

What is the Price Elasticity of Charitable Giving?

Toward a Reconciliation of Disparate Estimates

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Abstract

There are independent literatures in economics considering tax-price and match-price incentives for giving. The match-price literature has produced well-identified small price elasticities, but scholars have widely questioned whether these estimates can inform tax policy. The tax-price literature in contrast has produced a large range of estimates. Here, we explore and compare these different incentives. First, we consider tax incentives for giving by focusing on a state-level tax credit that creates a convex kink. We use traditional, as well as more novel, kink methods to estimate the tax-price elasticity of giving. Second, a subgroup of donors in our data were temporarily offered a match for their gifts, creating an opportunity to compare tax-price and match-price effects for the same group of donors giving to the same organization at the same time. We find the tax-price elasticity is about -2 . The match-price elasticity is essentially the same. Our results thus suggest a small tax-price elasticity, close to the match-price elasticity, and close to match-price elasticity estimates in the experimental match-price literature. The implication is that in the giving environment we investigate the match-price elasticity is informative for tax policy.

JEL codes: H31, D12, D64

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Introduction

Despite extensive work, there is a disparity of results about the magnitude of the price elasticity of charitable giving. Two literatures have developed independently. First, a tax-price literature has focused on variation in price induced by tax policy (e.g., Randolph, 1995; Barrett, McGuirk, & Steinberg, 1997; Auten, Sieg, & Clotelter, 2002; Bakija & Heim, 2011). Second, a match-price literature has manipulated the price of giving via matching grants in laboratory and field experiments (e.g., Eckel & Grossman, 2003, 2008; Davis, 2006; Karlan & List, 2007; Huck & Rasul, 2011; Huck, Rasul & Shephard, 2015).

The match-price literature has produced a relatively narrow range of estimates. The amount donated by individuals exclusive of the match (the so-called “checkbook” effect) has repeatedly been estimated as inelastic, with most checkbook elasticities ranging from approximately zero (Karlan, List, & Shafir, 2011) to -0.42 (calculated from Davis, 2006). In marked contrast, well-known papers in the tax-price literature have produced a wide range of elasticity estimates, from -0.5 (Randolph, 1995; Barrett, et al., 1997) to very elastic demand with elasticities more negative than -1 (e.g., Auten et al., 2002; Bakija & Heim, 2011; for a review see Peloza & Steel, 2005). Consequently, basic questions remain about both the magnitude of tax-price effects on giving and the extent to which they are different from match-price effects.

These literatures feature different benefits and drawbacks that make addressing these questions difficult. The strength of the match-price literature—estimates are identified by exogenously introduced experimental variation in price—is understandably a source of less certainty for the non-experimental tax-price literature. Papers in the tax-price literature have used very different identifying assumptions based on instrumental variables (Randolph, 1995), proxies for unobserved variables (Barrett, et al., 1997; Bakija & Heim, 2011), or income dynamics (Auten et al., 2002). Different approaches to identification could be seen as a strength if they all produced similar results, but this is not so. Moreover, in each of these papers identification rests on a maintained functional form assumption relating income to giving (cf. Bakija & Heim, 2011). Violations of that assumption would directly affect price elasticity estimates because papers

in this literature use variation in tax-prices driven by variation in marginal tax rates, and marginal tax rates are a function of income. Researchers often address this problem using major tax reforms (e.g., the Economic Recovery Tax Act of 1981 and the Tax Reform Act of 1986), but these complicated reforms changed many things besides marginal tax rates, including disposable income, in ways difficult to control for.

An obvious strength of the tax-price literature is that its object of estimation, the price elasticity evoked specifically by the tax code, is directly relevant for evaluating tax policy. This is less certain in the match-price literature for several reasons, an observation first discussed by Eckel and Grossman (2003) (see Vesterlund, forthcoming, for an overview). First, the match-price population being studied in a particular setting may not be the same as the relevant population in a tax-price study. Further, even the same individuals giving to the same charities may respond differently to a match compared to a rebate offered via the tax code because (for example) there may be differences in framing created by matches and rebates. Most match-price papers compare their estimates to those in the tax-price literature, but authors have acknowledged that it is unclear whether the results of the two literatures should be thought of as comparable. Karlan and List (2007, p. 1775) summarize the situation well: “it is not known whether ‘price’ changes via a matching grant influence behavior in the same manner that price changes via tax reforms alter behavior, and laboratory evidence suggests such framing matters.”

Reconciling the disparities between these literatures would require first producing a tax-price estimate with robust identification, while at the same time producing a match-price estimate to serve as a direct comparison: a match-price elasticity estimated from the same population giving to the same organization during the same time period. As pointed out by Meer (2014), however, this approach typically is not feasible because the same data rarely afford estimation of multiple elasticities in parallel.

In this paper, we conduct a tax-price study and a match-price study in parallel. First, we propose a novel estimation of the tax-price elasticity which dispenses with the traditional identifying assumptions in the literature. Second, a subgroup of donors in our study were offered a match for a certain period of time, allowing us to compare our tax-price estimate to a match-price estimate. This comparison is made

for the same sample of donors, giving to the same organization, during the same time period, in a non-laboratory, high-stakes setting.

We consider tax incentives for giving by focusing on a state-level tax-credit kink. The state of Indiana provides an income tax credit of fifty cents for every dollar donated to a within-state institution of higher learning. However, the maximum credit amount is capped, creating a convex kink in individuals' budget constraints. Because the kink comes from a *credit*, rather than a *deduction*, it is independent of the marginal tax rate and the consequent identification challenges that come with using marginal tax rates. In addition, we argue that our estimates impose identifying assumptions that are weaker than previous assumptions used in the tax-price literature: we are able to identify a tax-price elasticity without use of an instrumental or proxy variable, without an assumption about income dynamics, without a maintained functional relationship between giving and income, without relying on cross-state variation in states' marginal tax rates, and without a large tax reform.

We estimate tax-price elasticities using data that include donations made by over 150,000 people from 2004 to 2015 to a nationally-recognized university located in Indiana. Along with using standard kink methods, we also develop two new methods. The first uses a weaker identifying assumption than in Saez (2010) or in Kleven and Waseem (2013). The second exploits the existence of states in our data that do not face a kink. Our two new kink-based methods rely on identifying assumptions entirely different from each other but produce similar estimates.

We estimate the match-price elasticity using a \$3 million matching grant made to the university during our data period. The grant offered a one-to-one match for gifts up to \$250,000 to a subset of donors for a 19-month period, motivating a difference-in-differences approach. The \$3 million is 30 times larger than the largest matching grant previously investigated (\$100,000 in Karlan & List, 2007). Otherwise, the environment we investigate has several features in common with the natural field experiments that have previously estimated match-price elasticities: the matching grant occurred in the field and donors did not know that we would use the data they were generating to estimate their responsiveness to the match.

