that intertemporal shifting generates autocorrelation between anticipatory spikes and subsequent drops in giving. The new approach is easy to implement, and applicable in other contexts in which a policy change is pre-announced.

We estimate the effect of TCJA on giving using data from the *Panel Study of Income Dynamics*. The data describe 4,232 Americans over 2000-2018. Our approach to identification is to compare the change in giving among taxpayers predicted to stop itemizing as a result of TCJA to the change in giving among taxpayers not predicted to switch itemization status.[4] The policy effect can be identified while using extensive controls to ensure that taxpayer-specific variation in income does not confound the estimates.

The baseline intention-to-treat estimate is that TCJA caused giving to drop by $366 per taxpayer predicted to switch. The treatment-effect-on-the-treated estimate suggests an $880 drop in giving per taxpayer who actually switched. The policy decreased charitable giving by about $20 billion annually relative to trend; this result suggests the entire observed drop in aggregate giving in 2018 was due to TCJA.

[3] The one previous estimate of a compensated price elasticity is for giving to colleges and universities (Hungerman and Ottoni-Wilhelm, 2021).

[4] Note that IRS data, which measure giving for taxpayers only when they itemize, cannot be used to investigate the effects of a policy like TCJA that induced taxpayers to stop itemizing.

Applying the new approach to intertemporal shifting using different datasets and assumptions all produce a similar conclusion: about 20 percent of the post-TCJA drop in giving reflects gifts shifted intertemporally. After accounting for intertemporal shifting, the estimates imply a permanent price elasticity of .6. Furthermore, there are heterogeneous responses across taxpayers in line with theoretical predictions. For taxpayers facing relatively large positive income effects from the law, we find insignificant or in some cases positive effects of TCJA on giving. The overall decrease in giving is driven by those predicted to experience TCJA as a compensated price increase; for these taxpayers the compensated price elasticity is over 2.

As a fourth contribution, we break down the results by type of charitable donation.[5] The results indicate policy-relevant heterogeneous responses across different types of giving. TCJA caused little change in giving to religious congregations. Instead, the decrease was almost entirely in giving to organizations with other purposes, and among them a large portion of the drop was to organizations that help people in need.

Finally, the results have an important implication for interpreting the divergence of donor responsiveness estimates in the literature. The present results uncover significant heterogeneity in responsiveness across taxpayers and across types of nonprofit organizations using a single dataset, one policy shock, and a single methodological approach. Divergent estimates on the responsiveness of donors can represent true heterogeneity. We return to this implication in the conclusion.

We next discuss the TCJA in more detail. Sections 3 and 4 discuss the PSID and our empirical methodology. In Section 5 we present the empirical results, our retiming adjustment, and compensated elasticity estimates; the last section concludes.

[5] This too can not be done with IRS data. Nor is it possible in many tax credit contexts where incentives often are limited to a certain type of giving.

2. Background on the Tax Cuts and Jobs Act

The TCJA was introduced in Congress on November 2, 2017, passed on December 20, and signed into law on December 22. Implementation began in 2018. Along with changes to corporate income tax, the tax treatment of multinational firms, and certain excise taxes, the law made important changes to individual income tax.[6] Most notably, the law greatly expanded the standard deduction amount for all filers. Between 2017 and 2018, the standard deduction increased from $6,350 to $12,000 for single filers, from $9,350 to $18,000 for head-of-household filers, and from $12,700 to $24,000 for joint filers. (Appendix table A1 lists standard deduction values for different filers during the 2010s). The government additionally placed a $10,000 cap on the deduction of state and local taxes (SALT). As a result, about 20% of all taxpayers switched from itemizing their deductions to taking the standard deduction and consequently lost federal-tax-based incentives to give, regardless of their income or their marginal tax rates (McClelland, 2022). This was the largest change in US giving incentives since the Tax Reform Act of 1986. The potential effect of TCJA on giving was a prominent part of the discussion of the law prior to its passage (Husock, 2022).[7]

The implications of TCJA's change in the standard deduction are illustrated in figure 1. The figure depicts a taxpayer with pre-tax income M and income tax rate of t who chooses a level of charitable giving g and other consumption c. The solid line shows the taxpayer's consumption if they choose to itemize their giving; in this case each dollar spent on giving results in a fall in consumption of 1-t. Suppose that the standard deduction initially is at level s_0. If the standard deduction increases to s_1, the non-itemizer budget line shifts from

[6] For discussion of the TCJA beyond individual income tax, see Gale et al. (2018).

[7] Before Congress passed TCJA, legislation had been proposed to allow charitable giving to be deducted by all. The Universal Charitable Giving Act of 2017 (H.R.3988) was introduced October 5, 2017 (https://www.congress.gov/bill/115th-congress/house-bill/3988). The bill would have placed charitable deductions "above the line" with a cap/ceiling at one-third of the taxpayer's standard deduction. The Universal Charitable Giving Act was re-proposed in 2019 (H.R.5293) as was the similar Charitable Giving Tax Deduction Act (H.R. 651). In 2020, the Coronavirus Aid, Relief, and Economic Security Act included above-the-line deductibility, but only temporarily and with a very low cap ($300 single, $600 married-joint for 2021). The Charity 2022 Act (H.R.6490) proposed extending the CARE Act's above-the-line deductibility to 2022, but was not passed.

the inner dashed line to the outer dashed line.

The figure indicates that an increase in the standard deduction will work as both a price increase and an income effect for taxpayers, and that the relative magnitudes of these effects will differ depending on the taxpayer's pre-TCJA itemized giving. For taxpayers whose pre-TCJA giving placed them near A_0, TCJA effected a price increase but also a fairly large net-of-tax income increase, so giving could either decrease or increase as a result of the increased standard deduction.[8,9]

This is in contrast with an itemizer initially at bundle A. There are three noteworthy observations here. First, there is no ambiguity: the taxpayer will switch to the standard deduction after TCJA and give less (bundle B). Second, those at A identify the effect of a policy that eliminates itemization while holding taxable income constant. Note this would be revenue netural in a naive sense, as it would ignore any behavioral response. This raises a third observation: if one were to undertake a *utility* compensated price increase at bundle A, the resulting new bundle would be bundle C. For moderate income effects, bundles B and C should be sufficiently close that an estimate of the A-to-B response would uncover a compensated price elasticity of demand for giving.

In response to TCJA, taxpayers who were donating more than bundle A might either not change their behavior, or take the standard deduction and donate less. Overall, the figure makes clear that the effect of the TCJA is heterogeneous depending upon taxpayers' pre-TCJA level of giving, and more generally their pre-TCJA level of iemtized deductions.

Figure 1 focuses on the TCJA-induced change in the standard deduction, and is static. However, the methods we use will include other TCJA changes, and also will adjust for dynamic intertemporal shifting as taxpayers may have shifted into 2017 giving they had planned to do in 2018. A final issue not captured by figure 1 is the fact that tax returns

[8] For a married couple at A_0 facing a 24% tax rate, the net-of-tax income increase would be about $2,500, so even a modest propensity to donate out of income could lead to an increase in giving.

[9] Note the TCJA income effect is different from a policy that would entirely eliminate the deductability of giving, but compensate the taxpayer so they could still obtain A_0; in this case, the giving of an itemizer at A_0 would unambiguously go down.

only record giving for those who itemize: whether giving is observed by the IRS depends upon which budget line the taxpayer is on. Point B will never be observed in IRS data. We circumvent this problem by using the PSID.

3. Data

To estimate the effect of the TCJA we use the PSID's ten biennial interviews from 2001-2019. Each interview year measures the previous tax year's charitable giving, hence the even years 2000-2018.[10] Giving for religious purposes is measured with a question about giving to churches, synagogues, mosques, and TV/radio ministries; we refer to this as giving to "congregations." Giving to nine charitable purposes, such as helping people with basic needs, arts and culture, the environment, etc. are separately queried; aggregating the nine together we refer to this as giving to "other charitable organizations."[11] Most of the results to be presented use the aggregation of giving to congregations plus giving to other charitable organizations. We refer to this aggregation as "giving;" it is comparable to IRS charitable deductions.

The advantages of the PSID for determining TCJA effects on giving are two-fold. First, giving is measured for both itemizers and non-itemizers. Second, giving can be disaggregated into its different purposes. This allows us to determine whether the TCJA effects on giving are evenly spread across giving purposes, or if the effects fell disproportionately on some purposes rather than others. IRS data cannot track the giving of people who switched from itemizing to not; neither can IRS data be used to investigate disproportionate effects across charitable purposes.[12]

[10] When it is clearer to refer to the *interview* year in which the data were collected, we will refer to the interview year (e.g., 2019). When it is clearer to refer to the corresponding *tax* year (e.g., 2018), we will refer to the tax year.

