

**Preliminary and incomplete.
Do not cite without permission
of the authors.**

Identifying Tax Effects on Charitable Giving

Mark Ottoni Wilhelm and Daniel M. Hungerman *

December 5, 2007

*Wilhelm: Center on Philanthropy at Indiana University and Department of Economics, IUPUI. Hungerman: Department of Economics, University of Notre Dame and NBER. We are grateful to Dan Feenberg of NBER for his extraordinary efforts in adding features to TAXSIM necessary for our work, and to Inna Shapiro for her help with TAXSIM. We thank Rich Steinberg for extensive contributions in the early stages of this project. Thanks also to Rob McClelland for comments, and to Melissa Brown for sharing data from *Giving USA*. We thank the Center on Philanthropy Research Fund for financial support. Thanks also go to the agencies that fund the *PSID* and to Atlantic Philanthropies for funding the collection of the *Center on Philanthropy Panel Study*. Corresponding author: Mark Wilhelm, Dept. of Economics, 516 Cavanaugh Hall, IUPUI, Indianapolis IN 46202 or mowilhel@iupui.edu.

Identifying Tax Effects on Charitable Giving

Executive Summary

This paper estimates the effects of three federal tax acts—the Economic Growth and Tax Relief Reconciliation Act of 2001 (H.R. 1836), the Jobs and Growth Tax Relief Reconciliation Act of 2003 (H.R. 2), and the Working Families Tax Relief Act of 2004 (H.R. 1308)—on charitable giving using the *Center on Philanthropy Panel Study*, the philanthropy module in the *Panel Study of Income Dynamics (PSID)*. The *Center Panel* data have three advantages relative to tax return data for the policy analysis of charitable giving. The *Center Panel* (i) contains the giving of non-itemizers as well as itemizers, permitting the analysis of the effect of switching itemization status on giving; (ii) breaks aggregate giving down into giving to different types of charitable organizations, permitting the analysis of tax effects on different types of charities; and (iii) contains the *PSID*'s rich contextual information about family economics, permitting important methodological and substantive advances.

The key results are that the 2001–2004 tax acts raised the tax price of giving among the always-itemizers by 4.2 percent (Table 2), but this masks larger price changes: an 8.8 percent average increase for itemizers experiencing a price rise and a 5.2 percent average decrease for itemizers experiencing a price drop. Itemization switchers (those who go from itemizing to not itemizing, or vice versa) experienced larger price changes: on the order of 23 percent. Unconditional changes in giving indicate that both itemizers and switchers experiencing a price drop, give more—consistent with a strong tax policy effect on giving. Unconditional changes in giving are much smaller (and sometimes positive) for those experiencing a price rise.

Multiple regression results that control for changes in income indicate a strong secular giving response to changes in the tax price, by both switchers and itemizers (Table 3). Moreover for switchers, all types of giving are sensitive to tax-price—giving to help the poor, religious giving, and the aggregate giving that would be reported in tax return data (Table 4). In contrast, the aggregate giving of itemizers is less sensitive to the tax price. Hence, policy analysis with the *Center Panel* data reveal different results than would be observable with tax return data. The *Center Panel* significantly expands the range of policy analysis that can be employed to examine the tax effects on charitable giving.

Identifying Tax Effects on Charitable Giving

Abstract

This paper estimates the effects of three federal tax acts—the Economic Growth and Tax Relief Reconciliation Act of 2001 (H.R. 1836), the Jobs and Growth Tax Relief Reconciliation Act of 2003 (H.R. 2), and the Working Families Tax Relief Act of 2004 (H.R. 1308)—on charitable giving, and offers four extensions relative to previous work. First, we use new data—the *Center on Philanthropy Panel Study*, the philanthropy module in the *Panel Study of Income Dynamics*—that permit the estimation of the effect of switches in itemization status on giving. This is important because switches permit a direct answer to the question: How much of an increase in charitable giving is caused by tax deductibility? Second, the new data permit the estimation of tax effects on charitable giving to secular charities as well as to religious organizations. This is important because the main policy question in the literature on taxes and giving is to evaluate “treasury efficiency”—whether the Treasury can cause more money to flow to charitable organizations by allowing deductibility of giving than by eliminating deductibility and sending the increased tax revenue directly to charitable organizations. By using secular giving, we can focus on the type of giving most relevant to this policy question. Third, the new data allow for improved methodological approaches over past studies. Fourth, we argue that the 2001, 2003, and 2004 federal tax acts were timed such that they provide a set of tax changes suitable for identifying permanent effects of taxes on giving. The estimates based on the analysis of families who switch itemization status suggest that secular giving is price elastic, implying that treasury efficiency holds. In contrast, estimates which impose the restrictions facing other datasets suggest a statistically insignificant price elasticity.

Identifying Tax Effects on Charitable Giving

1. Introduction

The central question about taxes and giving is: How much of an increase in charitable giving is caused by deductibility? If tax deductibility causes a larger increase in charitable giving than the associated reduction in tax revenue, then the Treasury can cause more money to flow to charitable organizations by allowing deductibility of giving than by eliminating deductibility and sending the increased tax revenue directly to charitable organizations. For this reason, if tax deductibility causes a larger increase in charitable giving than the reduction in tax revenue, deductibility is said to be “treasury efficient.”

The standard indicator of treasury efficiency is whether a one percent decrease in the tax-induced price of giving (price = $1 - \text{marginal tax rate}$) causes more than a one percent increase in giving—that is, whether giving is “price elastic.” Therefore, the objective of the large literature on taxes and giving is to estimate the price elasticity of giving and determine whether the evidence is strong enough to conclude that giving is price elastic.

However, the literature is far from a consensus about whether giving is price elastic. Indeed, opposite conclusions about the price elasticity of giving have been reached by two important papers in the literature. Randolph (1995) concludes that the permanent response of giving to the tax-induced price of giving is not elastic, and therefore that deductibility is not treasury efficient. By “permanent” response, Randolph means the giving response to a long-term, sustained change in the price of giving. In contrast, Auten, Sieg and Clotfelter (2002) conclude that the permanent price of giving is elastic, and hence that deductibility is treasury efficient.

In addition to the absence of consensus, the large literature on taxes and giving has three serious limitations. First, the literature has not directly estimated the price elasticity of giving among families who switch between itemizing deductions and not itemizing. Therefore, the literature has not directly considered how much of an increase in charitable giving is caused by deductibility. Rather, the literature has estimated the price elasticity of giving among families who always itemize but over time are faced with drifting marginal tax rates, and used the “always-itemizer” price elasticity to infer indirectly the “deductibility-switcher” price elasticity.

Second, when estimating the always-itemizer price elasticity of giving, the literature typically uses aggregate giving. Aggregate giving includes giving to charitable organizations that provide human services, help people with basic needs, educate children, etc., but aggregate giving also includes religious giving to churches, synagogues, and mosques. However, religious giving should not be included when trying to draw a conclusion about treasury efficiency because the government is constitutionally prohibited from funding churches, synagogues, and mosques—religious giving falls outside the bounds of the constitutionally-relevant treasury efficiency question. This might not be much of a practical problem if religious giving were a small share of total giving, but religious giving makes up 40 percent of total giving (Brown and Rooney 2007).

Third, the literature faces methodological constraints, especially in the availability of what economists refer to as “instrumental variables” or simply “instruments.” An instrumental variable approach is necessary to estimate the permanent effect because “permanent” income and price are not directly observable. Indeed, the lack of consensus between Randolph’s results and Auten et al.’s results hinges on the different ways they use instruments to identify the permanent effect. In such a circumstance, it is necessary to examine alternative instruments to see if they

can resolve the lack of consensus.

The literature has not estimated the “deductibility-switcher” price elasticity, has not excluded religious giving from its price elasticity estimates, and has restricted itself to a narrow set of income-based instruments because that is all that can be done using the data at the foundation of the literature on taxes and giving: the Treasury Department’s tax return data. Tax return data have many advantages, but do not include data on the giving of families who do not itemize (hence, switchers cannot be observed), do not disaggregate giving into religious and secular categories (hence, religious giving cannot be excluded from price elasticity estimates), and consist mainly of income-based information (hence, there are no other variables that can reasonably serve as alternative instruments in order to check conclusions about how much of the observed price elasticity is a permanent effect).