We find clear visual evidence of bunching at the kink created by the tax credit. The implied tax-price elasticity is between -0.2 and -0.5 , with most estimates closer to the low-magnitude end of that range. Although these elasticities are large relative to other elasticities in the kink literature, they are small relative to elasticities in the tax-price literature. These small estimates are the same using (a) both of our two new methods despite their identifying assumptions being different, (b) using the previous methods of Saez (2010) and Kleven and Waseem (2013), and (c) regardless of how the technical details of the estimation are varied. Turning to the match-price elasticity, there is also clear visual evidence of the response to the match. The estimated checkbook elasticity is about -0.2 . This result also is robust to different specifications.

The match-price elasticity we find is thus comparable to elasticities found in the experimental match-price literature, and, importantly, similar to our own tax-price elasticity estimate. This represents novel evidence that estimates of the tax-price and match-price elasticities are, at least in some settings, similar. This suggests that prior experimental studies using matches may have produced results that correspond well to policy-relevant tax-price responses. Our results also suggest renewed effort be devoted to understanding the impact of identifying and functional-form assumptions in the tax-price literature. When we estimate the tax-price elasticity without these assumptions, the estimates are dramatically smaller than most in that literature.

Our match results provide a validation of a tax-kink estimate using price variation independent of the tax code. To our knowledge this has not been done previously, and builds confidence in kink-based methods. The new kink-based methods we develop also may be of interest to those using kink methods in other applications.

The next section discusses the kink-based methods, as well as the difference-in-differences. Section 3 describes the tax credit and the data. Section 4 presents the empirical results. Section 5 discusses the interpretations of tax- and match-price elasticities, and Section 6 concludes.

2. Estimation methods

2A. Compensated tax-price elasticity approach

The credit we consider reduces a donor's income tax at the rate of 50 cents for each dollar donated. The credit is available for contributions up to \$400 for married-joint filers (that is, \$400 donated earns a \$200 credit). This creates a large discrete change in the opportunity cost of giving at this threshold, suggesting a kink-based estimation method. Although kink-based estimation is well-reviewed elsewhere (Kleven, 2015) we discuss the basic intuition and estimation methods from which our extensions can be understood.

Our first approach follows the kink estimator described by Saez (2010), modified to fit our context. Consider an individual who receives warm glow utility $U(x, g; \theta)$ from giving g ; x is own consumption and θ is a smoothly-distributed parameter describing heterogeneous preferences for g . The government imposes a lump-sum tax τ but reduces the individual's tax burden with a credit t for each dollar donated. However, the tax credit is only provided for donations below a threshold g^* . With pre-tax income Y , the budget constraint is $g = Y - (\tau - t g) - x$ if donations are below g^* and $g = Y - (\tau - t g^*) - x$ if donations are above g^* . Hence, capping the tax credit creates a convex kink in the budget constraint at g^* .

Now imagine a counterfactual where the tax credit t is not capped, but extends for donations above g^* . This counterfactual budget line is shown in Figure 1: it is a solid line below g^* and a dashed line above g^* . If the government were to intervene in this counterfactual by eliminating the credit for donations over g^* , the solid kinked line would be the budget constraint. The individual furthest above g^* who subsequently bunches at g^* after the kink is introduced is depicted at the equilibrium bundle B. This individual will have an indifference curve tangent to the upper part of the kink, at point A, after the kink is introduced. Other individuals along a range of the counterfactual budget line—those choosing points between g_b and g_a in the counterfactual world in which the credit is extended—would also bunch at the kink.

For the individual with equilibrium A and with counterfactual equilibrium B, the creation of the kink by capping the tax credit is approximately a compensated price increase. The compensated demand elasticity would involve increasing the price of g from $(1 - t)$ to 1 while increasing income to put the individual back at the original utility level at point C. The compensated decrease in giving is $g_b - g_c$ in Figure 1. For small income effects, this difference will be very close to the difference $g_b - g_a$ that can be estimated from the data. The amount of bunching at the kink can thus be used to uncover the counterfactual interior solution at equilibrium B, and with it an estimate of the compensated elasticity.

Following Saez (2010), assume that utility is quasilinear: $U(x, g; \theta) = x + \frac{\theta}{1 + 1/e} \left(\frac{g}{\theta}\right)^{1 + 1/e}$ and maximize it with respect to $x + pg = Y - \tau$. It can be shown that (see Appendix A; this and subsequent appendices are available on-line or upon request):

$$\beta \cong \frac{h_{g^*} + h_{g^+} \left(\frac{p_1^e}{p_0^e}\right)}{2} g^* \left(\frac{p_0^e}{p_1^e} - 1\right) \quad (1)$$

where β is the fraction who bunch at the kink, $p_0 = 1 - t$ is the initial low price of giving that rises to p_1 above g^* , h_{g^*} is the limit of the density of giving as g approaches g^* from below, and h_{g^+} is the limit of the density as g approaches g^* from above. The limits h_{g^*} and h_{g^+} are from the observed density of giving and the policy parameters g^* , p_0 , and p_1 are known. Then, if one has an estimate of bunching at the kink β , equation (1) can be solved for the compensated elasticity e . The width of the bunching interval $g_b - g_a$ is estimated by $g^* \left(\frac{p_0^e}{p_1^e} - 1\right)$.

We will estimate β using three different methods: *nearest neighbor* (following Saez, 2010), *polynomial* (following Kleven & Waseem, 2013), and a new method we call *nearest round neighbor*. The methods differ according to the identifying assumption made about what the fraction of donors at the kink would have been in the counterfactual case where the credit is not capped at \$400. The nearest neighbor method, developed by Saez (2010), assumes that the counterfactual fraction of donors at \$400 would have looked like the average of the two fractions of donors just below and just above the kink.

Consider centering a bin of bandwidth w at \$400, and also form one bin below this and one bin above, both of width w . Using only the fractions of donors in those three bins, consider the regression:

$$f_b = \alpha + \beta d_{b=400} + \varepsilon \quad (2)$$

where f_b is the fraction of donors at bin b , $d_{b=400}$ is a dummy indicating the bin at \$400 and ε is noise.

The coefficient $\hat{\alpha}$ estimates the counterfactual fraction at the kink and the coefficient $\hat{\beta}$ estimates bunching at the kink.¹

Individuals making donations may favor round numbers. When the kink is located at a round number the nearest neighbor method cannot avoid capturing in its estimate of bunching at the kink the tendency for people to make donations at round numbers of \$100s—a tendency that has nothing to do with tax policy. This would bias the estimated elasticity away from zero. For example, if a bandwidth $w = \$25$ is chosen, the bin centered at the kink is (\$387.50, \$412.50) and the left and right bins are (\$362.50, \$387.50] and [\$412.50, 437.50); neither the left nor right bin contains a round number donation in \$100s (the next two closest round numbers are at \$300 and \$500), so the tendency for people to donate at \$100 increments is not accounted for in the counterfactual.