[11] The other six purposes are organizations that serve a combination of purposes (like the United Way), health, education, youth/family services, neighborhoods/community, and international aid and relief. There also is an open-ended "other" category.

[12] When the Office of Tax Analysis evaluates a policy proposal involving the giving of non-itemizers, they use the PSID (e.g., Ackerman and Auten, 2006). CBO estimates of the distribution of giving across purposes

To build our analysis sample we begin with the PSID's nationally representative "Survey Research Center" (SRC) sample of N = 4,715 reference persons in the 2017 interview year who were also reference persons (or spouse of a reference person) in the 2019 interview. The 2017 and 2019 interview years (tax years 2016 and 2018) are necessary to construct the intention-to-treat variable "predicted-to-switch" (described in Section 4 below), and to measure giving before and after the TCJA. We drop N = 327 who began, or ended, a marital or cohabiting relationship between the 2017 and 2019 interview years, because their central family structure changed coincident with the TCJA. We drop N = 127 whose income or marginal tax rate in the 2016 were negative (i.e., just before TCJA they received refundable credits from the government), one whose intention-to-treat variable predicted they would switch from not-itemizing to itemizing because of the TCJA (a defier); and N = 28 who in any year gave more than 50 percent of adjusted gross income.[13] The analysis sample is N = 4,232.

Our main analyses focus on the post-Great Recession years (interview years 2011-2019). During this period there are $N * T$ = 18,509 person-year observations. Table 1 presents summary statistics. Average giving is \$1,670, just under two percent of average income (\$92,762). Just over one fifth of the person-year observations are from N = 882 who were predicted-to-switch by the TCJA.

The distribution of giving measured in the PSID among itemizers matches the IRS distribution of itemized charitable deductions up to the 90$^{\text{th}}$ percentile (Wilhelm, 2006). The reason the PSID does not match the top ten percent is because nationally-representative samples do not pick up large numbers of very rich people.[14] For the purposes of our investigation this is not a problem for the sample size of taxpayers predicted-to-switch because

rely on the PSID (Congressional Budget Office, 2011).

[13]For these N = 28 not all giving can be deducted but the excess can be carried forward. Because carried-over amounts can be deducted in any of the subsequent five years, to include these we would have to make ad hoc assumptions about the years into which they carried forward the amounts, and which tax incentives were relevant.

[14]For example, the PSID's measurement of wealth matches the wealth distribution from the Survey of Consumer Finance (the SCF has a high-income oversample) until about the 92$^{\text{nd}}$ percentile (Juster, Smith, and Stafford, 1999).

they are middle to upper-middle class. However, it does imply that the comparison group of (typically high income) taxpayers that itemize prior to TCJA and are predicted to continue itemizing is of modest size ($N = 302$). The comparison group of taxpayers not itemizing before TCJA is much larger ($N = 3{,}048$).

4. Methodology

Our approach focuses on taxpayers who because of TCJA are predicted to switch from itemizing expenses to taking the standard deduction. To identify those predicted to switch, we use tax year 2016 data (the final pre-reform year of data) to predict 2018 itemization in a counter-factual world where TCJA was never enacted. When doing so, we adjust the data for inflation, allowing for nominal income growth from 2016 to 2018. We then use the inflation-adjusted 2016 data to predict itemization status under TCJA.[15] In both scenarios we use "first dollar" giving to account for the fact that giving itself can influence the itemization decision. The two predictions of itemization use the same inputs, so that different predictions will be driven entirely by changes from TCJA.

Our predictions under each scenario use the TAXSIM software hosted by the National Bureau of Economic Research (Feenberg and Coutts, 1993).[16] This allows us to account for changes in itemization beyond just the change in the standard deduction such as changes in SALT deductability.[17] Let *switcher* be a dummy variable denoting a taxpayer predicted to switch from itemization to non-itemization as a result of TCJA. We begin by estimating:

$$g_{it} = \alpha + \beta\ switcher_i \times post_t + \gamma switcher_i + \lambda_1 f(income_{it},\ mtr_{it}) + \delta X_{it} + Y_t + R_i + \epsilon_{it} \quad (1)$$

[15] The use of predicted status avoids endogeneity issues related to using actual itemization status (cf. Backus and Grant, 2019).

[16] We thank Dan Feenberg at the NBER for his extremely helpful assistance in advising us on our use of TAXSIM.

[17] Other changes our analysis accounts for include TCJA's decrease in marginal tax rates, replacement of the personal and dependent exemptions with an expanded child tax credit, and reduction of the Affordable Care Act's individual-mandate penalty to zero. TCJA also made several changes less important for our nationally-representative sample of taxpayers (but which we also account for) including changes in the AMT and capital gains brackets.

where g_{it} is charitable giving (in 2018 dollars) by taxpayer i in year t. The variable $switcher$, which is not indexed by t, is a dummy denoting a taxpayer predicted to switch itemization because of TCJA, and $post$ is a dummy for post-TCJA t.

The term $f()$ captures that TCJA's increased standard deduction can change taxpayer incentives even holding constant pre-tax income and marginal tax rates (mtr) with respect to income. This permits estimation of the policy effect while controlling for linear and quadratic income and mtr, the log of income (plus 1) and mtr (plus .01), the interaction of income and mtr, a set of dummies for each income decile interacted with year dummies, and a set of quintiles for mtr in 2017 also interacted with year dummies.[18] This approach, which has not been possible in previous deduction-based studies, addresses the problem that tax incentives to give are closely related to income and marginal tax rates; here we can exploit the fact that TCJA introduces large changes in incentives holding income and mtr constant.

The term X_{it} represents a set of taxpayer controls: filing status, race, hispanicity, age, education dummies, and number of children. Y_t represents a set of year dummies (which subsume a not-interacted $post$ variable) and we include a set of region dummies R_i (some specifications will use state instead of region dummies). We will also consider individual fixed-effects specifications.

We consider several extensions to estimating (1). First, we check sensitivity by using Tobit (which does not matter), and by including earlier years (2000-2008) as well as focusing only on the years pre- and post- reform (the results are qualitatively similar). Second, we estimate placebo regressions (discussed below). Third, we investigate heterogeneity, both according to type of giving (separating religious congregations from other charitable organizations), and type of taxpayer (in line with the discussion of figure 1).

[18] We use mtr quintiles as there are not enough values to support deciles.

5. Estimates

5.1. *Intention-to-Treat Estimates*

5.1.1. *Overall Effects of TCJA*

Figure 2 gives a non-parametric depiction of the effects of TCJA. The figure focuses on joint filers (for whom the standard deduction rose from $12,700 to $24,000 after TCJA) whose total deductable expenses in 2016 (including giving) were between $5000 and $25,500; the figure includes both itemizers and non-itemizers. We plot kernel estimates of (a) total giving in 2016 (the dashed line) and (b) total giving in 2018, against deductions in 2016. The two lines are reasonably close to each other, but there is a drop in 2018 giving relative to 2016 giving. This drop appears to be near the high end of the itemization range where taxpayers would be induced to switch—akin to bundle A in figure 1, where theory would predict an unambiguous decline. Figure 2 is simple and suggestive, but focuses only on a subset of taxpayers; others (e.g., single filers and those with larger deductible expenses) may have responded to the law. Also, the figure gives little intuition on the magnitude of any overall effect or on precision.

To address these issues, table 2 presents estimates of (1) using various controls. The first column presents estimates where giving is regressed on the set of demographic controls X_{it}, the income and tax-rate controls $f()$, and year and region dummies. Standard errors are clustered by taxpayer. The coefficient indicates that among those predicted to stop itemizing, charitable giving falls by $366; the decline is statistically significant. We discuss magnitudes more below, but take this as an economically meaningful decline.

In column 2, we replace the region dummies and year dummies with a set of state-by-year dummies. While the PSID sample does not have extremely large populations in all states, identification should be independent of geography, and the estimates are close to before. Column 3 includes individual fixed-effects, exploiting the panel properties of our data. This specification is close to baseline. The next column considers Tobit estimation; the marginal effect is slightly larger but similar overall to the main estimate.