Using new panel data on charitable giving—the *Center on Philanthropy Panel Study*, a module collecting giving data in the *Panel Study of Income Dynamics (PSID)*—we are able to overcome all three limitations: we estimate the price elasticity of giving among switchers, while not including religious giving in our measure of charitable giving. And we can examine a variety of alternative instruments to see if they can resolve the lack of consensus about the permanent effect of taxes on giving.

Our results, at this point to be regarded as preliminary, indicate that switching deductibility has a large, price-elastic effect on charitable giving. Specifically, the direct answer to the constitutionally-relevant treasury efficiency question is that deductibility causes a larger charitable giving increase than the reduction in tax revenue. In contrast, estimates which impose the restrictions facing other datasets suggest a statistically insignificant price elasticity. In addition, we conclude that giving whose primary purpose is to provide human services or help

the people with basic needs is also price-elastic. Our results are preliminary for several reasons, but the most important reason is that we have not fully implemented our strategy to determine whether how much of the price elasticity we estimate is permanent.

2. Previous Literature

Randolph's (1995) paper is important because it uses panel data on tax returns from 1979-1988, a time period during which there were two major tax policy changes. The panel data allow him to examine changes in giving in response to changes in tax rates, and thereby draw stronger inferences about changes in tax rates causing changes in giving. Randolph's paper is important also because he was the first to point out the distinction between permanent and transitory responses to changes in tax rates. He argued that, because a family can anticipate the effects of pre-announced changes in tax rates and temporarily shift its giving to take advantage of anticipated tax changes (the transitory response), analysts should model the family's anticipation of pre-announced tax changes. Indeed, Randolph concluded that it was his modeling of the anticipation of pre-announced changes in tax rates that led to his result that the permanent price of giving is not elastic.

Auten, Sieg and Clotfelter (2002) also use panel data from essentially the same time period (1979-1993), so why do they reach the opposite conclusion about the permanent price elasticity of giving? The reason is that analysts must make a statistical assumption to identify the permanent response from the transitory response in the data (only the combined permanent + transitory response is observable), and Randolph and Auten et al. make different statistical assumptions to identify the permanent response. Furthermore, Auten et al. do not model a person's anticipation of pre-announced changes in tax rates, arguing that the data suggest people

do not respond to future changes in tax rates.¹

The opposite conclusions reached by Randolph and Auten et al. are a motivation for our present work, but there is another, equally important, motivation: two data limitations have forced Randolph and Auten et al. (and virtually the entire literature on taxes and giving) to ask a different question than the treasury efficiency question that is most policy-relevant. The first data limitation is that tax return data do not allow analysts to study directly the effect of deductibility on giving. The second limitation is that tax return data do not allow analysts to study the kind of charitable giving that the government would be constitutionally permitted to replace should deductibility be eliminated and the increased tax revenue sent directly to the charitable organizations.

Instead of studying directly the effect of deductibility on giving, the taxes-and-giving literature estimates the relationship between tax rates and giving among people who always itemize. However, it is the effect on giving among people switching from not itemizing to itemizing or vice versa—switching the deductibility of giving—that is the direct parallel to the treasury efficiency question: Does deductibility itself cause a larger charitable giving increase than the reduction in tax revenue? Virtually the entire literature on taxes and giving has asked a different question—Does a change in tax rates among people who always itemize cause a larger charitable giving increase than the reduction in tax revenue?—and assumed that the answer to the question about “always itemizers” is the same as would be the answer to the switching-deductibility question. The always-itemizer question has been the focus of the literature because

¹Barrett, McGuirk and Steinberg (1997) and Bakija (2002) make statistical assumptions to identify the permanent price elasticity of giving similar to Randolph’s and also conclude that the permanent price elasticity of giving is not elastic.

tax return data (obviously) cannot measure a family's charitable giving unless the family itemizes.

There is good reason to think that the answer to the always-itemizer question will differ from the answer to the switching-deductibility question. It often happens in empirical modeling that responses to large discrete changes are different than responses to smaller price changes (cf. Solon 1999, fn. 14). Moreover, there is experimental evidence that precisely this may be happening in charitable giving. For instance, Karlin and List (2006) find that matching the amount a person gives does increase giving but that further increases in the match rate do not further increase giving.²

The second limitation of tax return data is that a tax return measures a family's aggregate giving to all the organizations to which it gives. Tax return data do not disaggregate giving into the amounts given to different kinds of charitable organizations. However, one kind of charitable giving—religious giving to churches, synagogues, and mosques for the purposes of sustaining worship and spiritual development—could not be replaced constitutionally by government from the increased tax revenue should deductibility be eliminated, and therefore falls outside the bounds of the treasury efficiency question.

Our present research estimates the tax price elasticity of giving, and makes four contributions. First, we estimate the price elasticity of giving using data from the 2001-2005

²The lowest match rate in Karlin and List's experiment is very high: a 1:1 match, implying a \$.50 price of giving. The \$.50 price is lower than the lowest price (\$.65) currently created by the top federal tax rate, so when Karlin and List examine higher match rates (generating prices lower than \$.50) they are varying the experimental price outside the range created by current tax rates. Nevertheless the experiment illustrates that it may not be acceptable to assume that the answer to the always-itemizer question is the same as the answer to the switching-deductibility question.

waves of the *PSID*. With the *PSID* data we are able to observe families who switch between itemizing and not itemizing (or vice versa), and this lets us ask the switching-deductibility question. Of course, we can also ask the always-itemizer question. We find that the two questions have different answers for some kinds of giving.

Second, we estimate the tax price elasticity for giving to different kinds of charitable organizations. This lets us ask the constitutionally-relevant treasury efficiency question. Also, we estimate the price elasticity of giving to provide human services and to help people with basic needs, an issue of recent policy interest (see, for example, Preston 2007).

Third, work in this area faces methodological constraints, especially in the availability of what economists refer to as “instrumental variables.” The *PSID* provides many more potential instrumental variables other than the income-based instruments used by Randolph and Auten et al.

Fourth, we examine the charitable giving response to tax changes enacted by the Economic Growth and Tax Relief Reconciliation Act of 2001 (H.R. 1836), the Jobs and Growth Tax Relief Reconciliation Act of 2003 (H.R. 2), and the Working Families Tax Relief Act of 2004 (H.R. 1308). We argue that these acts are nearly ideal for estimating the permanent effect of taxes on giving because the acts were passed in a way that mitigated a family’s ability to anticipate tax changes.

3. The 2001, 2003, and 2004 Tax Acts

The Economic Growth and Tax Relief Reconciliation Act of 2001 was signed into law on June 7, 2001 and was effective immediately. The 2001 Act scheduled gradual tax policy changes that would reduce marginal tax rates over the years 2001-2010, and broaden the 15 percent tax

bracket for married-joint filers. Federal marginal tax rates were scheduled to drop one percentage point from their 2000 values of 15, 28, 31, 36, and 39.6 percent to 15, 27, 30, 35, and 38.6 percent in 2002 (plus the creation of a 10 percent rate for low taxable incomes). The gradual drop was to continue: another one point drop in 2004, and again in 2006 a one point drop in the lower brackets and a 2.6 point drop in the top bracket; these 2006 rates were to remain in effect through 2010. The Jobs and Growth Tax Relief Reconciliation Act of 2003 was signed into law May 28, 2003 and made the 2006 rates effective immediately.

We will argue in more detail in Section 4 that the uncertainty surrounding the closeness of the 2000 presidential election offered little chance for families to alter their giving in 2000 in anticipation of tax changes that, from their 2000 perspective, might occur in 2001. Whether or not our argument that the 2001 Act could not have been anticipated in 2000, it is clear that the 2003 Act could not have been anticipated when people were doing their giving in 2002. More importantly, the 2003 Act considerably weakened incentives to shift giving from the future to 2003 (“give more now because tax rates will be lower in the future”) because rates were lowered immediately to the levels originally designated for 2006 (15, 25, 28, 33, and 35 percent) and were to remain at these levels until 2010.