The *polynomial* method of Kleven and Waseem (2013) addresses this problem by using a dummy variable to indicate a donation amount at any multiple of \$100, i.e. at \$100, \$200, \$300, \$400, \$500, etc. Accounting for average donations at multiples of \$100 necessarily involves moving away from “near” neighboring bins. Therefore, in addition to the dummy indicator for \$100s, Kleven and Waseem identify the counterfactual fraction of donors at the kink by assuming that the “regular” pattern of the distribution can be captured by a third-order polynomial:²

$$f_b = \alpha + \beta d_{b=400} + \varphi d_{b \text{ at } 100s} + \sum_{j=1}^3 \omega_j \frac{b^j}{10^{j-1}} + \varepsilon \quad (3)$$

¹ The use of three bins of equal width w is slightly different than Saez’ original method; Saez used a bin centered on the kink with a width of $2w$ rather than w (he represents the size of bins using δ instead of w for notation). We use equal-width bins so that “bandwidth” is defined the same across the three methods presented in this section. Using a centered bin twice as wide does not qualitatively affect any of the results and produces similar but somewhat smaller elasticity estimates (see Appendix B).

² Chetty, Friedman, Olsen and Pistaferri (2011) use a similar method.

where $d_{b \text{ at } 100s}$ is a dummy indicating that bin b is a multiple of \$100. Although not shown in (3) we also include round number dummies for donations ending in \$25 and \$50. In (3), as in (2), the counterfactual fraction at the kink is estimated by the prediction of the regression with $d_{b=400}$ set to zero, and $\hat{\beta}$ estimates bunching at the kink. Unlike (2), estimation of (3) includes bins farther from the kink, for example from \$200 through \$1,000. And of course, consistent estimation is based on the regression functional form in (3) being correct in the sense that it adequately captures the shape of the counterfactual fractions across the bins.

We developed the *nearest round neighbor* method to combine the focus on bins that are relatively near the kink, as in Saez (2010), with the recognition that some portion of the fraction at \$400 is there because people tend to make donations at round numbers, as in Kleven and Waseem (2013). The idea is to estimate the counterfactual fraction at \$400 using the fractions at \$300 and \$500, denoted f_{300} and f_{500} . An advantage of the nearest round neighbor method is that a weak identifying assumption—that the counterfactual fractions are monotonic near the kink—is sufficient to identify lower and upper bounds on the elasticity. For the lower bound: in the extreme case where the counterfactual was a flat line from \$300 to \$400, the counterfactual fraction at \$400 would be simply the fraction at \$300 ($f_{400}^{cf} = f_{300}$). With a decreasing counterfactual, using the fraction at \$300 for the counterfactual would thus provide a lower bound estimate of bunching at the kink, and hence a lower bound estimate of the elasticity. Likewise, taking the counterfactual fraction at \$400 to be the fraction at \$500 would lead to an upper bound estimate of the elasticity.³

The observed density below the kink matches what the counterfactual density would be, but the observed density above the kink, in this case f_{500} , does not. Although in many applications this discrepancy can be ignored (see Kleven, 2015), it is straightforward to adjust the nearest round neighbor

³ Although monotonically decreasing fractions continuously from \$300 to \$500 is sufficient to identify these lower and upper bounds, all that is necessary is that in the counterfactual, $f_{300} \geq f_{400} \geq f_{500}$, i.e., only that the fraction decreases point-to-point from \$300 to \$400 and that the fraction decreases point-to-point \$400 to \$500. If the counterfactual was increasing, so that the fraction at \$300 was smaller than the fraction at \$500, the bounds would be upper and lower, respectively, and the identifying assumption would be $f_{300} \leq f_{400} \leq f_{500}$ in the counterfactual.

method to take it into account: the counterfactual fraction at \$500 is $f_{500} (p_1^e/p_0^e)$. The p_1^e/p_0^e adjustment to the observed fraction at \$500 is the same adjustment used in equation (1) to convert the observed density above the kink, h_{g^*} , to the counterfactual density (see Appendix A). The upper bound estimate uses this adjusted fraction to form the counterfactual: $f_{400}^{cf} = f_{500} (p_1^e/p_0^e)$. We also use this adjustment in a linearly interpolated estimate of the counterfactual fraction locating at the kink: $f_{400}^{cf} = \frac{1}{2}[f_{300} + f_{500} (p_1^e/p_0^e)]$. For each counterfactual f_{400}^{cf} , the fraction estimated to bunch at the kink because of the tax credit cap is:

$$\hat{\beta} = f_{400} - f_{400}^{cf}. \quad (4)$$

These three kink-based methods bring an important advantage to the estimation of the tax-price elasticity relative to the methods used in the previous tax-price literature: identification is based on much weaker assumptions. Specifically, identification of the compensated elasticity does not require an instrumental variable, a proxy variable, or an assumption about income dynamics. Furthermore, the identification assumptions avoid taking strong stands on the exogeneity of tax reforms, or the exogeneity of marginal tax rates, or on correctly specifying the functional-form relation between income and giving, as long as marginal tax rates and the income/giving relationship do not coincidentally create discontinuities in the distribution of giving precisely at the kink.

However, there are potential limitations to the kink-based approach. First, as is clear from Figure 1, this approach overestimates e to the extent that it also picks up the income effect from the price change. This does not appear to be a practical problem for our purposes because (a) our $\hat{e}$ s are small, suggesting if anything that the actual compensated elasticity is even smaller, (b) there is evidence indicating that even when kinks are large, income effects are unlikely to substantially bias this estimation approach (Bastani & Selin, 2014), and (c) in Section 2B we discuss an alternative approach that estimates an uncompensated elasticity that turns out to yield similar results, indicating that income effects created by the kink are likely modest.

Second, intertemporal concerns have been raised in the kink literature (e.g., le Maire and Schjerning, 2013), the match literature (Meier, 2007; Meer, 2016), and especially in the tax-price literature where since Randolph (1995) it has been argued that substitution in giving between time periods will lead to estimates that overstate the long-term sensitivity of giving to price. Because our estimates are small relative to the tax-price literature, this would imply that if intertemporal substitution is a problem the true estimates are even smaller than what we find. But, because we have panel data we can check for this by comparing the elasticity estimated among infrequent donors to the elasticity estimated among frequent donors; if bunching is driven by intertemporal shifting we would expect a higher elasticity among the frequent donors.