Column 5 introduces a post-TCJA interaction with those predicted to itemize in all years, so that (those predicted to be) never-itemizers are the control group. We call this a lower-bound estimate since our prediction of never itemizers is based on first dollar giving, so that some in this group may in fact respond to the law. This estimate is smaller but qualitatively similar to the main estimates. In the final column we instead include an interaction of never-itemizing with a post-reform dummy, effectively leaving those predicted to always itemize as the control group. This is a group whose influence on giving has been growing over time, so that a decline by those predicted to switch after TCJA relative to this group may overstate the effects of the law; we take this estimate as an upper-bound estimate. It is accordingly larger, but less precise.

Table 3 presents results on several alternate measures of giving behavior. The first column repeats the baseline effect. The next three columns break down the results by type of charitable donation. Giving to religious congregations (column 2) appears to be relatively insensitive to tax treatment; the estimate is only -$77 and not significant.[19] Instead, the results appear to be driven by giving to all other charitable organizations (column 3), and among them, especially by giving to organizations that help people in need (column 4).[20]

Column 5 uses log giving (recombining congregations and charitable organizations). The coefficient is negative but not statistically significant. This suggests that the baseline estimates are driven by a subset of relatively large donors among predicted-switchers. This would fit the theory from figure 1 earlier, where larger donors at or beyond bundle A are especially likely to lower giving in response to the reform. Column 6 of table 3 is from a regression on a dummy for whether one gives at all. The coefficient is small and insignificant, suggesting that responses to the reform are driven by the intensive margin.

[19] Although the share of aggregate giving going to religious congregations is declining, it remains large at 29 percent (Giving USA, 2020).

[20] There are four types of organizations in this group: organizations that provide basic necessities, international aid, and youth/family services, as well as organizations like the United Way that serve a combination of purposes. A large percentage of, though not all of, giving to these organizations is redistributive (Rooney and Brown, 2007).

The results are qualitatively similar when we focus on only the pre- and post- reform years, as well as when we use all years 2000-2018 (-\$295, *s.e.* = 137 and -\$322, *s.e.* = 175, respectively).[21] Figure 3 presents a set of placebo regressions where different tax years are assigned "treatment year" status. The 2002-2016 coefficients and 95% confidence intervals are each from a separate regression, where the *switcher* dummy is interacted with a different "treatment" year; these are placebos. These estimates use one placebo treatment year and up to four prior interviews as control years (to mimic the use of four prior interviews in the main estimates); placebo estimates using all years rather than just the prior four interviews produce similar results. The 2018 coefficient shows the effect of the actual policy change; this coefficient is much lower than any other estimate, lying at or below the bottom of all the placebo-estimate confidence intervals. Large negative coefficients among the placebo estimates would raise concerns. But there are no large and negative coefficients other than the one corresponding to the actual policy change.

5.1.2. Heterogeneity

The results to this point describe the overall effect of TCJA. But the theory in Section 2, as well as the results in figure 2, suggest that response to the law might be driven by a subset of taxpayers. To consider this further, figure 4 shows coefficients from a regression that breaks down the ITT effect by the amount of 2016 deductible expenses a predicted-switcher incurred (the regression also includes non-interacted dummies for each type of deductible expense). We estimate:

$$g_{it} = \alpha + \beta \ switcher_i \times post_t \times bin_j + \gamma switcher_i + \omega_j bin_j + \lambda_1 f() + \delta X_{it} + Y_t + R_{it} + \epsilon_{it} \quad (2)$$

which matches (1) with the addition of a set of interactions for different bins bin_j of deduction amounts in 2016. The estimates are for joint filers, for two reasons. First, this makes interpretation of the bins straightforward. Second, estimates using joint filers are close to

[21] Detailed results are in Appendix table A2.

the overall-sample estimates and are more robust.[22]

As before, predicted-switcher status is defined omitting giving, but the bins used in figure 4 are based on deduction amounts that include giving, following the intuition of figure 1. The first bin shows the coefficient for those predicted-to-switch who deducted $10k-20k in 2016 ($\sim$ 48% of all predicted switchers). Taxpayers in this range may experience TCJA primarily as an income effect, as at point A_0 in figure 1. The figure 4 estimate shows a small and positive effect, suggesting that some taxpayers who lost itemization increased their giving. This fits with figure 1 and the logged result in table 3 earlier.

The second bin contains predicted switchers who deducted $20k-30k (38% of all predicted switchers). This is the range where the price effect of TCJA is most likely to cause drops in giving, as at point A in figure 1. The figure 4 estimate shows a large (-$1,284) and significant decline.

The last two bins are for those predicted-to-switch who deducted $30k-40k (9% of all predicted switchers), and over $40k (5%), represented by those giving more than point A in figure 1. Taxpayers in these bins had high pre-TCJA levels of deductions (not including their giving) that were reduced because of the law (e.g., SALT). Recall from figure 1, for these taxpayers it is possible that TCJA had no effect or a large negative effect. The estimates show a large $-1,106 but insignificant ($p = 0.149$) decline for those in the $30k-40k bin, and small but imprecisely estimated effects for taxpayers above that amount.[23] In summary, the results in figure 4, as with figure 2 earlier, match what the theoretical discussion predicts.

All estimates to this point are reduced-form intention-to-treat estimates. One could also calculate treatment-on-the-treated estimates scaling the estimates by the effect of the law on those whose itemization actually changed. Intention-to-treat estimates matter for policy

[22]Redoing the baseline estimate with joint filers produces a main coefficient of -$430 (214). Redoing this with single filers produces a coefficient of -$55 (201), although estimates for single filers are generally sensitive to specification. This may be driven by the fact that joint filers make up 75% of predicted-switchers in the data and have much greater variation in their levels of deductions; there are in total fewer than 20 single-filing predicted-switchers across the three largest bin categories we use here. The PSID is too small to allow for a full exploration of heterogeneity for single-filers.

[23]There are few joint filers with deductions below $10,000 and who are predicted to switch, we include an interaction for them but omit it from the figure.

and qualitatively illustrate the importance of financial incentives to give. Treatment-on-the-treated estimates can be used to estimate an elasticity of giving; we do this next.

5.2. Treatment on the Treated and Elasticity Estimates

We calculate TOT estimates in two ways. First, we consider two-stage-least-squares (2SLS) estimates where we take the TAXSIM-reported itemization for a taxpayer (i.e., the endogenous outcome using actual data and actual laws in all periods) and instrument for this with our $switcher_i \times post_t$ variable. Second, we implement the three-step Censored-Quantile-IV (CQIV) estimator from Chernozhukov, Fernández-Val, and Kowalski (2015), which is a control-function based estimation approach similar to the (non-IV) quantile estimator of Chernozhukov and Hong (2002). Again the instrument is $switcher_i \times post_t$. To achieve convergence, these estimates omit decile- and quintile-by-year income and mtr dummies, use the 2017 and 2019 surveys only, obtain the confidence interval by bootstrapping, and use OLS for first-stage estimates. We consider both the 50^{th} and 90^{th} percentile effects.

The first three columns of table 4 show 2SLS results, standard errors, and the 95% confidence intervals for the 2SLS coefficient. Below this, we report an arc elasticity. If $\hat{\beta} > 0$ is the absolute value of the coefficient on itemization, then (the absolute value of) the arc-elasticity $|\hat{e}|$ is calculated as:

$$|\hat{e}| = \frac{\hat{\beta}/[(\bar{g}_0 + (\bar{g}_0 + \hat{\beta}))/2]}{\bar{m}/[(1 + (1 - \bar{m}))/2]} \tag{3}$$

where for the 2SLS estimates $\bar{g}_0$ is average giving for (actual) switchers in 2016 and $\bar{m}$ is their average tax rate that year. For the quantile estimates, we use the corresponding quantile value of giving for $\bar{g}_0$. For the 2SLS estimates the standard errors and confidence intervals are calculated using the delta method, and for the CQIV estimates we use the bootstrapped values of $\hat{\beta}$ to construct the confidence interval for $\hat{e}$.

The 2SLS results show main coefficients that are a little over twice the ITT estimates, suggesting a first stage coefficient on the instrument less than one half; it is -.30, *s.e.* = .018. The implied elasticities are less than unity for both congregations and charitable organizations, and the overall effect of .8 is smaller than, for example, the estimate in Duquette (2016). It further suggests that the foregone tax revenue from the itemization was greater than the fall in giving.