Table 1 shows the federal tax rates in effect from 2000 through 2004. The tax rate changes induced changes in the price of giving for families that itemize, and Table 1 also shows the prices in effect over 2000-2004 by 2000 income bracket. Between 2000 and 2002 itemizers in the lowest bracket saw a 5.9 percent increase in their price of giving, itemizers in the next bracket saw no change in price, and the other itemizers saw increases from 1.4 percent (middle bracket) to 1.7 percent (highest bracket). Although the 2000-2002 percentage price increases seen by middle-to-high bracket families were modest and fairly uniform, the percentage price

increases in effect by 2004 were larger: from 4.2 percent at middle brackets to 7.6 percent at the highest bracket. Figure 1 shows how prices varied as a function of taxable income in 2000, 2002, and 2004. Unlike Table 1, Figure 1 shows the additional variation caused by changes in brackets.

The 2001 Act scheduled other changes that would have led families in 2002 to expect higher future prices and also higher after-tax incomes. The 2001 Act scheduled a gradual increase in the standard deduction available to married-joint filers relative to single filers (the other part of what the Act did to eliminate the “marriage penalty”; typically standard deductions increase year-to-year for both types of filers, but only nominally). In 2000 the married-joint standard deduction was 167 percent of the single standard deduction, but the 2001 Act scheduled the married-joint standard deduction to increase to 174 percent (of the single standard deduction) in 2005 and then increase in steps reaching 200 percent in 2009. Increasing the standard deduction increases the price of giving to the extent it induces married families to switch from itemizing deductions to claiming the higher standard deduction, but the standard deduction increases were not scheduled to begin until 2005.

However, the 2003 Act immediately increased the married-joint standard deduction to 200 percent for the 2003 and 2004 tax years, but the immediate increase was also temporary because the 2005 married-joint standard deduction would revert back to 174 percent (as originally designated by the 2001 Act) in 2005 and resume its gradual increase to reach 200 percent in 2009. Note that the 2003-2004 increase in the married-joint standard deduction could not have been anticipated by families in 2002 and hence likely had no effect on 2002 giving. However, some families would have expected the price of giving to be lower in 2005 (when the married-joint standard deduction dropped back to 174 percent), and may have been delaying

giving they would have otherwise done in 2004 in order to take advantage of the lower price of giving in 2005.

Such expectations and incentive to delay giving were eliminated by the Working Families Tax Relief Act of 2004 signed into law on October 4, 2004. The 2004 Act kept the married-joint standard deduction at 200 percent for 2005-2010. Families no longer expected the 200 percent married-joint standard deduction to end in 2005. Had a family been delaying its giving from 2004 to 2005 (anticipating that the price of giving would be lower in 2005) it would have had from October 4 until the end of the year to undo the delay.

The 2001, 2003, and 2004 Tax Acts created a time pattern of changes in the upper threshold for the 15 percent bracket for married joint filers that paralleled the time pattern of changes in the married-joint standard deduction just described. The 2001 Act scheduled a gradual increase in the upper threshold from 167 percent of the single-filer upper threshold: the increase was to start in 2005 and reach 200 percent in 2008. The 2003 Act immediately and temporarily increased the threshold to 200 percent of the single-filer upper threshold for 2003 and 2004, but the 2004 Act kept the threshold at 200 percent for 2005-2010.

The 2001, 2003, and 2004 Tax Acts created a similar time pattern of changes in the child tax credit. The 2001 Act scheduled a gradual increase in the child credit from its 2000 level of \$500 to \$1,000 in 2010. The 2003 Act immediately and temporarily increased the credit to \$1,000 for 2003 and 2004, but then the 2005 credit was to revert back to \$700, the level designated by the 2001 Act. The 2004 Act kept the child credit at \$1,000 for 2005-2010.

4. An Empirical Model of Giving as a Function of Tax Policy

In this section we describe an equation which depicts charitable giving as a function of a

number of other variables; in the following sections we will empirically estimate the parameters of this equation. Our baseline equation is:

$$g_{it} = \tau_1 (p_{it} - E(p_{it+1} | I_t)) + \pi_1 E(p_{it+1} | I_t) + \theta_1 (y_{it} - E(y_{it+1} | I_t)) + \varphi_1 E(y_{it+1} | I_t) + \mathbf{X}_{it}' \boldsymbol{\beta} + \alpha_i + \alpha_0 t + u_{it} \quad (1)$$

where g_{it} is the giving done by family i in year t , p_{it} is family i 's price of giving (one minus the marginal tax rate), y_{it} is after-tax permanent income. The term $E(p_{it+1} | I_t)$ represents family i 's expectation in year t of what its price of giving will be in year $t + 1$ based on the information I_t the family has in year t . $E(y_{it+1} | I_t)$ is the expectation of next year's after-tax permanent income. The term α_i represents unobservable and time-invariant characteristics of family i that influence its giving and may be correlated with its price and income (e.g., unobservable preference characteristics such as generosity and religiosity). The term $\alpha_0 t$ is a time trend capturing macro effects. Other observable characteristics that may influence giving (e.g., age, marital status, number of dependents, education, wealth) are collectively referred to as the vector \mathbf{X}_{it} . The main variables (g_{it} , p_{it} , and y_{it}) are in logarithms so that the τ_1 , π_1 , θ_1 , and φ_1 parameters are elasticities: τ_1 is the transient price elasticity and π_1 is the permanent price elasticity.

Likewise, θ_1 and φ_1 are the transient and permanent income elasticities.

Equation (1) embodies the idea developed by Randolph (1995), Barrett et al. (1997), and Bakija (2002) that expectations of future prices and incomes can affect present giving. For example, controlling for the expected future price of giving $E(p_{it+1} | I_t)$, the current price affects giving only to the extent that it deviates from the expected future price: $p_{it} - E(p_{it+1} | I_t)$. In our conceptual framework the term $E(p_{it+1} | I_t)$ captures the notion of a long-run price—the expected

price once all known tax policy changes are implemented and no further policy changes are expected. In this sense $E(p_{it+1} | I_t)$ captures “permanent” price effects and $p_{it} - E(p_{it+1} | I_t)$ captures “transitory” price effects from the family’s adjusting the timing of its giving to take advantage of pre-announced changes in tax rates that affect $E(p_{it+1} | I_t)$.

The impetus to model permanent and transitory price effects comes from historical experiences such as the 1986 Tax Reform Act: it was known in October of 1986 that the price of giving would rise in 1987 and families had plenty of time to shift to 1986 giving originally intended to have been done in 1987 (Bakija 2002). And families did shift a lot of giving to 1986 that would otherwise have been done in 1987 (Clotfelter 1990; Auten, Cilke and Randolph 1992). If the transitory shift from 1987 to 1986 is not modeled, all of the drop in giving from 1986 to 1987 (Figure 2 shows the drop in charitable deductions as a percentage of personal income) would be erroneously attributed to the permanent change in price effected by the 1986 Act.

Equation (1) does not include terms such as $\pi_s (p_{it} - E(p_{it+s} | I_t))$ for $s = 2, 3, \dots, T$. Rather, equation (1) models a year-to-year intertemporal substitution, but implicitly assumes that substitution across two-year, three-year, ..., T-year time spans is zero—an assumption routinely made in previous work. We will maintain this assumption in our theoretical discussion, but check the assumption when we conduct the empirical work [yet to be done]. The assumption that substitution across two-plus-years is zero, or at least negligible, is our first identifying assumption.

Our second identifying assumption is that, holding family i ’s income (y_{it}) and other characteristics (X_{it}) constant, $E(p_{it+1} | I_t, y_{it}, X_{it}) = p_{it}$ for $t = 2000, 2002, \text{ and } 2004$ —that is, the family expects next year’s price of giving to be the same as this year’s price, as long as the

family's income and other characteristics are held constant. By holding income and characteristics constant the assumption means that a family in, say, 2000 is expecting no change in tax policy that would set up an incentive for it to shift giving across years. Before discussing why we think the assumption is reasonable, we point out how we use the assumption: if $E(p_{it+1} | I_t, y_{it}, X_{it}) = p_{it}$ for $t = 2000, 2002, \text{ and } 2004$ then the transient price term $p_{it} - E(p_{it+1} | I_t, y_{it}, X_{it})$ drops out of (1) and the permanent elasticity π_1 can be identified with data on the (observable) p_{it} . Essentially, our assumption is that changes in price caused by the Tax Acts “surprised” families and hence can be used to identify the permanent elasticity by estimating the response before and after the point of surprise.