Third, we estimate an assumed homogeneous e , as is standard in the tax-price literature; Kleven and Waseem (2013) point out that in the presence of heterogeneity kink-based methods would produce a population-weighted average of elasticities. Fourth, the exposition above assumes that frictions do not prevent individuals from bunching exactly at the kink. Although frictions certainly may affect kinking behavior for some outcomes such as earned income (see, e.g., Chetty, et al., 2011), they are less relevant in our case because the tax credit is focused on a behavior that individuals can adjust with ease and precision. Indeed, in Section 3 we will present clear visual evidence of bunching precisely at our kink.⁴ Fifth, because the phenomenon of bunching at round numbers is a tendency expressed in terms of bunching at nominal (round) dollar amounts, we do not adjust our giving amounts for inflation. For the nearest round neighbor method we could inflation-adjust the giving amounts, the kink locations, and the two round number mass points used for identification, that is we could adjust our definition of the two

⁴ Other kinds of “frictions,” such as a lack of information about the credit, may affect the behavior of some potential bunchers. In this sense our estimated elasticity is different from a frictionless-full-information “structural” elasticity. However, of course, to the extent our estimate differs from frictionless-full-information, it does so in a policy-relevant way that could apply to other real-world tax-induced variation in price. Possible differences in information about credits and matches in a real-world context would add to the reasons provided in the Introduction for why tax- and match-price elasticities might differ, further motivating our match-price analysis below.

relevant “nearest neighbors” by inflation each year, but this adjustment would mechanically return exactly the same estimates of bunching.⁵

Finally, the discussion above ignores the possibility of corner solutions—individuals choosing zero giving in some scenario. If preferences are convex, then extending the credit as in Figure 1 will not cause extensive margin effects (Kleven, 2015). Specifically, any corner solution in the presence of the cap on the credit will not move to an interior solution above g^* if the credit is extended, and anyone at an interior solution in the presence of the cap will stay in the interior if credit is extended. The kink we investigate contrasts with notches, where extensive margin effects can matter (Kleven & Waseem, 2013).

2B. Uncompensated tax-price elasticity approach: A second counterfactual

The credit creating the kink we investigate comes from Indiana, but we have data on donors from other states who do not face this kink. Estimation based on these “control” states produces an uncompensated elasticity estimate, denoted e_u .

In contrast to the counterfactual in Figure 1 where the cap is removed and the credit extended for every dollar donated, Figure 2 considers a different counterfactual where the credit is unavailable for all donations, as in the non-Indiana states. Under normality, anyone who gives at least g^* in the absence of the credit+kink will stay at an interior solution with giving greater than g^* once the credit+kink is introduced. This means that bunching at the kink will come entirely from individuals who would, were the credit to be eliminated, move from the kink to some lower level of giving below g^* . Consider individual R at the kink; this is the donor whose donations fall by the most after the kink is eliminated. This individual’s non-kink equilibrium bundle is represented by the equilibrium S in Figure 2. This individual’s indifference curve at R is tangent to the lower edge (with slope $= -(1 - t)^{-1}$) of the kink after the credit+kink is introduced. Because individual R at the kink would relocate to a solution below the kink under the counterfactual, the difference $g_r - g_s$ is an uncompensated price effect.

⁵ Alternately, one could adjust for inflation while ignoring round number bunching; redoing the nearest neighbor estimates in this way produces results qualitatively similar to those presented below.

Two questions arise: How can a single kink capture both bunching from above (Figure 1) and bunching from below (Figure 2), and does bunching from below bias the estimate of the compensated elasticity discussed in Section 2A? The important point to realize is that Figures 1 and 2 represent two entirely different counterfactuals. It is possible that the same individual would give at the kink and would give more if the credit were expended as in Figure 1 (hence she “bunches from above” in Figure 1), but would give less if the credit were eliminated as in Figure 2 (hence she “bunches from below” in Figure 2). In fact, under quasilinear utility it is straightforward to show that the set of $\theta \in [\theta_{min}, \theta_{max}]$ individuals who bunch at the kink is identical in the two counterfactuals. θ_{max} is the individual who would increase her giving the most if the credit were extended, and θ_{min} is the individual who would reduce his giving the most if the credit were eliminated.

The uncompensated elasticity approach has several benefits. First, if one assumes the distribution of giving in control states can serve as a counterfactual for giving in Indiana, it is possible to estimate the location of S without any specified utility function at all. Second, because the target of estimation is an uncompensated elasticity that by design includes any income effect, both large-price-change kinks and small-price-change kinks should uncover e_u well. Third, the approach can easily accommodate round number bunching.

Using nonlinear budget constraints to estimate uncompensated price elasticities of giving is not new (see Reece and Zieschang, 1985). What is novel here is to show that this type of analysis extends to recent kink methods and can do so in a simple way without making strong assumptions about the utility function.

Our method for estimating e_u involves finding the marginal buncher who reduces his giving the most in the counterfactual where the credit is eliminated, and then using his level of giving to estimate e_u . Consider the population of donors in Indiana who make donations in a certain range around the kink, $\Theta = [\underline{g}, \bar{g}]$, where $\underline{g} < g^* < \bar{g}$. Let f be the fraction of donors in the Θ range around the kink that are below g^* , so that the percentile value of the *marginal donor* in Indiana just below the kink is $\rho = 100 * f$.

This donor is the person whose θ (and giving level) is just below individual θ_{min} , that is, the individual at the kink in Figure 2 who would reduce his giving the most if the credit were eliminated. Then we take the set of donors residing in the control states who give amounts in the Θ range, find the ρ -percentile donor, and use that donor's giving amount $g(\rho)$ as an estimate of what the marginal donor in Indiana would give in the counterfactual where the credit is eliminated.⁶ The arc elasticity of giving is then:

$$\hat{e}_u = - \frac{(g^* - g(\rho)) / ((g^* + g(\rho)) / 2)}{(p_1 - p_0) / ((p_1 + p_0) / 2)}. \quad (5)$$

The greater the bunching at the kink in Indiana, the lower the percentile value ρ of the Indiana resident just below the kink, consequently the lower will be the amount $g(\rho)$ from the control states, and the larger will be $\hat{e}_u$.⁷

Our baseline estimate pools donors in the Θ range from all control states to find $g(\rho)$. The identifying assumption is that donors in other states can be used to study giving behavior in Indiana in the absence of a state tax credit. Our data come from a school in Indiana, so that donors in Indiana not only face a credit, but include alumni who stay in-state after graduation; it is possible that alumni residing in the other states differ in unobserved ways. Accordingly, we do several checks intended to diagnose problems with the identifying assumption.

First, we solicited qualitative information from university administrators who work closely with alumni and donors. The administrators report that donors in Indiana are, in terms of age, income, and “school spirit,” similar to donors in other states. This qualitative indicator of similarity, like any check of an identifying assumption, is a necessary though not sufficient condition.

⁶ As a concrete example, consider gifts from \$201 to \$1000, so that $\Theta = [201, 1000]$. For gifts in Indiana, a gift of \$399 would be the $\rho = 49.4^{\text{th}}$ percentile of gifts in this range. Outside of Indiana, the $\rho = 49.4$ -percentile level of giving in this range is $g(\rho) = \$335$.