One could also quantify this overall effect by comparing it to the overall drop in giving observed in 2018. Using data from Giving USA (2020), a line fitted on giving from 2011 to 2017 would predict $460 billion in 2018 (this is shown in Appendix figure A1); instead actual giving was $439 billion, a difference of $21 billion. At the same time, the number of taxpayers claiming a charitable deduction after the reform fell by about 23 million (Internal Revenue Service, 2020). If these taxpayers on average contributed $880 less as a result, as suggested in table 4, the predicted decline in giving would be about $20.4 billion. Our results suggest that essentially the entire observed decline in giving in 2018 can be explained by the TCJA reform. This decline would represent about a 4% decrease in aggregate giving, an effect close to the predictions made in the pre-reform work of Meer and Priday (2020), but more generally on the small end of what was predicted at the time of the reform.[24]

The last two columns in table 4 report CQIV results. The first CQIV result uses the median, and finds a negative and significant coefficient of -785. This suggests that the median switcher's donation increased as a result of the reform (while they stopped itemizing), again fitting with the logged results earlier and the estimates in figure 4 breaking the ITT effect apart by 2016 deductions (where the smallest-deduction group, which increased giving in response to the reform, represented about half of the sample). In contrast, the 90th percentile result is of the opposite sign; the confidence interval is larger as well. The estimate here is consistent with switchers with high levels of donations undertaking large decreases in

[24]The $20.4 billion predicted decline is relative to trend; this implies a $10 billion decline relative to giving in 2017. By way of comparison, Meer and Priday (2020) predicted a year-to-year drop of $9.8 billion. Other predictions included drops of $4.9 to $13.1 billion (Lilly Family School of Philanthropy, 2017), $17.2 billion (American Enterprise Institute, 2018), $12 to $20 billion (Urban Institute, 2017), and $22 billion (Penn-Wharton, 2018).

response to the reform.[25]

Overall the estimates, across a variety of estimation techniques, specifications, samples, and placebo tests, indicate that the TCJA lowered giving, particularly giving to non-congregational charitable organizations. However, the estimates presented to this point combine TCJA's permanent effects with any effects of gift retiming due to TCJA's enactment before the conclusion of the 2017 tax year. We turn now to a new approach to adjust estimates to take out retiming effects.

5.3. Gift Retiming

5.3.1. Adjustment Approach

Following the introduction and passage of TCJA in the fourth quarter of 2017, forward-looking taxpayers may have anticipated losing itemization in 2018, and consequently retimed gifts into 2017. Such anticipation matters for interpreting the estimated effects of changes in tax policy (Randolph, 1995; Bakija and Heim, 2011). Conversely, Andreoni and Durnford (2019) discuss the possibility that TCJA's relatively quick enactment at the end of 2017 may have limited anticipatory behavior from taxpayers in 2017.

In this section we use IRS zip code data to adjust the estimates for retiming. The IRS zip code data are publicly available and measure annual giving; as will be made clear below, annual data are necessary to adjust for retiming. The zip code data, however, do not contain information about prices and only cover the giving of itemizers. Under reasonable assumptions our adjustment approach handles both limitations.

The adjustment extends the dynamic model first used by Auten, Seig, and Clotfelter

[25] Using administrative tax-return data, Fack and Landais (2010) estimate elasticities based on censored-quantile regressions. Their estimates cannot include quantile effects below the 90th, because in their setting (early 2000s France) the proportion of taxpayers reporting donations was .12 to .13. Our results, estimated in a different context with much higher proportion of donors, suggest large differences across the distribution of givers. The present .475 elasticity at the 90th quantile, however, is consistent with the range of their estimates for the 90th to 99th quantiles (.155 to .576).

(2002) and subsequently by Bakija and Heim (2011). The extension is:

$$g_t = b_0 p_t + b_1 \zeta_t + b_2(\hat{p}_{t+1|t} - p_t) - b_2(\hat{p}_{t|t-1} - p_{t-1}) + \epsilon_t \tag{4}$$

where p_t is the long-term permanent price of giving, and $b_0 < 0$ is the response of giving to its permanent price, typically the parameter of interest. Auten, Seig, and Clotfelter's insight was to conceptualize an observed change in the price of giving as having both permanent and short-term transient effects on giving; in (4) the transient price is ζ_t (zero mean) and its effect on giving is $b_1 < 0$. Bakija and Heim extended this framework so that giving changes in response to past, present, and future price shocks. Here $\hat{p}_{t+1|t}$ represents the expected future price of giving for a taxpayer in period t. If policy makers, for example, implement a higher price in period $t+1$ but pre-announce this shock in period t, then giving may rise in period t, captured by the $b_2(\hat{p}_{t+1|t} - p_t)$ term in (4), $b_2 > 0$. Our extension is to use this framework to model the *retiming* of a gift: the case where an anticipatory increase in period t reflects giving shifted <u>out</u> of the $t+1$ period of implementation. The next term $b_2(\hat{p}_{t|t-1} - p_{t-1})$ in (4) reflects the decrease in giving that was retimed to an earlier period. Giving in period t incorporates current prices, retiming in response to future shocks, and retiming decisions made in earlier periods. ϵ_t is exogenous noise (zero mean).[26]

Figure 5 applies (4) to the years of TCJA's enactment (2017) and implementation (2018). The figure assumes that in expectation $\Delta\zeta_t$ and $\Delta\epsilon_t = 0$, and that in 2017, the price increase to come in 2018 was accurately anticipated: $\hat{p}_{t+1|t} - p_t = p_{t+1} - p_t \triangleq \omega$. Then the 2018-2016 difference $\Delta g_{\text{2018-2016}} = (b_0 - b_2)\,\omega$. This is the difference estimated using the biennial PSID data, and it is larger in absolute value than the permanent effect b_0 because of the retiming response $-b_2 < 0$.[27] For the PSID estimate $\hat{\beta}_{\text{PSID}}$, the necessary adjustment r so that $r\hat{\beta}_{\text{PSID}} = b_0$ is $r = \frac{b_0}{(b_0 - b_2)} = \frac{1}{(1 - b_2/b_0)}$. The adjustment requires the

[26]It is also possible that taxpayers respond by shifting giving between many post-enactment periods and a pre-announcement period. We consider this below.

[27]Alternately, anticipatory giving in period 1 could come not from future giving, but from other nongiving expenditures. In this case, the 2018-2016 difference would identify the permanent response b_0. This increase in giving would not be retiming.

ratio of the retiming effect to the permanent effect b_2/b_0, and that can be calculated from $\frac{\Delta g_{\text{2017-2016}}}{\Delta g_{\text{2018-2016}} + \Delta g_{\text{2017-2016}}} = b_2/b_0$, where the 2017-2016 difference $\Delta g_{\text{2017-2016}} = b_2\ \omega$.

We use IRS zip code data to calculate b_2/b_0. The limitation that the IRS data do not contain price information is not an obstacle if retiming taxpayers accurately anticipate the coming price increase, ω.[28] The second limitation, that IRS data only cover the giving of itemizers, can be handled by scaling up the IRS data to account for the giving done by non-itemizers. If x_{pre} and x_{post} are the scale-up factors pre- and post-announcement (both > 1), then b_2/b_0 can be calculated from $\frac{(x_{\text{post}}\ g_{2017} - x_{\text{pre}}\ g_{2016})}{(x_{\text{post}}\ g_{2018} - x_{\text{pre}}\ g_{2016}) + (x_{\text{post}}\ g_{2017} - x_{\text{pre}}\ g_{2016})} = \frac{(g_{2017} - \alpha g_{2016})}{(g_{2018} - \alpha g_{2016}) + g_{2017} - \alpha g_{2016})}$ where $\alpha = x_{\text{pre}}/x_{\text{post}}$. We consider several calculations of α; each produces similar results.

The appeal of this approach is that it combines the PSID estimates—which have good information on nonitemization and prices—with the model-based retiming adjustment using zip code data—which has good information on retimed giving in 2017. Intuitively, a retimed gift done in anticipation of a policy change does not represent a permanent reduction in giving, and will match what Auten, Seig, and Clotfelter model as a transitory response. If the response to the price change is driven by retiming ($b_0 \approx 0$), $\Delta g_{\text{2018-2016}}$ and $\Delta g_{\text{2017-2016}}$ will be strongly negatively correlated, the denominator of $\frac{\Delta g_{\text{2017-2016}}}{\Delta g_{\text{2018-2016}}+\Delta g_{\text{2017-2016}}}$ will approach zero, $\frac{b_2}{b_0}$ will become large, and $\hat{\beta}_{\text{PSID}}/(1 - \frac{b_2}{b_0})$ will approach zero. At the other extreme, if there is no retiming, then there should not be a systematic relationship between any spike in giving in the pre period and any drop in giving after implementation, so that $\Delta g_{\text{2017-2016}}$ goes to zero while $\Delta g_{\text{2018-2016}} + \Delta g_{\text{2017-2016}}$ goes to b_0, and as $r \to 1$ our PSID estimates will return the permanent effect b_0.