The argument about why it is reasonable to assume $E(p_{it+1} | I_t, y_{it}, X_{it}) = p_{it}$ for $t = 2000, 2002, \text{ and } 2004$ is detailed in Appendix A. The Appendix A detailed argument is lengthy because we have to consider all possible shifting—e.g., 2000←2001, 2001←2002, 2002←2003, 2003←2004, etc.—based on what a family knew in advance about each of the 2001, 2003, and 2004 Tax Acts. Although the details of the argument are lengthy, our main point is this: each of the three Tax Acts surprised families so that a family could not take advantage of incentives to shift giving from the future into the present or from the present to the future.

For example, part of our argument is that the Bush-Gore election was too close to predict and was decided too late in the year (December 13, 2000) to allow a family to shift much giving 2000←2001. Some may dispute this, arguing that 18 days was enough time for family to shift giving 2000←2001, but recall that much 2000 giving would have been delayed to the end of the year, making it less likely that additional giving would have been shifted 2000←2001. In any event, the time series evidence in Appendix A offers, at best, only weak evidence that 2000←2001 shifting occurred. Another part of our argument is that the incentive to shift giving

2003←2004 shift was completely erased by the 2003 Act. We think this part of the argument cannot be disputed.

5. *The Center on Philanthropy Panel Study*

We use data describing charitable giving from the *Center on Philanthropy Panel Study*. The *Center Panel* data are of very high quality compared to other survey data on giving in that the reported amounts compare well to the distribution of charitable deductions obtained from tax return data, except above the 90th percentile (Wilhelm 2006). Other survey data lose their comparability to tax data at much lower percentiles and, in addition, suffer from serious missing data problems (Wilhelm 2007).

The giving–income profile and the within-income class frequency distribution of giving as a percentage of income measured in the *Center Panel* and in tax data are fairly similar, again except at the top of the income distribution (Wilhelm 2005). At the top of the income distribution, giving measured by the *Center Panel* is lower than charitable deductions from tax return data because random sample surveys do not pick up enough families at the very top of the income distribution to produce a precise estimate for the giving of very high-income families. Juster, Smith and Stafford (1999) make the same point about the *PSID*'s measure of wealth. So while the *Center Panel* compares well to tax return data it is important to keep in mind the difference at the very top when thinking about the results.

The data we use to estimate (1) differ in three ways from the data from federal income tax returns used in most studies of taxes and giving. The first difference between our study and most previous studies is that our sample includes non-itemizers as well as itemizers.

The second difference is that the *Center Panel* builds up a measure of aggregate giving

comparable to tax return data from a series of questions a respondent answers about amounts given to organizations for religious purposes (i.e., to churches, synagogues, mosques, TV and radio ministries for purposes of worship or spiritual development), combined purposes (e.g., United Way), basic needs (e.g., for people in need of food and shelter), health, education, and a catch-all “other” category that includes giving to youth and family services, the arts, environmental protection, neighborhood and community organizations, international relief, and any other purpose the respondent mentions. The questionnaire asks the respondent to report religious giving separately from all other giving by instructing her to “not include donations to schools, hospitals, and other charities run by religious organizations” in reporting religious giving because donations for these types of purposes will be asked about later. We combine the four main types of (non-religious) giving—combined purposes, basic needs, health, and education—into a “secular” giving variable, and use the estimated tax price elasticity of this secular giving variable to answer the constitutionally-relevant treasury efficiency question. Also, we present price elasticities for giving that is mostly redistributive—giving to combined purposes and to help people with basic needs.³ Finally, we present price elasticities for religious giving and aggregate giving (religious plus secular). Tax price elasticities for aggregate giving allow us to compare our findings with previous work based on tax return data.

The third difference is that because the *Center Panel* is a module within the *PSID*, there is a much richer set of control variables available than in tax-return based studies. There are variables describing education, religious affiliation, religious attendance, wealth (including home

³Most giving for combined purposes goes to human service organizations, and many of those served by human service organizations have low incomes. Brown and Rooney (2007) estimate that 77 percent of giving for combined purposes serves people with low income.

value and homeownership even for families no longer paying mortgage interest), wages, health, etc. The rich set of control variables allows us to consider instruments besides the income-based instruments typically used in tax return-based studies. The rich set of control variables also allows us to check for omitted variable bias in ways not possible with tax data.

We use the *PSID*'s rich set of variables describing earnings, capital income, transfer income, housing expenses (monthly payments, term of loan, property tax), child care expenses, and demographics to estimate tax rates and payments using NBER's TAXSIM (Feenberg and Coutts 1993). The only input to TAXSIM that is not available from the *PSID* is realized capital gains, so we set capital gains to zero when estimating tax rates and payments. If any of the other variables necessary for TAXSIM are missing, we use simple imputation techniques to fill in the missing information. In calculating the tax payments TAXSIM incorporates state as well as federal tax law and handles phase-outs of deductions and credits. These two features generate more variation in prices of giving than displayed by the six brackets in Figure 1.

The sample we use contains families from the *PSID*'s nationally representative subsample (i.e., dropping the low-income oversample) who did not experience a major change in family structure across the 2001, 2003 and 2005 interviews, meaning that single family heads in 2001 were still single in 2005 and couples in 2001 were still couples in 2005 (couples could be unmarried cohabitators). The "no-major-family-structure-change" sample (henceforth, "stable" families) ensures that our results are not affected by family structure changes that would greatly alter family finances; however we note that tax return-based studies do not restrict their attention to stable families, and that may be a problem.⁴ Sixty-five percent of the *PSID* families from the

⁴The potential problem is that after a divorce, income often falls because there is one less earner in each of the two family units. The income fall may cause a drop in the tax rate and a rise

nationally representative subsample are 2001-2005 stable families (3,382 out of 5,175). We drop 73 families who were not asked the *Center Panel* questions (because the respondent was neither the head or wife, usually because the head or wife was physically or cognitively unable to respond to the survey) and 22 families because their after-tax income was zero or negative.⁵ The sample is $n = 3,287$.

There are two ways to determine itemizers in the sample. First, the *PSID*'s income module directly asks the respondent if the family itemizes deductions. However, there is some measurement error in the responses, especially among low-income respondents (too many of whom respond that they itemize; Ackerman and Auten 2006). Second, TAXSIM output includes whether the family itemizes based on variables in the *PSID*. A key variable used by TAXSIM, the mortgage interest payment, is itself estimated from several *PSID* variables, and likely contains measurement error. Despite these sources of measurement error, the *PSID* direct question about itemization agrees with the TAXSIM determination of itemization for 80 percent of the sample.

The effects of the measurement errors in the two determinations of itemization can be particularly harmful if they lead us to mis-identify those who switch itemization status.

Therefore, we attempt to reduce the effects of the measurement errors by including in our analysis only families for whom the *PSID* direct question about itemization agrees with the TAXSIM determination of itemization. This leaves us with a sample of “verified” never-

in the tax price of giving. If giving also falls it will be difficult to separate out the effect of the divorce from the effect of the rise in tax price. Rooney, Brown and Wu (2007) find that divorce reduces donations in the years immediately following the divorce.

⁵The terms “head” and “wife,” though outdated, are standard *PSID* terminology and have precise, well-understood meaning to *PSID* users.

itemizers, always-itemizers, and switchers.

Finally, we only include verified always-itemizers and switchers in our analysis if they would have itemized even if their charitable giving had been zero—so-called “exogenous itemizers.” The people we exclude—those who TAXSIM determines itemized because their charitable giving is so high—likely are making their itemization and giving choices simultaneously (“endogenous itemizers”) and including them would bias our estimates toward the finding that giving is price elastic.

Table 2 presents a first look at the data by presenting summary statistics for 2000 and 2004. The sample consists of $n = 985$ verified never-itemizers (families that did not itemize in either 2000 or 2004, as determined by both the *PSID* direct question and the TAXSIM itemization determination), $n = 186$ verified switchers (families that itemized in either 2000 or 2004, as determined by both the *PSID* and TAXSIM, and TAXSIM determines their itemization to be exogenous), and $n = 777$ verified always-itemizers (families that itemized in both 2000 and 2004, as determined by both the *PSID* and TAXSIM, and TAXSIM determines their itemization to be exogenous). Among the switchers, $n = 104$ went from not itemizing in 2000 to itemizing in 2004 (“0-to-1 switchers”) and therefore experienced a drop in the price of giving. The remaining $n = 82$ switchers switched in the other direction (“1-to-0 switchers”) and experienced a price rise. Among the $n = 777$ always-itemizers, $n = 220$ experienced a price drop and the remaining $n = 557$ experienced a price rise.