⁷ We investigated the presence of credits in other states for giving to the university located in Indiana, and cannot identify any other credit in these states that would bias our results. If there were a set of control states that offered an uncapped credit for donations, we could use them and the percentile-based approach to estimate the marginal individual bunching from *above* the kink and thereby estimate the compensated elasticity; this would be an alternative to the kink methods described in Section 2A. However, without such a set of control states, our use of the percentile method is limited to estimating the uncompensated elasticity below the kink.

Second, to the extent that there is heterogeneity across states, we can exploit variation in the $g_j(\rho)$ amounts from the $j = 1, \dots, N_{\text{states}}$ separate states to construct “heterogeneity lower and upper bounds.” To do this we find the ρ -percentile donor in each state separately, and form a set of those ρ -percentile donors $\{g_j(\rho)\}_{j=1, j \neq \text{IN}}^{N_{\text{states}}}$. The smallest $g_j(\rho)$ amount from this set, when used in (5), will produce the largest $\hat{e}_u$ from among the control states. At the other extreme, the largest $g_j(\rho)$ amount from this set will produce the smallest $\hat{e}_u$. The smallest and largest $\hat{e}_u$ s estimated in this way are the lower and upper bounds constructed from the full range of heterogeneity across the states. Using the smallest-to-largest $\hat{e}_u$ interval to bracket e_u involves a weaker identifying assumption: that at least one state in that interval can serve as a control state for Indiana.

Third, if unobserved heterogeneity causes differences in the giving of donors in Indiana compared to the giving of donors in other states—for example, if in-state alumni were especially fervent supporters of the university and especially generous donors—then it would be likely that we would find a nonzero spurious elasticity at some other location above the kink, say at \$500—or even at \$401. We check for this possibility by redoing the estimation using a series of “placebo kinks” above the true kink.

However, unobserved heterogeneity would not be the only possible interpretation of a sizable “elasticity” at a placebo kink above the true kink; an alternative interpretation would be that the tax credit produces a large income effect. To understand why return to the first counterfactual depicted in Figure 1 and note that a portion of the budget constraint (the part below the kink) is exactly the same both before and after the kink is introduced. But that is not true in the second counterfactual: in Figure 2 the donors in Indiana are always on a different budget line than those in the control states, no matter how much or little they give (as long as they are not at the corner). In Figure 2, for a person in Indiana giving slightly above the kink, say \$450, the tax credit works as a pure income effect. For this Indiana donor, the price of donating one extra dollar is the same as it would be in any other state—but the Indiana donor has \$200 more income than she would in the control states because she qualifies for the \$200 Indiana credit. Now assume for the moment that there are no income effects at all; then the Indiana donor would be

unresponsive to the \$200 income shock created by the credit, and would give the same \$450 even if the credit were eliminated. This argument holds not just for \$450 but for any value of giving above \$400: in the case of no income effects, the distribution of donors giving more than \$400 in Indiana would match the distribution of donors giving more than \$400 in the control states.⁸ Relaxing the assumption of no income effects: if income effects are positive but small, the distributions of giving in Indiana and the control states should be similar and a placebo kink above the true kink should return a near-zero estimate.

We interpret placebo kink checks as primarily informative about heterogeneity because our prior expectation is that income effects in our giving environment are likely small.⁹ Alternatively, if one expects large income effects, a sizable “elasticity” at a placebo kink would be indicative of either unobserved heterogeneity or a sizable income effect or both. In any event, “elasticities” near zero at placebo kinks above the true kink, combined with an elasticity estimate at the true kink similar in magnitude to the compensated elasticity estimate described in Section 2A, would suggest that the elasticity estimate at the true kink is driven by bunching at the tax kink and not heterogeneity creating differences in the distributions of giving in Indiana compared to the control states, nor large income effects.¹⁰

Finally, the identifying assumption for the second counterfactual is qualitatively different from the identifying assumptions used in the variety of kink methods described in Section 2A, which do not use control state information in any way. Thus, although the identification assumption in (5) should be kept in mind while thinking about the e_u estimates, we note that there are strong tests of the robustness of the control state estimator, and that the estimates of the compensated elasticity do not rely on it.

⁸ It is straightforward to verify this in the quasilinear model; see Appendix C.

⁹ Although the tax-price literature suggests that the income elasticity of giving is close to 1 (e.g., Randolph, 1995; Auten, et al., 2002), we expect the income effects in our giving environment to be small because the \$200 income shock is a very small percentage change in donors’ incomes.

¹⁰ Below the kink, interpretation of placebo estimates is more complicated, even if income effects are zero. It can be shown that for placebo kinks at a distance below the true kink (e.g., placebo kinks at \$200 or \$250) the “elasticity” estimate should be close to zero, but as the placebo kink location approaches the true kink from below (e.g., at \$390) the placebo “elasticity” approaches the true elasticity (see Appendix C); this pattern is confirmed in our data. We focus on placebo locations above the kink because placebos above the kink provide a sharper robustness test of unobserved heterogeneity.

2C. Match-price elasticity specifications

In 2009, a donor from the class of 1960 made a \$3 million matching gift to the university to support the Class of 1960 Scholarship Endowment. Donations from members of the 1960 class made between December 1, 2008 and June 30, 2010 were matched one-to-one up to \$250,000 per donation.¹¹ We discuss our data more momentarily, but here we note that the data span the period of this match and allow us to identify the graduating class of donors as well as the date a donation is made, so we can compare the giving behavior of alumni from the 1960 class to the giving behavior of other alumni who were ineligible for the match, before, during, and after the 19-month period of the match.

We use difference-in-differences, although unlike standard diff-in-diff our “treatment” turns both on and off over time. The baseline specification will be:

$$y_{isctm} = \delta \text{match}_{ictm} + X_{ictm} \beta + \phi_c + \varphi_t + \lambda_m + \varepsilon_{isctm} \quad (6)$$

where y_{isctm} is one of three dependent variables (described below) for individual i , living in state s , of alumni class c , in year t and month m . The variable of interest “ match_{ictm} ” is a dummy that equals unity from December 1, 2008 to June 30, 2010, for $c = 1960$ class, and zero otherwise. The ϕ_c , φ_t , and λ_m represent class, year-of-donation, and month-of-donation dummy variables, capturing variation in giving across classes, trends in giving across years, and seasonal variation within a year.

The match period includes the class of 1960’s 50th anniversary, or more specifically, the first six months of the year in which the anniversary occurs. To control for natural increases in giving that occur at significant anniversaries, the X regressors include dummies for 25, 50, and 75 years following a class’s graduation. Because the match was available for 13 months prior to the start of the 1960 class’s 50th anniversary calendar year and lasted only for the first six months of 2010, we also include in X a “placebo match” variable that switches on 13 months before each class’s 50th anniversary begins, and changes back to zero in July of the class’s 50th anniversary year. The placebo match thus controls for any tendency for

¹¹ The university development office has confirmed that there were not any other similar large-scale matches made during our data period.

giving from the other classes to increase in the 19-month time period around their 50th anniversaries corresponding to the 19-month match period for the 1960 class.