5.3.2. Empirical Results

Table 5 reports estimates where several different calculations of r are used to adjust the results from tables 3 and 4 earlier. In the first row, we calculate the ratio r using a simple

[28]The justification is that taxpayers who are sophisticated enough to retime their gifts will rationally expect the coming price change.

back of the envelope: we fit a trend line to total charitable giving in the US from 2011 to 2017; we then calculate the difference between actual and predicted giving the year before TCJA and the drop below trend the year after. Total charitable giving in the US comes from Giving USA (2020) and is depicted in Appendix figure A1. The basic time series of aggregate giving in the figure does appear to show a modest increase in giving relative to trend in 2017, raising the possibility that retiming occurred. We use the ratio of this increase and the subsequent post reform fall for b_2/b_0; this ratio is $-.36$. We adjust our estimates by a factor of $r = 1/(1+.36) = .74$, indicating retiming of 26 cents on the dollar. This indicates an important role for retiming, although estimates are qualitatively similar to before.

It is possible that some of the anticipatory increase in giving would have occurred not in the first year of reform but from other years further into the future. In this case, the importance of retiming on the initial estimates will be lessened. This is illustrated in the second row, which redoes the calculation in the first row assuming the initial spike in giving reflects retimed gifts from each of the first two post-reform years. We thus make a ratio of the pre-reform spike in giving over the combined drop in giving, relative to trend, in both 2018 and 2019. The estimates are now closer to the original unadjusted estimates. Intuitively, if the anticipatory spike in giving is drawn from many future years, then the observed drop in giving in any post-reform year will predominantly reflect the *permanent* change, as the importance of retiming becomes "diluted" across many post-reform periods. To the extent giving was retimed from far in the future, the rest of the table (which assumes all retiming comes from the initial post period) represents a overestimate of the importance of retiming.

The last three rows use zip code giving data to calculate r. The results are weighted by the average number of returns in a zip code (unweighted results are similar). The third row estimate of r uses the ratio of itemizers pre- and post announcement for α; here r is about .88. The fourth row sets $\alpha = 1$, thus ignoring variation over time in the fraction of itemized returns. The results are very close to before; this (along with the results in the first two rows, which are based on aggregate giving and do not require any use of α) suggests that our reliance on data using itemized zip code data for our adjustment is not consequential. The last row calculates r in zip codes that had an above-median change in the fraction of itemized returns—zip codes populated by the switchers that identified our PSID estimates. The results are close to before. Across table 5, these results suggest that about 20% of the post-reform drop in giving was retimed to the year before the law's implementation. Applying this retiming adjustment to the TOT elasticity estimate from the PSID (.8) suggests that the permanent elasticity over all taxpayers in response to TCJA is .6

5.4. Compensated Elasticity Estimate

Recall that figure 1 predicts heterogeneous responses to TCJA depending upon taxpayers' pre-TCJA level of iemtized deductions. Figure 4 provides intention-to-treat evidence of heterogeneous responses in line with figure 1 predictions. To create treatment-effect-on-the-treated estimates that capture this heterogeneity, we create a set of first-stage variables, $itemizer_{it} \times bin_j$, and estimate a 2SLS regression where these itemizer variables are instrumented for by the $switcher_i \times post_t \times bin_j$ variables. The detailed results for married-joint taxpayers are in Appendix figure A2 and accord with theory. For married-joint taxpayers whose 2016 deductions were between \$20,000-\$30,000, close to the TCJA standard deduction of \$24,000 (i.e., bundle A in figure 1), the TOT effect of itemization on giving is about \$2000. Assuming that 20 percent of this response is retiming, and using the average pre-TCJA donation amount and tax rate for this group (about \$3300 and .23), yields a compensated elasticity of 2.6 ($s.e.$ = 1.01). This is a large response, although several factors should be kept in mind when considering it.

First, the same estimation approach that produced the compensated elasticity, when applied to all groups of switchers, yields a much smaller elasticity estimate (.6) which in turn implies an overall estimated impact of TCJA that, as noted earlier, is similar or smaller in magnitude than the effects predicted ex ante by experts. Second, findings where the price-sensitivity of giving are driven by a responsive subset of donors have precedent in the literature. For example, in Karlan and List's (2007) field experiment, the response to matches were driven by a subset of donors (see also Duquette, 2016; Hickey et al., 2023). A straightforward but often overlooked implication of these types of results is that the underlying variation in elasticities between the most and least responsive can be large.

Next, this relatively large negative response is driven by the taxpayers for whom theory predicts a relatively large negative response. Moreover, this elasticity estimate is reasonably close to the permanent elasticity estimates in Auten, Seig and Clotfelter (2002) whose sample covers the years of the previous very large change in tax policy (the Tax Reform Act of 1986; see, for example, Auten et al.'s table 3 column 2). Although our approach to giving dynamics is similar to theirs, their estimate is based on an entirely different sample and estimation strategy. But both TRA-1986 and TCJA were large, salient policy changes, and large salient policy changes may lead to large responses (Chetty, 2012). In short, the results for the most responsive taxpayers, while large, are based on an approach that over all taxpayers produces estimates consistent with ex-ante predictions of the effect of the reform on giving, consistent with the idea that large policy shocks can generate large responses, with findings of heterogeneity in past research, and with the theoretical framework.

6. Conclusions

The Tax Cut and Jobs Act's near doubling of the standard deduction created large changes in financial incentives to give for millions of taxpayers. A straightforward theoretical analysis indicates that exactly how incentives changed for a particular taxpayer depends on their pre-TCJA level of itemized deductions: taxpayers experienced TCJA as a positive income effect and a negative price effect to varying degrees. Setting this heterogeneity aside for the moment, among the taxpayers who switched to the standard deduction because of TCJA, giving fell by about $880 on average, implying a price elasticity of giving = .8.

A pre-announced change in tax policy that reduces incentives to give can lead taxpayers to shift some of their giving to the present year, before the reduced incentives take effect. Estimates that do not account for potential shifting therefore can overstate the permanent effects of policy changes. We introduce a new approach to adjust estimates to account for shifting. The approach is applicable generally to contexts in which a change in tax policy is pre-announced. Applied to the TCJA context, the adjustment suggests that about 80 percent of the post-TCJA fall in giving is permanent. This indicates a smaller permanent price elasticity of giving, .6.

The results indicate heterogeneity in two dimensions. First, very little, if any, of the TCJA-caused decrease in giving was due to decreased giving to religious congregations. Instead the decrease fell on organizations with other charitable purposes, and among them a large portion fell on organizations that help people in need. Second, theory predicts that the largest responses to the policy would be among taxpayers whose pre-TCJA level of itemized deductions was around the level of the new, much-increased TCJA standard deduction. For those taxpayers, TCJA effected a large compensated price increase. The results are in line with this prediction: the estimated compensated (permanent) price elasticity of giving is over 2.

Previous research has produced divergent estimates of the charitable giving response to price incentives. The present evidence of heterogeneous responses, within the context of a single policy change, suggests a different interpretation of previous divergent estimates: they may plausibly reflect true heterogeneity. We now know that there are theoretical reasons to expect different responses to a reduction in marginal tax rates, changes in price due to a tax credit, expansion of the standard deduction, elimination of itemization, and matches. Simply put, there may not be one homogeneous price elasticity of giving that an internally valid study of <u>any</u> policy or intervention in <u>any</u> context should produce. Indeed, the literature's two estimates of compensated price elasticity—the present elastic estimate for all giving and Hungerman and Ottoni-Wilhelm's (2021) inelastic estimate for giving to education—suggest that heterogeneity by type of giving may exist in a parameter that is of fundamental importance for both policy and welfare analysis. It appears that future work on price incentives to give is needed that directly investigates several dimensions of context: type of policy/intervention, size of policy/intervention, type of donor, and type of charitable organization.