Table 2 begins with the price of giving the first dollar to a charitable organization, the “first-dollar price.” For the never-itemizers, the price of giving is, of course, \$1.00 in both years. Among the switchers the average price drops from .90 to .87, a small $-.03$ drop. But this small drop masks large price drops and large price rises within this group. For the 0-to-1 switchers, the

first-dollar price of giving drops on average from \$1.00 to \$0.77, a 23 percent price drop. For the 1-to-0 switchers, the price of giving rises on average 23 percent. For the always-itemizers, the price rises from \$0.71 to \$0.74, an average increase of four percent. Among the always-itemizers experiencing a price drop, the price drops five percent, and among the always-itemizers experiencing a price rise, the price rises nine percent on average. Not surprisingly, the switchers experience larger price changes than do always-itemizers. The last row in this section presents the price change for all the observations in the sample.

Table 2 continues with the “first-dollar after-tax income”—that is, the family’s income minus taxes owed before tax reductions due to charitable giving are calculated. For never-itemizers, after-tax income increases by \$1,315. For 0-to-1 switchers the average income increase is much larger: \$22,601. The income drop among 1-to-0 switchers is also large: -\$11,544. Always-itemizers have a \$9,643 increase on average. Among the always-itemizers experiencing a price drop, after-tax income increases \$29,668, but among the always-itemizers experiencing a price increase, after-tax income increases only \$1,733.

We note that for both 0-to-1 switchers and always-itemizers experiencing a price drop, the average income increase is large. Also, for 1-to-0 switchers the average income decrease is large. This means that for much of the sample experiencing a price change, income is changing in the opposite direction, with the implication that the effects on giving of changes in price are hard to distinguish from changes in income. This is a well-known problem in the taxes and giving literature. Another reason we regard our present results as preliminary is that we have some ideas about how to distinguish price effects from income effects, but we have not yet implemented these ideas.

The next section in Table 2 presents secular giving, the giving relevant for the treasury

efficiency question. Secular giving among never-itemizers decreases by \$10 between 2000 and 2004. Among 0-to-1 switchers secular giving increases \$210, but among 1-to-0 switchers the giving change is negligible. Among always-itemizers experiencing a price drop, secular giving increases by \$250, and even among always-itemizers experiencing a price rise giving increases a little (\$43). Giving to help people with basic needs in the next section shows a similar pattern.

The next section of Table 2 presents religious giving. Religious giving increases \$344 among 0-to-1 switchers, drops -\$130 among 1-to-0 switchers, increases \$342 among always-itemizers experiencing a price drop, and increases a little (\$85) among always-itemizers experiencing a price rise. The religious giving pattern is qualitatively similar to the secular giving pattern, with the exception that among 1-to-0 switchers religious giving drops (-\$130) whereas the secular giving change is negligible (\$1). However, the magnitudes of the religious giving changes are larger than the secular giving changes. The larger responsiveness of religious giving compared to secular giving in these summary statistics lends credence to our concern that price elasticities estimated with aggregate giving may be influenced by changes in religious giving that fall outside the bounds of the treasury efficiency question.

The final section in Table 2 presents aggregate giving (secular + religious). Aggregate giving among 0-to-1 switchers increases \$719, and among 1-to-0 switchers decreases -\$143. Among always-itemizers experiencing a price drop aggregate giving increases \$721, but even among always-itemizers experiencing a price rise giving increases \$225.

6. Results

Table 3 contains our main results. The dependent variable in each regression is the 2004-minus-2000 change in the log secular giving. The independent variables are the 2004 minus

2000 changes in the log tax price (first-dollar), the log after-tax income (first-dollar), marital status, over-65 exemptions, and the number of dependents. The first column shows the results for the switchers, and column 2 shows the results for the always-itemizers. For both switchers and always-itemizers we include families only if they gave to a charitable organization in at least one of the two years 2000 or 2004 (or in both years): $n = 140$ out of the 186 switchers and $n = 717$ out of the 777 always-itemizers gave in at least one of the two years. We present ordinary least-squares estimates. In the future version of the paper we will use more advanced methods to account for families giving zero in one or both of the years.⁶ We expect the advanced methods to produce changes in the results, but only minor changes. But for the present, our results should be interpreted as applying only to families that give in at least one year.

Column 1 shows that the estimate of the price elasticity of giving among switchers is -2.00 (s.e. = $.70$). This large price response is our estimate of the deductibility-switcher elasticity that should be used to evaluate treasury efficiency and, further, the secular giving dependent variable means that the estimate can be used to evaluate constitutionally-relevant treasury efficiency. The large point estimate implies that deductibility is treasury efficient. The 95 percent confidence interval is -3.39 to $-.62$, meaning that although elasticities less than one cannot be categorically ruled out, most of the interval lies to the left of -1 : the hypothesis that the elasticity is less than or equal to -1 cannot be rejected.

The estimate of the price elasticity of giving among always-itemizers in column 2 is

⁶Of course, by including families only if they gave in 2000, 2004, or both we are selecting on the dependent variable and thereby potentially introducing sample selection bias. In the future version we will use Honoré's (1992) fixed-effects Tobit to properly handle the zero-giving years. For the present we exclude families who give zero in both years because our first-difference, least-squares regression treats a never-giving family as if there was no change in their giving, just like a family who gives \$500 in both years.

qualitatively similar, though smaller in magnitude: -1.45 (s.e. = .93). The 95 percent confidence interval is wider: zero price responsiveness cannot be ruled out at conventional significance levels, but 10 percent significance is just missed (p -value = .12). Again, the hypothesis that the elasticity among always-itemizers is less than or equal to -1 cannot be rejected.

Table 4 presents estimates from models similar to those in Table 3, but for different definitions of charitable giving as the dependent variables. The specification of the independent variables is identical to the independent variables in Table 3, but Table 4 presents only the tax price elasticity estimates. Hence, each estimate in Table 4 is a price elasticity from a different regression. Column 1 contains the regressions for switchers and column 2 contains the regressions for always-itemizers.

Row 1 repeats Table 3's price elasticities for secular giving to allow easy comparison. In row 2, the charitable giving is that for combined purposes and for helping people with basic needs, giving that is mostly redistributive. The price elasticity estimates among both switchers and always-itemizers are similar to their row 1 counterparts. The price elasticities for religious giving in row 3 are different: the estimated elasticity among switchers is -2.50 (s.e. = 1.08), somewhat larger in magnitude than the secular elasticity. The estimated elasticity among always-itemizers is $-.77$ (s.e. = 1.05), smaller but not significantly different from the secular elasticity.

Row 4 presents the estimates for aggregate giving (secular plus religious). Among switchers the estimate is -2.02 (s.e. = .52), still indicating a large price response. In contrast, among always-itemizers the estimate is $-.73$ (s.e. = .60). This is the estimate that most closely mimics the previous literature—it answers the always-itemizer price elasticity question rather

than the deductibility-switcher question and it includes religious giving rather than focus on the constitutionally-relevant treasury efficiency question. Although the hypothesis that the always-itemizer elasticity is less than or equal to -1 cannot be rejected, the $-.73$ point estimate itself suggests that deductibility is not treasury efficient, similar to the conclusion reached by Randolph (1995) if one interprets the $-.73$ estimate as an estimate of the permanent elasticity. Comparing the $-.73$ estimate to the column 1, row 1 estimate of -2.00 indicates that asking the deductibility-switcher question and asking about the kind of giving that is constitutionally-relevant in terms of the treasury efficiency question does matter.

7. Discussion and Conclusion

Most of the previous literature on taxes and giving has estimated the price elasticity of aggregate giving (including religious giving) using samples of families who always itemize. The previous literatures' price elasticity estimates are then used to infer indirectly the price elasticity of constitutionally-relevant giving (excluding religious giving) for families who switch itemization status. Using new panel data on charitable giving—the *Center on Philanthropy Panel Study*—we are able to directly estimate the price elasticity of constitutionally-relevant giving on a sample of families who actually switch itemization status. In addition, we follow the procedures of the previous literature by estimating the price elasticity of aggregate giving using a sample of families who always itemize.