The first dependent variable we investigate is the logarithm of the checkbook amount donated. The coefficient of interest δ represents the percentage change in the amount donated in response to the match. $\hat{\delta}$ is converted to a match-price elasticity $\hat{e}_m$:

$$\hat{e}_m = - \frac{\hat{\delta}}{(p_1 - p_0) / \left(\frac{(p_1 + p_0)}{2} \right)} \quad (7)$$

where p_0 is the match-price $1/(1 + m) = 1/2$ and p_1 is the non-match price (e.g., 1). Because each observation in this specification corresponds to a separate gift, the estimates are identified off of the intensive margin: the estimated elasticity is the percentage change in giving in response to a percentage change in price, conditional on making a donation.

To investigate the effect of the match on the extensive margin, the second dependent variable is the total number of gifts aggregated into class x state x month x year cells. To investigate a price elasticity that captures both extensive and intensive margins, the third dependent variable is the donation amount, again aggregated into class x state x month x year cells (logged). In this case δ is the percentage change in total donated amounts in response to the match.

Finally, we check robustness to the inclusion of state dummies, and to the inclusion of a set of interaction state-by-year dummies λ_{st} and month-by-year dummies φ_{tm} ; the latter subsume the φ_t and λ_m dummies in (6). The interactions flexibly control for year patterns by state and secular year patterns by month.

3. Background on the tax credit policy and the data

Indiana income taxpayers who make a donation to a college, university, university foundation, or seminary located in Indiana are eligible for the Indiana College Credit. The credit is available for private, as well as public, institutions. The credit is 50 percent of the donation, up to a maximum donation of \$400 for joint filers or \$200 for others. With the cooperation of a university in Indiana we have obtained data

on donations made to the university from 2004 through May 2015. The data contain a (scrambled) identifier for each donor, alumni status and graduating class, the date of the donation, the amount, state of residence, and whether the donation was being jointly given with a spouse.¹² We use residence in Indiana as an indicator that the donor pays Indiana income taxes. Because donations designated as joint could only have been made by a husband and wife, we take joint as an indicator of a donation made by those likely to file a joint tax return.

Several aspects of donating to the university could cause donors to bunch at other amounts for reasons that have nothing to do with tax policy. For instance, an Indiana resident can receive a customized license plate by making a \$25 donation. Non-alumni can receive a subscription to the university magazine by making a \$35 donation. For most alumni, and in most years, a donation of \$200 would allow them to enter a lottery for football tickets. The nearest neighbor and nearest round neighbor methods should not be affected by bunching at amounts not near the kink. But for our other methods we take several steps, some standard in the kink literature and some particular to our setting, in response to this. First, where applicable we vary the range of the giving amounts around the kink used to estimate the elasticities to see if this affects the results. Second, we examine the 2004-2006 data separately from the 2007-2015 data; during 2004-2006 the football lottery donation was \$100, not \$200, for most alumni. Third, for some donors the football lottery donation was not \$200: recent alumni, senior alumni (e.g., 50 or more years since graduation), and non-alumni (who must give a very large donation to enter the lottery); we check robustness by estimating the elasticity for this non-lottery group in isolation. None of these checks reveals any sizeable impact on the estimates.

Our empirical work focuses on joint donations: there are 373,994 joint donations across the time period, of which 41,129 were made by donors residing in Indiana. Although single filers in Indiana are eligible for the credit, for them the cap is at \$200 and for most years of the data \$200 also is the exact

¹² We have more than 50 “states” because there are donations made from several territories other than the 50 states. These include American Samoa, the Federated States of Micronesia, Guam, the Mariana Islands, the Armed Forces Americas, Armed Forces Europe, Armed Forces Pacific, Puerto Rico, Palau, and the Virgin Islands.

amount people needed to donate to enter the football lottery. A further problem analyzing singles is that, although if the university knows a donor is married that donation is designated as joint, if the university does not know the donor's marital status that person's donation is designated as "non-joint." That is, "non-joint" in the data set can mean either single or that marital status is missing or that the donation was made by a corporation or other legal entity. For these reasons, we focus on joint donors facing a kink at \$400. Results for "non-joint" donations during 2005–2006 (when the kink for singles and the football lottery amount were different) are similar to the estimates for joint donations and are reported in Appendix D.

There is another source of variation in price: gifts used for the credit can be deducted from federal taxable income. However, the tax credit lowers state income taxes paid and state income tax is itself deductible from federal taxable income; the two effects work against each other so that the impact on prices from considering federal income taxes would be small. Further, if we were able to adjust individuals' tax rates, then prices both with and without the tax credit would be at a lower level, implying the prices we use in our elasticity estimation may be somewhat too high, the percentage change in price we use too small, and our (already small) elasticity estimates biased too large.

Table 1 provides summary statistics. The leftmost column considers Indiana residents, the rightmost column includes all donors. Although the average annual gift among all donors is larger, it is typical with charitable giving data that averages are sensitive to outliers: focusing on the 99.96 percent of donations that are less than \$1 million drops the average annual gift among all donors by almost half. In the \$200–\$1,000 range of giving around the kink there are several thousand observations, an adequate number for kink-based approaches, and the average annual gifts among Indiana residents and all donors combined are nearly identical—\$391 and \$377 respectively—and near the kink location. We use this range of the data for our baseline kink-based approaches, although results are not sensitive to changing the range.

The last two rows show gifts for the 1960 class, the relevant group for the match. Because the match-price study uses within-year variation in the availability of the match, these averages are at the

level of each separate gift—that is, in the last two rows each person’s gifts are not aggregated to an annual level. The average for all donors is again sensitive to outliers, including the \$3 million matching grant itself.

Our kink-based approaches focus on Indiana residents who donate to an educational institution. Table 2 uses giving data from another source to describe how donors to education in Indiana compare to the broader national population of donors. The data come from the 2005, 2007, and 2009 waves of the *Philanthropy Panel Study*, the generosity module within the *Panel Study of Income Dynamics (PSID)*. Column 1 describes Indiana residents who donate to an educational institution. Column 2 describes residents of all states who give more than \$1,000 in total; these donors give about 80 percent of all donations measured in the *PSID*. Rows 1-3 indicate that Indiana residents who donate to education are somewhat younger and more likely to be married, and unsurprisingly give more to education—\$466 compared to \$237—although both averages are reasonably close to the relevant kink in our study. This, however, is the only giving difference across the two columns: the average amount given to all charitable organizations (including education), to congregations, and to both categories combined are nearly identical.¹³ Hence, the two columns indicate that Indiana residents who donate to education resemble the population of donors who give over \$1,000 both in terms of their total giving and in terms of the split between charitable organizations and congregations.