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Table 1: Means of Variables

Giving	1670	Black	0.105
	[4151]		[0.307]
Switcher	0.224	Hispanic	0.036
	[0.417]		[0.186]
Income	92762	Age	47.8
	[102406]		[16.0]
2017 Tax Rate	16.1	High School Graduate	0.255
	[9.2]		[0.436]
Joint Filer	0.586	College Graduate	0.38
	[0.493]		[0.485]
Single Filer	0.292	Number of Children	0.736
	[0.455]		[1.15]

Observations: 18509 from 4,232 unique individuals in the PSID from 2011 to 2019. Standard deviations in brackets. The variable switcher is a dummy for whether an individual would be predicted to itemize in 2019 if the TCJA law were not passed and to take standard deduction after the law is passed. These predictions are based on 2017 itemizable expenses excluding charitable giving and adjusted for inflation. Income is in 2018 dollars, and the 2017 tax rate is the marginal tax rate with respect to earnings as determined by Taxsim.

Table 2: ITT Estimates of an Exogenous Change in Itemization Status on Giving

	Baseline	State-Year Fes	Individual Fes	Tobit	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)
Switcher × Post Reform	-366.2	-364.3	-382.6	-390.4	-272.7	-707.4
	(159.3)	(159.7)	(162.3)	(200.7)	(134.0)	(378.3)
Region FEs?	Yes	Yes	Yes	Yes	Yes	Yes
State by Year FEs?	No	Yes	No	No	No	No
Individual FEs	No	No	Yes	No	No	No

Observations: 18,509. Standard errors, clustered by individual, in parentheses. The sample is from the PSID, from interview years 2011-2019. The dependent variable is total charitable giving. Each cell is from a separate regression and shows the coeffiicent for being a switcher interacted with a dummy for the period after the TCJA. All regressions include controls for filing status, switcher status, race, hispanicity, age, education, number of kids, year dummies, region dummies, and always/never/switcher status. A set of income-decile-by-year dummies, tax-rate-quantile-by-year dummies, and linear, log, and quadratic controls for income and the tax rate are also included.

Table 3: Itemization Status on Different Types of Giving

	Baseline	Religious congregations	Other charitable	Help people in need	Log Giving	Giving Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
Switcher × Post Reform	-366.2	-76.99	-289.3	-194.0	-0.0493	-0.00312
	(159.3)	(106.8)	(116.7)	(88.3)	(0.0593)	(0.0164)
Region FEs?	Yes	Yes	Yes	Yes	Yes	Yes

Observations: 18,509. Standard errors, clustered by individuals, in parentheses. The sample is from the PSID, from interview years 2011-2019. The dependent variable is total charitable giving in columns 1, 5, and 6. Column 2 uses giving to congregations and column 3 uses all other giving. Column 4 is giving to organizations that provide basic necessities, international aid, youth/family services, and that serve a combination of purposes (column 4 is a subset of column 3). Each cell is from a separate regression and shows the coeffiicent for being a switcher interacted with a dummy for the period after the TCJA.

Table 4: TOT Estimates of Itemization and Giving

	IV Other charitable orgs. (1)	IV Religious congregations (2)	IV All (3)	CQIV All (5)	CQIV All (6)
Itemization Coefficient	697.4	185.6	883.0	-785	977
Standard Error	(279.9)	(256.1)	(380.1)	-	-
CI	[149, 1246]	[-316, 618]	[138, 1628]	[-1165, -260]	[-1630, 3072]
Elasticity	0.654	0.186	0.81	-7	0.479
Standard Error	(.240)	(.250)	(0.311)	-	-
CI	[.184, 1.12]	[-.303, .677]	[.200, 1.42]	[-20, -1.4]	[-0.96, 1.32]
Method	2SLS	2SLS	2SLS	CQIV	CQIV
Percentile	NA	NA	NA	50	90

Observations: 18,509. Standard errors, clustered by individuals, in parentheses, and 95% confidence intervals in brackets. The top row shows 2SLS and CQIV estimates of the second-stage coefficient on whether an individual itemizes. Below this is a row showing arc-elasticity estimates. These are calculated for the average switcher (ie, using average donation and marginal rate information in 2016) for the 2SLS estimates and for the 50th and 90th percentile switcher for the CQIV estimates. The standard errors for the elasticities are calculated using the delta method based on the standard errors in the 2SLS approach, and the confidence intervals for the CQIV estimates take the bootstrapped confidence interval and apply the arc elasticity to the confidence interval boundaries.

Table 5: The TCJA and Giving--Adjusted for Retiming

	Other charitable orgs. (1)	Religious congregations (2)	All (3)	TOT Elasticity (4)
One Year Trend-Based Adjustment				
Estimate	-212.4	-56.5	-268.8	0.595
Standard Error	(85.7)	(78.4)	(116.9)	(0.228)
Two Year Trend-Based Adjustment				
Estimate	-250.3	-66.6	-316.8	0.701
Standard Error	(101.0)	(92.4)	(134.5)	(0.269)
Autocorrelation Adjustment: Baseline				
Estimate	-261.6	-69.6	-331.1	0.732
Standard Error	(105.5)	(96.6)	(144.0)	(0.281)
Autocorrelation Adjustment: $\alpha = 1$				
Estimate	-254.0	-67.6	-321.5	0.711
Standard Error	(102.5)	(93.8)	(139.9)	(0.273)
Autocorrelation Adjustment: Switcher Zip Codes				
Estimate	-257.0	-68.4	-325.3	0.720
Standard Error	(103.7)	(94.9)	(141.5)	(0.276)

This table reports estimates of the level effects in table 3 and the all-giving elasticities in table 4, where here the estimates have been adjusted to remove changes in giving that reflect retiming. Standard errors, also adjusted (via the delta method), in parentheses. In the first row we fit a trendline to total charitable giving in the US from 2011 to 2017 and calculate the difference between actual and predicted giving the year before TCJA and the drop below trend the year after; we use the ratio of these differences as the fraction of the drop in giving that was retimed. In the second row we do the same but use the combined drop below trend in the first two-post reform years in the denominator of our ratio. The last three rows use IRS data to calculate the median of the ratio $(g_{17}-g_{16})/(g_{18}-g_{16} + g_{17}-g_{16})$. The results are weighted by the average number of returns in a zip code (unweighted results are similar). The fourth row makes this calculation assuming that the fraction of giving captured by itemized returns in the IRS data stays constant over time. The last row is the same as row 3 but only zipcodes with above-median changes in the fraction of itemized returns following TCJA's passage. The estimates of *r* used in rows 1 through 5 are .734, .865, .904, .878, and .888.