The estimates from the two procedures are different. The direct estimate of the price elasticity of constitutionally-relevant giving indicates that the tax deductibility of giving is treasury efficient—giving induced by deductibility is greater than the foregone tax revenue that

the government would be constitutionally allowed to send to the charitable organizations. In contrast, the estimate of the price elasticity of aggregate giving using a sample of families who always itemize is smaller—if this estimate were used to infer whether the deductibility of constitutionally-relevant giving is treasury efficient, we would conclude that deductibility is not treasury efficient. Nevertheless, the price elasticity of aggregate giving among always-itemizers answers an important policy question: How did the tax rate cuts in the 2001-2004 Tax Acts affect charitable giving? Our preliminary result is that among always-itemizers, charitable giving changed little: the effects on giving of the tax price increase caused by cutting tax rates and the income increase caused by raising after-tax incomes offset each other.

Our results are preliminary for several reasons, but the most important reason is that we have not implemented our strategy to determine how much of the price elasticity we estimate is a permanent response. The other reasons our results are preliminary form a list of additional tasks we will do in the future version of the paper:

- use instrumental variable methods to identify the permanent effect of taxes on giving, and to mitigate the measurement error in itemization status
- properly handle the econometrics of the zero-giving years
- check the sensitivity of the results to the use of controls available in the *PSID* but not in the tax return data.

Although our results are preliminary, they provide an early indication that the data advantages delivered by the *Center on Philanthropy Panel Study* allow us to ask policy questions that previously could not be asked. The early answers to those questions suggest that the data advantages matter. Substantively, the early answers suggest that deductibility induces more giving to the charitable organizations constitutionally eligible for government funding than the

government itself could provide by eliminating deductibility and sending the increased tax revenues directly to the organizations.

References

- Ackerman, Deena and Gerald Auten. 2006. "Floors and Ceilings, and Opening the Door for a Non-Itemizer Deduction." *National Tax Journal* 59(3): 509-530.
- Auten, Gerald and Gabriel G. Rudney. 1986. "Tax Reform and Individual Giving to Higher Education." *Economics of Higher Education Review* 5:167-78.
- Auten, Gerald E., Holger Sieg and Charles T. Clotfelter. 2002. "Charitable Giving, Income, and Taxes: An Analysis of Panel Data." *American Economic Review* 92 (1): 371-382.
- Barrett, Kevin Stanton, Anya M. McGuirk and Richard Steinberg. 1997. "Further Evidence on the Dynamic Impact of Taxes on Charitable Giving." *National Tax Journal* 50(2): 321-334.
- Bakija, Jon. 2002. "Distinguishing Transitory and Permanent Price Elasticities of Charitable Giving with Pre-Announced Changes in Tax Law." Mimeo, Williams College.
- Brown, Melissa and Patrick M. Rooney. 2007. "How Much Charitable Giving Addresses the Needs of the People Most in Need?" Paper presented at the 37th Annual Meeting of the Association for Research on Nonprofit and Voluntary Activity, Atlanta, November 15-17.
- Feenberg, Daniel R. 1987. "Are Tax Price Models Really Identified? The Case of Charitable Giving." *National Tax Journal* 40(4): 629-633.
- Feenberg, Daniel R. and Elisabeth Coutts. 1993. "An Introduction to TAXSIM." *Journal of Policy Analysis and Management* 12(1): 189-194.
- Giving USA*. 2007. New York: AAFRC Trust for Philanthropy.
- Honoré, Bo E. 1992. "Trimmed LAD and Least Squares Estimation of Truncated and Censored Regression Models with Fixed Effects." *Econometrica* 60: 533-565.
- Juster, F. Thomas, James P. Smith and Frank Stafford. 1999. "The Measurement and Structure of Household Wealth." *Labour Economics* 6: 253-275.
- Karlin, Dean and John A. List. 2006. "Does Price Matter in Charitable Giving? Evidence from a Large-Scale Natural Field Experiment." Mimeo, University of Chicago.
- Preston, Caroline. 2007. "Charity Leaders Say Antipoverty Efforts Need to be Higher Priority for Nonprofit World." *Chronicle of Philanthropy* (online article, October 24, 2007).
- Randolph, William C. 1995. "Dynamic Income, Progressive Taxes, and the Timing of Charitable Contributions." *Journal of Political Economy* 103(4): 709-738.

Rooney, Patrick M., Melissa Brown and Ke Wu. 2007. "Donor Stability." Paper presented at the 37th Annual Meeting of the Association for Research on Nonprofit and Voluntary Activity, Atlanta, November 15-17.

Solon, Gary. 1999. "Intergenerational Mobility in the Labor Market." In O. Ashtenfelder and D. Card (eds.) *Handbook of Labor Economics* Volume 3. Amsterdam: Elsevier Science: 1761-1800.

Wilhelm, Mark O. 2005. "Basic Facts about Charitable Giving from the *Center on Philanthropy Panel Study*." Mimeo, IUPUI.

_____. 2006. "New Data on Charitable Giving in the *PSID*." *Economics Letters* 92(1): 26-31.

_____. 2007. "The Quality and Comparability of Survey Data on Charitable Giving." *Nonprofit and Voluntary Sector Quarterly* 36 (1): 65-84.

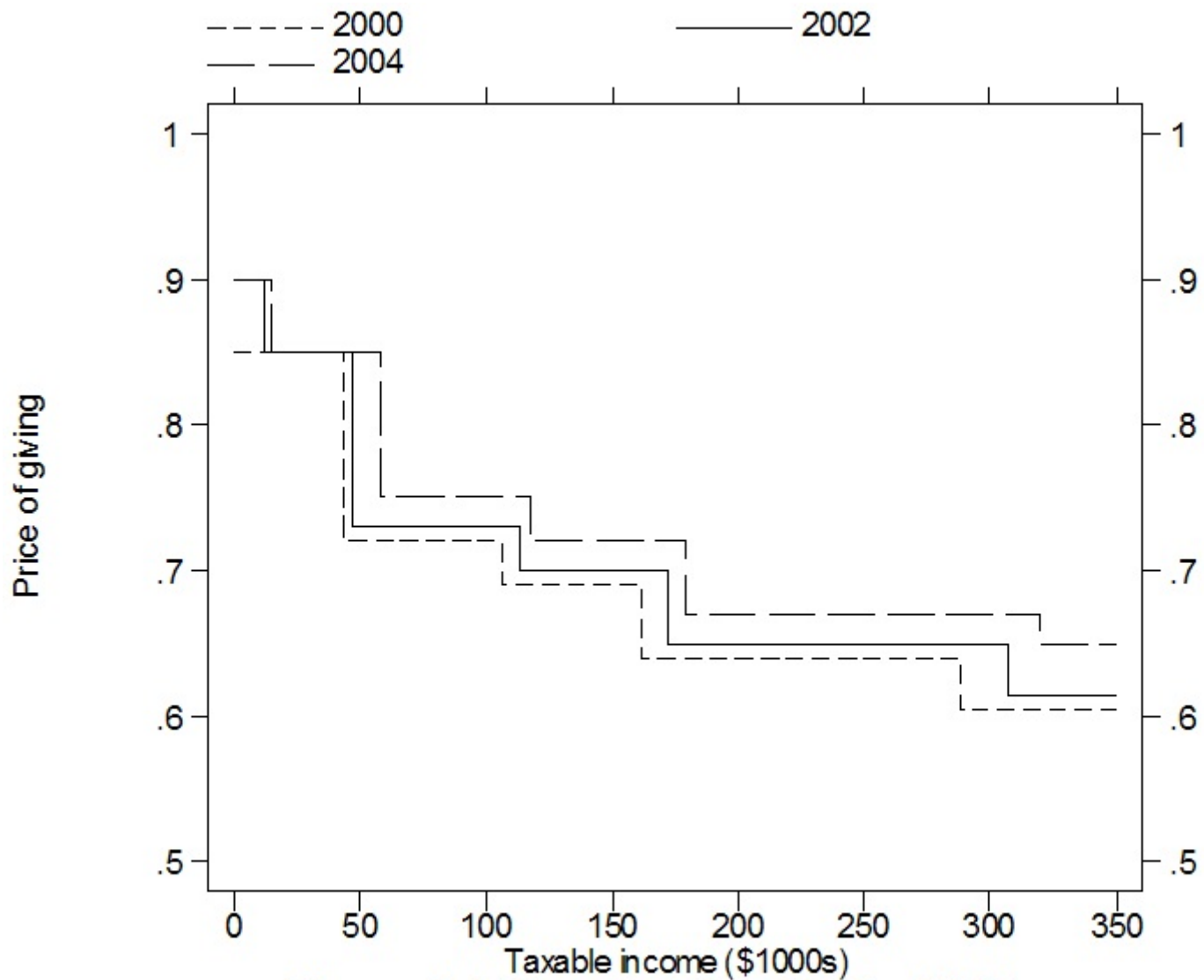


Figure 1. Price of giving 2000 - 2004.