Our data consist of donations to one university, but donors could give to more than one higher education institution. If a donor, bunching at the kink, splits donations between several schools, then we might incorrectly categorize this individual as a non-buncher, leading to an underestimate of the price elasticity. On the other hand, donors observed here at the kink, if they gave to other schools as well, would lead us to overestimate bunching. The schedule on which this credit is claimed requires individuals to list the different schools in Indiana that they have donated to. While that information is not

¹³ To account for the fact that the samples overlap when testing equality of variable means, we first regress each variable on an indicator for an individual belonging to the first column in the table (the coefficient on the indicator necessarily matching the mean in the table), then regress the variable on an indicator for the second column, and test equality of the coefficients using seemingly unrelated regression.

publicly available, we have consulted with officials at the Indiana Department of Revenue, and they have told us that the vast majority of credits claimed—on the order of 90 percent—are for donations made to a single school, so that this multiple-school donation concern should not affect the results. Relatedly, because our kink method based on the second counterfactual exploits giving behavior in other states, its estimate should net out any common two-school-donation behavior among donors across states, and results from this method are very similar to the estimates from the methods based on the first counterfactual.

4. Results

4A. *Compensated tax-price elasticity estimates*

Figure 3 provides simple graphical evidence about the nature of bunching in Indiana. The figure presents a histogram of joint gifts between \$200 and \$700, in bins of \$10, for residents of Indiana (grey bars) and elsewhere (clear bars). Even in this somewhat narrow range of donation amounts we have over 7,000 donors in Indiana alone. While both groups see much higher densities of giving at \$400 than \$10 above or below that amount, the figure shows evidence of particularly large bunching for those in Indiana compared to other states. The pattern of declining densities at multiples of \$100 is broken at \$400 in Indiana, but not so in other states.¹⁴ Hence, a simple visual inspection suggests that the tax incentive at least to some extent “matters” in Indiana. We use this excess bunching in Indiana to estimate the compensated tax-price elasticity.

Table 3 presents results using the three methods described in Section 2A. Row 1 begins with the nearest round neighbor method from equation (4). The first column presents the lower bound estimate where the counterfactual is based on the fraction giving at \$300. The last column shows the upper bound estimate using \$500 as the counterfactual, and the middle column’s counterfactual is the average of the

¹⁴ The large mass points for both Indiana and the other states at \$200, omitted from Figure 3, are about three times the respective mass points at \$300. We did not show the \$200 mass points in Figure 3 to allow the scale of the figure to more clearly display the bunching at \$400 in Indiana.

counterfactual in the first and last columns. The lower bound estimate is $-.121$. The upper bound estimate is $-.293$. Both estimates have small standard errors. The $-.121$ to $-.293$ range is fairly narrow; that is, the lower and upper bounds are informative. The point estimate of the elasticity in the middle column is $-.197$ (s.e. = $.024$); the 95% confidence interval is $-.150$ to $-.243$.

Row 2 presents the nearest neighbor estimate developed by Saez when a bandwidth of \$25 is used: $-.465$ ($.036$). It is important to understand why this estimate is larger (more negative) than the nearest round neighbor estimates in row 1. First, note that with a bandwidth of \$25, the estimate in row 2 is comparable to a nearest round neighbor estimate that uses mass points at \$375 and \$425 instead of \$300 and \$500. Second, the fractions of donors at \$375 and \$425 are very small and not typical of donation amounts that are multiples of \$100, as can be seen upon examination of Figure 3. Therefore the row 2 estimate confounds bunching at the kink because of the tax policy with the greater tendency to donate at \$400 because it is a multiple of \$100. Even so, the estimate produced is smaller than most in the tax-price literature. When the nearest neighbor bandwidth is expanded to \$50 (row 3), so that the neighbor below the kink includes the mass point at the round number \$350 (as well as the mass point at \$375) and the neighbor above the kink includes the mass point at round number \$450 (as well as the mass point at \$425), the estimate falls to $-.290$ ($.019$). The polynomial method is presented in row 4. The elasticity estimate is $-.259$ ($.022$).¹⁵

Rows 5-8 return to the nearest round neighbor method and examine its sensitivity to various estimation choices. Row 5 doubles the bandwidth used to estimate the counterfactual density below and above the kink; the change in estimates is negligible compared to the baseline in row 1. Row 6 doubles to

¹⁵ The polynomial estimate was produced using donation amounts from \$200 to \$999, a range we selected out of concern that large mass points at amounts outside that range (at \$25, \$50, \$100 and \$1,000) may distort the polynomial from accurately capturing the counterfactual pattern of the distribution around \$400. Accordingly, we examined the sensitivity of this method to the choice of range: expanding the range to the left to include \$100, \$50 and \$25, and expanding the range to the right to include \$1,000 and \$1,500, as well as doing sensitivity analyses of other estimation choices: doubling the bandwidth, doubling the bin width, using different polynomials (linear through fifth-order), using no polynomial (i.e., using just the round number dummies), using only the bins at multiples of \$100, and “dummying out” the football lottery-influenced mass point at \$200 so that it does not contribute to forming the counterfactual fraction at the kink. These sensitivity tests produced a range of estimates that in no case changed the substantive findings of the table: the smallest magnitude was $-.017$ (s.e. = $.018$; using a linear polynomial) and the largest $-.369$ (s.e. = $.026$; using a fourth-order polynomial).

\$50 the width of the bins into which we put the data; the resulting estimates are smaller magnitude, the lower bound not being significantly different from zero. Row 7 uses the mass point at \$250 (in place of the mass point at \$300) in the estimation of counterfactual fraction who choose the kink because the kink is at a round number: the linearly interpolated estimate is smaller ($-.136$), the lower bound is essentially zero, and the upper bound is, of course, unaffected.¹⁶ Row 8 uses the mass point at \$600 (in place of the mass point at \$500). The resulting linearly interpolated estimate ($-.231$) is not much different than baseline, but the upper bound estimate ($-.477$) is larger. This upper bound estimate based on the fraction at \$600 is a more conservative upper bound because the upper bound based on the fraction at \$500 (row 1) may capture a tendency to give in multiples of \$500, over and above the tendency to give in multiples of \$100. In any event, the main conclusion from Table 3 remains: regardless of which estimator we use, the elasticity estimates reflect evidence of clear bunching but are much smaller than estimates from the previous tax-price literature.