Figure 1: Effect of Expansion in the Standard Deduction

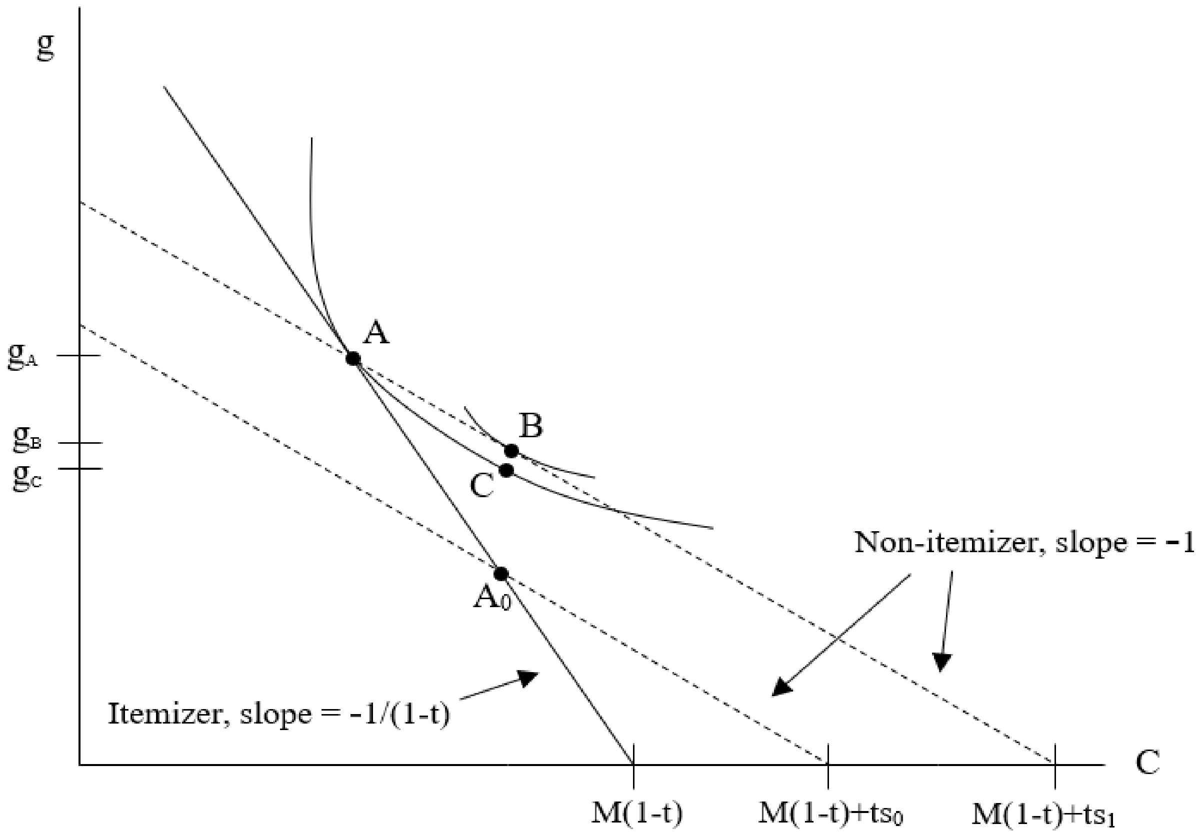
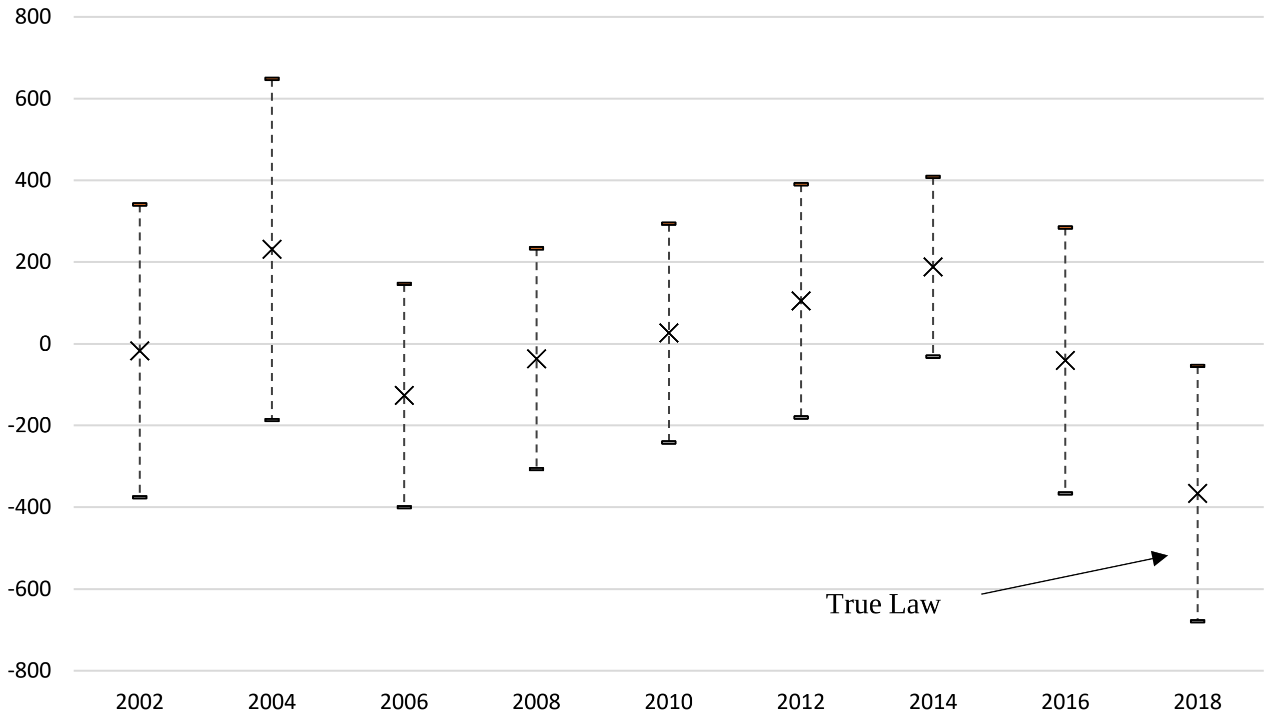


Figure 2: Nonparametric Estimate of Change in Giving

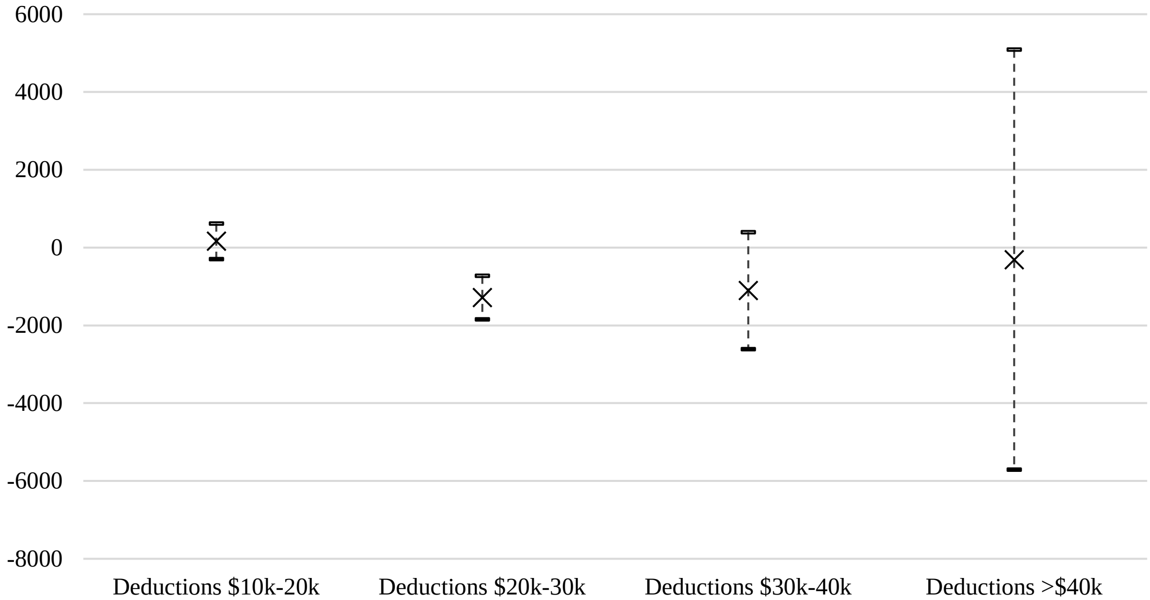
The figure shows two kernel regressions of giving, one for 2018 and one for 2016. Each year of giving is regressed on deductable expenses in 2016. The figure is for joint filers. Each estimator fits a linear polynomial, using Epanechnikov weights, at each percentile value on the x axis.

Figure 3: Imposing Law Change in Other Years



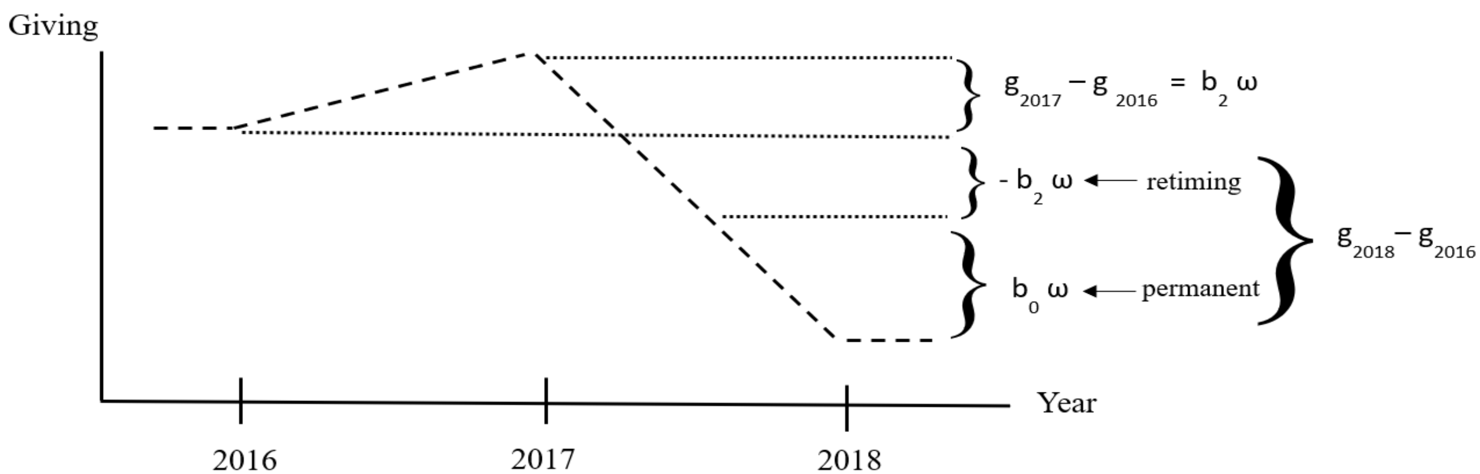
The figure shows a set of coefficients and 95% confidence intervals, each from a separate regression, where the TCJA treatment year is assumed to be in effect in a different year. Each regression uses all available years in the sample.