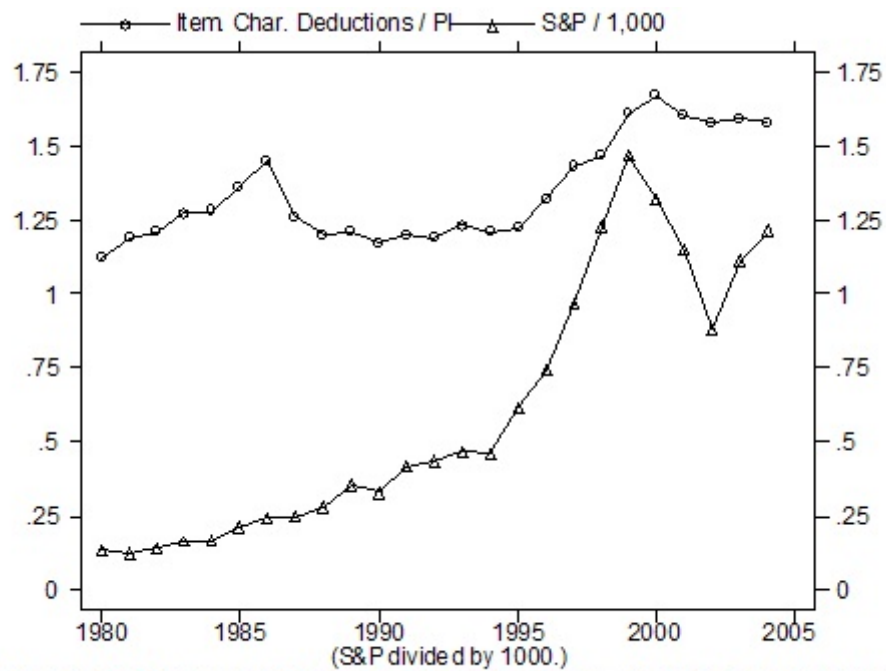


Figure 2. Itemized Charitable Deductions / Personal Income.

Table 1. Federal Tax Rates and the Price of Giving in Effect from 2000–2004.

2000 Income bracket	Year				
	2000	2001	2002	2003	2004
	Tax Rates (percent)				
\$0 to \$11,999 ^a	15	15	10	10	10
\$12,000 ^a to \$43,849	15	15	15	15	15
\$43,850 to \$105,949	28	27.5	27	25	25
\$105,950 to \$161,449	31	30.5	30	28	28
\$161,450 to \$288,349	36	35.5	35	33	33
\$288,350 and over	39.6	39.1	38.6	35	35
	Price of giving for itemizers (cost of a one dollar gift)				
\$0 to \$11,999	.85	.85	.90	.90	.90
\$12,000 to \$43,849	.85	.85	.85	.85	.85
\$43,850 to \$105,949	.72	.725	.73	.75	.75
\$105,950 to \$161,449	.69	.695	.70	.72	.72
\$161,450 to \$288,349	.64	.645	.65	.67	.67
\$288,350 and over	.604	.609	.614	.65	.65
	Percentage increase in price 2000 to 2002				
\$0 to \$11,999	-----	5.9	-----		
\$12,000 to \$43,849	-----	0	-----		
\$43,850 to \$105,949	-----	1.4	-----		
\$105,950 to \$161,449	-----	1.4	-----		
\$161,450 to \$288,349	-----	1.6	-----		
\$288,350 and over	-----	1.7	-----		

			Percentage increase in price 2000 to 2004		
\$0	to	\$11,999	-----	5.9	-----
\$12,000	to	\$43,849	-----	0	-----
\$43,850	to	\$105,949	-----	4.2	-----
\$105,950	to	\$161,449	-----	4.3	-----
\$161,450	to	\$288,349	-----	4.7	-----
\$288,350	and	over	-----	7.6	-----

Notes: The table does not describe the shifts in brackets that occurred between 2000 and 2004; see Figure 1.

^a This bracket became effective in 2002.

**Table 2. Sample Means for Never-Itemizers, Always-Itemizers, and Switchers:
2000 and 2004.**

Variable (in bold) and sample	2000	2004	Difference (2004 - 2000)
Price			
Never-itemizers	1.00	1.00	.00
Switchers	.90	.87	-.03
Did not itemize in 2000, but itemized in 2004 (price drops)	1.00	.77	-.23
Itemized in 2000, but did not itemize in 2004 (price rises)	.77	1.00	.23
Always-itemizers	.71	.74	.03
Price drops	.76	.72	-.04
Price rises	.68	.74	.06
All	.87	.88	.01
After-tax income			
Never-itemizers	31,845	33,179	1,315
Switchers	58,027	65,575	7,548
Did not itemize in 2000, but itemized in 2004 (price drops)	52,184	74,785	22,601
Itemized in 2000, but did not itemize in 2004 (price rises)	65,438	53,894	-11,544
Always-itemizers	103,561	113,204	9,643
Price drops	83,953	113,620	29,668
Price rises	111,306	113,039	1,733
All	62,950	68,210	5,234

	2000 mean	2004 mean	Difference
Secular giving			
Never-itemizers	119	110	-10
Switchers	252	370	118
Did not itemize in 2000, but itemized in 2004 (price drops)	204	414	210
Itemized in 2000, but did not itemize in 2004 (price rises)	314	315	1
Always-itemizers	1,041	1,143	102
Price drops	797	1,047	250
Price rises	1,138	1,182	43
All	500	547	47
Combined purpose and basic needs giving			
Never-itemizers	93	81	-12
Switchers	205	293	88
Did not itemize in 2000, but itemized in 2004 (price drops)	172	325	153
Itemized in 2000, but did not itemize in 2004 (price rises)	246	251	5
Always-itemizers	717	762	45
Price drops	609	752	142
Price rises	760	766	6
All	353	373	20

	2000 mean	2004 mean	Difference
Religious giving			
Never-itemizers	306	351	45
Switchers	275	409	135
Did not itemize in 2000, but itemized in 2004 (price drops)	228	572	344
Itemized in 2000, but did not itemize in 2004 (price rises)	333	203	-130
Always-itemizers	1,558	1,716	159
Price drops	1,029	1,372	342
Price rises	1,767	1,852	85
All	803	901	99
Aggregate giving			
Never-itemizers	447	500	53
Switchers	567	907	339
Did not itemize in 2000, but itemized in 2004 (price drops)	451	1,170	719
Itemized in 2000, but did not itemize in 2004 (price rises)	715	572	-143
Always-itemizers	2,831	3,196	365
Price drops	1,970	2,691	721
Price rises	3,171	3,395	225
All	1,409	1,614	205

Notes: All the entries in the table are in dollars. All the observations ($n = 1,948$) have verified itemization status in both 2000 and 2004. Also, all the itemizers are exogenous itemizers. The sample is split into three groups: never-itemizers, switchers, and always-itemizers. Switchers are also split into two groups: those who do not itemize in 2000 and switch to itemizing in 2004 (price drops for them), and those who itemize in 2000 and switch to not itemizing in 2004 (price rises for them). Similarly, always-itemizers are also split into two groups: those for whom the price drops from 2000 to 2004, and those for whom the price rises.

(notes continue on the next page)

Sample sizes are:

Never-itemizers	$n = 985$
Switchers	$n = 186$
Did not itemize in 2000, but itemized in 2004	$n = 104$
Itemized in 2000, but did not itemize in 2004	$n = 82$
Always-itemizers	$n = 777$
Price drops from 2000 to 2004	$n = 220$
Price rises from 2000 to 2004	$n = 557$

Table 3. The 2004-2000 Change in Log Secular Giving.

Independent variables	Switchers	Always-itemizers
Log price (first-dollar)	-2.00*** (.70)	-1.45 (.93)
Log after-tax income (first-dollar)	.26 (.25)	.41* (.21)
Married	.03 (.75)	.91 (.74)
Age 65 or older	-.91 (.97)	-.75* (.42)
Number of dependents	.46* (.27)	.10 (.09)
constant	.45	.19
adjusted - R^2	.12	.02
<i>n</i>	140	717

Notes: The dependent variable is the change in log secular giving 2004 - 2000. Secular giving is giving to combined purposes, basic needs, health, and education. The independent variables are the 2004 - 2000 changes in the indicated variable.

In column 1 the sample contains families who switched itemization status between 2000 and 2004. In column 2 the sample contains families who itemized in both 2000 and 2004. Both samples contain only verified itemizers and exogenous itemizers. Both samples contain only families who gave in either 2000 or 2004, or in both years.

Standard errors are in parentheses.

*** – significant at .01

* – significant at .10

Table 4. Price Elasticities for Different Kinds of Giving.

Dependent variable	Switchers	Always-itemizers
Secular giving	-2.00*** (.70) [140]	-1.45 (.93) [717]
Combined purpose and basic needs giving	-1.80** (.84) [125]	-1.32 (1.10) [679]
Religious giving	-2.50** (1.08) [104]	-.77 (1.05) [575]
Aggregate giving	-2.02*** (.52) [161]	-.73 (.60) [765]

Notes: The dependent variables are the changes in log giving 2004 – 2000 for the different kinds of giving listed in the left-most column. The estimates shown are price elasticities estimated from the 2004 – 2000 change in the log first-dollar price—each estimate comes from a different regression. The other independent variables included in each regression, but not shown in Table 4, are the same as in Table 3.

In column 1 the sample contains families who switched itemization status between 2000 and 2004. In column 2 the sample contains families who itemized in both 2000 and 2004. Both samples contain only verified itemizers and exogenous itemizers. Both samples contain only families who gave in either 2000 or 2004, or in both years.

Standard errors are in parentheses. Sample sizes are in square brackets.

*** – significant at .01

** – significant at .05

Appendix A. The Argument that $E(p_{it+1} | I_t) = p_{it}$ for $t = 2000, 2002, \text{ and } 2004$.

Now we construct an argument that the 2001-2004 tax acts were passed in such a way that resulted in $E(p_{it+1} | I_t) = p_{it}$ for $t = 2000, 2002, \text{ and } 2004$. Then the $p_{it} - E(p_{it+1} | I_t)$ term drops out of (1) and the permanent elasticity β_1 can be identified with data on the (observable) p_{it} . The maintained assumption that substitution across two-or-more-year spans is negligible and the implication of the 2001-2004 tax acts that $E(p_{it+1} | I_t) = p_{it}$ are our strategy to identify the permanent price elasticity.

The argument starts with the price elasticity, assuming for the moment that prices change but after-tax income does not. The 2001 Act eventually would effect a permanent increase in the price of giving, but this increase was to be phased in over 2001-2006 giving a family the incentive to shift giving from the future to the present. We must consider how much giving was shifted from the future into 2000 (2000←future) and how much was shifted from the future into 2002 (2002←future). We argue that it is likely that very little giving was shifted 2000←future: the 2001 Act was signed into law (obviously) well after year 2000 giving was complete and back in 2000 there was large uncertainty about what future tax policy would be. Although in 2000 everyone would have thought that a Bush victory in the 2000 presidential election would increase the likelihood of a tax cut and a Gore victory would increase the likelihood of a tax increase, the election was very close and not decided until the Supreme Court's ruling on December 13, 2000. That left very little time for a family to shift to 2000 giving intended to have been done in 2001 even if the family on December 13 now expected a 2001 tax cut.

Even though there was still some time left to effect a shift, the time series evidence of charitable deductions shown in Figure 2 at best offers only weak evidence that shifting occurred:

there is a 2000-to-2001 drop in charitable deductions as a percentage of personal income (consistent with a 2000←2001 shift), but the drop is not nearly as dramatic as the 1986-to-1987 drop. In addition, stock prices (the Standard and Poor's 500 index divided by 1,000 is plotted with triangles alongside deductions) were falling at the same time, in contrast to the stock price increase over 1986-to-1987. Hence, the evidence of a 2000←2001 shift is much weaker than the evidence of a 1986←1987 shift, and could be a response to changes in the stock market rather than a temporary shift in response to tax policy. In short, we argue very little 2000←2001 shifting occurred. During 2000, families expected the price of giving to be the same in 2001 as it was in 2000: $E(p_{i2001} | I_{2000}) = p_{i2000}$.

Similarly, we argue that very little 2001←2002 shifting occurred. To make this part of the argument and to trace through the sequences of changes set into motion by the 2001 and subsequent tax acts, we discuss a family in 2001 with taxable income at \$100,000; but the argument generalizes to families with other levels of taxable income. In 2001 the \$100,000 family faced a 27.5 percent federal marginal tax rate, a price of giving equal to .725. Assuming that no change in income was expected (an assumption to be relaxed below), that family knew its price of giving would be .73 in 2001. We argue that this small price increase (less than one percent) would have caused little 2001←2002 shifting.⁷ Putting this argument together with the argument in the previous paragraph, we conclude that little 2000←2002 shifting occurred.

We next consider how much giving might have been shifted from the future into 2002. Following the previous paragraph's family with \$100,000 taxable income, in 2002 that family expected no price change in 2003, but a price rise to .74 in 2004, no change again in 2005, and a

⁷It turns out that $t = 2001$ is the only year from 2000 to 2004 that $E(p_{it+1} | I_t) \neq p_{it}$.

price rise to .75 in 2006. There is no tax advantage for a 2002←2003 shift in giving because $E(p_{i2003} | I_{2002}) = p_{i2002}$, but, in principle, there is advantage for a two-year shift: 2002←2004. However, recall our maintained assumption that intertemporal substitution across two-or-more-year spans is negligible.

We have to consider 2003←2004 shifting as well. As just described, under the 2001 Act the family we are following would have, in 2003, anticipated a tax advantage by a 2003←2004 shift in giving because in 2003 the price was .73 and in 2004 the price would be .74. This incentive to 2003←2004 shift giving, albeit small, was completely erased by the 2003 Act: the family's price was immediately raised to .75 for 2003 and 2004 causing $E(p_{i2004} | I_{2003}) = p_{i2003}$. The price level was scheduled to remain .75 until 2011. Shifting from 2004→2011, we assume, is negligible.

In summary, our argument is that the 2001, 2003, and 2004 tax acts were such that little-to-no shifting occurred either into, or out of, 2000, 2002, and 2004. This mitigated transitory responses to the price changes, allowing us to identify a permanent response.

Of course, this identification strategy depends on the validity of the assumption that substitution across two-or-more-year spans is negligible and the argument that $E(p_{it+1} | I_t) = p_{it}$. The argument that $E(p_{it+1} | I_t) = p_{it}$ we have made is for a family with constant income (say \$100,000) so that the only changes in prices are driven by changes in the tax acts.

Recall that the 2001, 2003, and 2003 tax acts made other changes in tax policy that raised the price for some families (raising the standard deduction for all married-joint families to 200 percent of the single standard deduction, and the increase of the 15 percent bracket amount for married-joint families to 200 percent of the single 15 percent bracket amount) and increased the

after-tax income of some families (the child tax credit). In both cases, the arguments about shifting we made above about tax rates apply again: the changes were such that little-to-no shifting occurred, and this mitigated transitory responses to the changes.

The tax rate cuts raised after-tax income at the same time they raised prices. Holding pre-tax income constant, any cross-year shifting in giving to take advantage of transitory differences in after-tax income, $y_{it} - E(y_{it+1} | I_t)$, would have been zero in the years $p_{it} - E(p_{it+1} | I_t)$ was zero, and would have been in the opposite direction of the price-induced shifting in the years $p_{it} - E(p_{it+1} | I_t)$ was not zero. The opposite direction shifting to take advantage of transitory differences in after-tax income would further mitigate any shifting to take advantage of anticipated differences in price.