Figure 4: Effect By 2016 Deduction Amount



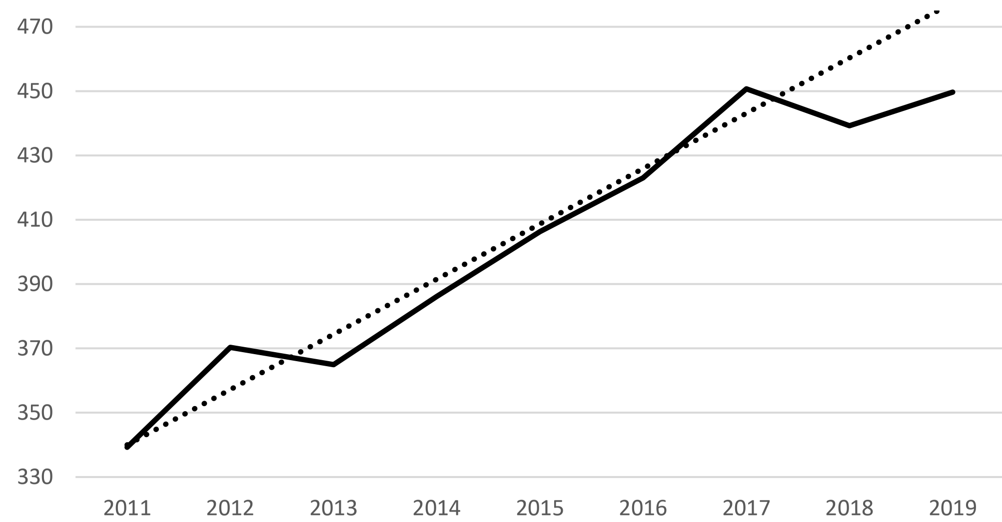
The figure shows a set of coefficients, all from one regression, where the treatment effect of the TCJA law is interacted with a set of dummy variables for the amount of total deductable expenses in 2016. (A set of non-interacted dummies for deduction size is also included). The sample is joint filers. The regression otherwises matches the baseline specification in Table 2. Those deducting $10k-20k make up about 48% percent of all switchers, those deducting $20k-30k about 38%, those deducting $30k-40k about 9%, and those deducting over $40k about 5%.

Figure 5: Dynamic Timing of Giving Prior to Price Increase



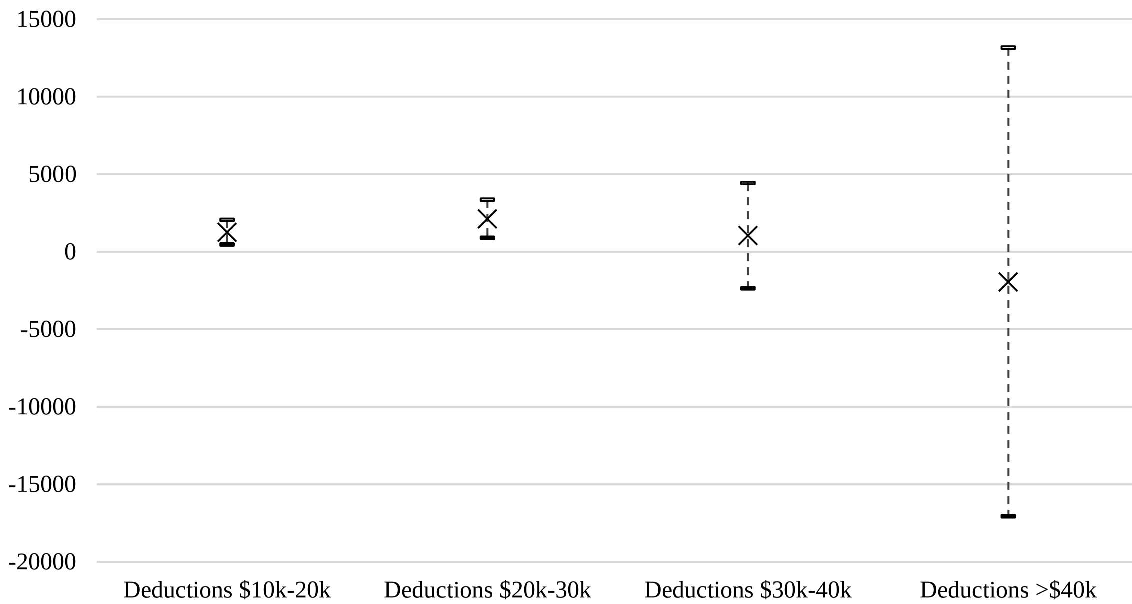
The picture depicts a response to a policy change (a permanent increase in price of size ω) in 2018 that was announced in period 2017. If donors anticipate the price increase by shifting giving into 2017, then the long-difference change in giving captured by the PSID will be $\beta_{PSID} = b_0 - b_2$. This long-difference price response can be adjusted by the retiming response r: $r\,\beta_{PSID} = b_0$ for $r = b_0 / (b_0 - b_2) = 1 / [1 - (b_2 / b_0)]$. We thus estimate b_2 / b_0 and adjust the PSID estimates.

Appendix Figure A1: Total Giving and Projected Giving, 2010-2018



Source, Giving USA (2020) page 333. The fitted values are calculated using the years 2011-2017. Giving in 2018 was $439 billion, while the projected fitted value is $460.

Appendix Figure A2: 2SLS TOT Estimates on Itemization By 2017 Deduction Amount



The figure shows a set of coefficients, all from one regression, where the coefficients of interest are itemization dummies interacted with 2017 deduction amounts for joint filers. These variables are instrumented using a set of switcher-interatcted-with-2017-deduction variables. (A set of non-interacted dummies for deduction size is also included in both stages). The coefficients and standard errors for the bins are: 1255 (404), 2122 (623), 1032 (1732), and -1961 (7715), respectively.

Table A1: Standard Deductions, 2010-2019

Year	Single	Head of Household	Married Couple
2010	5,700	8,400	11,400
2011	5,800	8,500	11,600
2012	5,950	8,700	11,900
2013	6,100	8,950	12,200
2014	6,200	9,100	12,400
2015	6,300	9,250	12,600
2016	6,300	9,300	12,600
2017	6,350	9,350	12,700
2018	12,000	18,000	24,000
2019	12,200	18,350	24,400

Source: Tax Policy Center. Married-filing-separately households faced a standard deduction equal to the single filer deduction above

Appendix Table A2: Estimates of an Exogenous Change in Itemization Status on Giving--Different Time Periods

Panel A: PSID Years 2017 & 2019

	Baseline	Individual Fes	Tobit	Lower Bound	Upper Bound	IV
	(1)	(3)	(4)	(5)	(6)	(4)
Switcher × Post Reform	-295.1	-330.8	-334.4	-235.6	-503.7	429.1
	(187.2)	(186.9)	(243.5)	(184.8)	(379.7)	(270.6)

Panel B: PSID Years 2001-2019

	Baseline	Individual Fes	Tobit	Lower Bound	Upper Bound	IV
	(1)	(3)	(4)	(5)	(6)	(8)
Switcher × Post Reform	-322.0	-283.8	-302.7	-244.7	-606.0	1063.1
	(169.7)	(161.1)	(208.1)	(139.7)	(424.5)	(553.3)

Observations: 8379 in for the baseline regression in Panel A, and 30699 in Panel B. Standard errors, clustered by individual, in parentheses. The estimates redo those in Tables 2 and 4, but the top panel restricts the sample to the PSID interview years 2017 & 2019, and the bottom panel includes all waves between 2001 and 2019 (giving questions were first asked in 2001).