Table 4 subjects our nearest round neighbor method to a series of placebo tests. Each row provides estimates of “elasticities” at the placebo kink listed in column 1. For example, row 1 shows the elasticity estimates from a placebo kink at \$300, using mass points at \$250 and \$500 as the nearest round neighbors. The lower bound and linearly interpolated estimates are nonsensically positive, and the upper bound is a very small $-.096$. Row 2 examines a placebo kink at \$500: the lower bound estimate is positive, the linearly interpolated estimate is $-.110$, and the upper bound is $-.240$. These larger negative placebo results are consistent with the point raised in the previous paragraph that the fraction at \$500 may capture a tendency to give in multiples of \$500 over and above the tendency to give in multiples of \$100. The six remaining linearly interpolated estimates in rows 3-8 include two that are negative but small ($-.078$ and $-.084$) and four that are positive. The remaining upper bound estimates include two that are positive and four that range from $-.059$ to $-.166$, magnitudes much smaller than Table 3’s $-.293$ baseline

¹⁶ It would seem at first that, instead of using the mass point at \$250, the mass point at \$200 is a natural choice for this sensitivity test, but the fraction at \$200 includes those who choose that amount because that is the lowest donation that makes them eligible for the football lottery. Using \$200 as the lower bound mass point produces wrong-signed estimates of the elasticity. We further consider the football lottery’s impact on the results below.

upper bound and $-.477$ more conservative upper bound. In short, these tests indicate that the estimates based on the mass point at the \$400 kink are picking up more than just a placebo.

Table 5 returns to substantive results using the nearest round neighbor method and mass points at \$300 and \$500. Row 1 repeats the baseline estimates from the first row of Table 3. Row 2 uses the subsample of donors who would not be eligible to enter the football lottery by making a donation of \$200. This checks to see whether gaining eligibility for the lottery by donating \$200 affects the range of giving around the kink. Removing the donors becoming eligible for the lottery when giving \$200 produces a somewhat larger estimate of the lower bound ($-.223$), but linearly interpolated and upper bound estimates ($-.231$ and $-.293$) not much different than the baseline. An alternative way to check for the effect of the lottery on the estimates is to split the sample into two subsamples: 2004–2006 during which time a donation of \$100 made most alumni eligible for the lottery and 2007–2015 when a donation of \$200 became necessary. Estimates from both subsamples in rows 3 and 4 are similar to baseline, indicating that the location of the lottery amount has little effect on the estimates.

Rows 5 and 6 look respectively at donations made by people who over the twelve year period made joint gifts less frequently (one to five times) and more frequently (from six to 12 times). The linearly interpolated estimates indicate a near-zero elasticity among those who give less frequently: $-.058$ (.143) compared to those who give more frequently $-.211$ (.023). But the difference is reversed in the upper bounds: $-.438$ (.276) versus $-.281$ (.028). The much larger standard errors in the “less frequent” subsample prevents us from drawing the conclusion that the differences between the two groups are statistically significant.¹⁷ Rows 7 and 8 use an alternative definition of “less frequent” and “frequent” by also including in the determination of frequency those donations labeled as non-joint (non-joint gifts are used along with joint gifts to divide the sample between frequent and infrequent givers, but only the joint

¹⁷ The standard errors are large in the subsample giving less frequently because, not surprisingly, people who give less frequently also tend to give smaller amounts. The median amount given per year in the less frequent group is \$50, whereas the median amount given per year in the more frequent group is \$225. There are about one-half as many gifts in the \$300-\$500 range used to estimate the elasticity in the less frequent group than in the more frequent group.

gifts are included when calculating the elasticity). With this alternative definition the point estimates are essentially the same regardless of how frequently individuals make contributions. We return to estimates for more- and less-frequent givers in the next section.

In summary, the estimates suggest a compensated tax-price elasticity between $-.121$ and $-.293$, with a more conservative upper bound estimate of about $-.477$. Estimates from nearest neighbor ($-.290$) and polynomial ($-.259$) methods are smaller magnitude than the conservative upper bound. The standard errors on these estimates are fairly small. We compare these compensated elasticity estimates to other elasticity estimates below, but note that the estimates just presented are much smaller than those from the previous tax-price elasticity of giving literature.

4B. Uncompensated tax-price elasticity estimates

Table 6 provides uncompensated elasticity estimates as discussed in Section 2B and equation (5). Panel A contains substantive results and Panel B the placebo tests. The table focuses on a $\Theta = [\$201, \$1,000]$ range around the kink, but the results are similar using alternate ranges (see Appendix Table D). The baseline elasticity estimate is $-.265$ (.042); the 95% confidence interval is $-.183$ to $-.347$. Columns 2 and 3 present the heterogeneity lower and upper bounds: from zero to $-.429$. Column 4 focuses on the subsample not eligible to enter the football lottery by making a donation of \$200: the $-.310$ estimate is only slightly larger than baseline. Columns 5 and 6 look respectively at less and more frequent donors: the elasticity among less frequent donors is $-.288$ and among more frequent donors $-.265$. Although the standard errors again preclude a judgment that the difference is statistically significant, there is no evidence in the point estimates that the main results are driven by the most frequent donors, as we would expect to see if there was extensive intertemporal shifting. The last two columns use the alternative definition of “less frequent” and “frequent” donors, and, as in Table 5, this increases the elasticity estimate of the less frequent donors, which again does not fit a story where the results are driven by intertemporal substitution among the most frequent givers. Results using different Θ ranges of giving, examining 2005–2006 when entry into the football lottery required a smaller \$100 donation, and examining non-joint donations produce results similar to those presented in Panel A (see Appendix Table

D). The overall similarity of these results with Section 4A's compensated elasticity estimates suggests that income effects do not create dramatic differences between the uncompensated and compensated elasticities.

Panel B of Table 6 tests the sensitivity of the control state identifying assumption by looking for "elasticities" at placebo kinks above the true kink. If the distribution of donations in Indiana differs from other states in a way that biases the estimates in Panel A, then we would also expect to see this bias leading to spurious estimates not only at the true \$400 kink, but at other amounts above this kink as well.

Going just one dollar above the real kink reveals a strikingly different estimate. The estimate in this case is a wrong-signed .069 and insignificantly different from zero. The estimates remain close to zero at placebo locations farther above the kink. The results in Panel B thus show that the uncompensated elasticity estimates are local to the true kink, and that when looking at other donation levels close to but not precisely at the kink, the distribution of donations is similar in Indiana compared to the control states. This does not support a heterogeneity story where donors in Indiana are simply more generous at all levels of giving, but does indicate that our estimates are driven by bunching precisely at the kink.

To summarize, the estimates indicate an uncompensated tax-price elasticity that is small and precisely estimated. The estimates are very close in magnitude to the compensated elasticity estimates from the previous section. Hence, kink-based approaches based on two different counterfactuals produce tax-price elasticities that are much smaller than those from the previous tax-price literature. We now compare these tax-price elasticities to the match that occurred during the time of our study.

4C. Match-price elasticity estimates

To facilitate comparison with the tax-price elasticities just presented, we continue with a focus on joint donations in nominal dollars. Because the treatment varies by graduation class we restrict the sample to alumni. We use donations less than or equal to \$250,000, the maximum gift that would be matched under the grant. We check the sensitivity of the results to these decisions below.

Figure 4 presents simple visual evidence of the response to the matching grant. For the 1960 class we aggregate the donations in each month, take the log, and smooth the data for the figure by averaging

the logged donations over six month periods from 2007 through 2012. Because December 2008 is the first month of the match, it is averaged in with the first half of 2009. We do the same thing for the nearby control classes 1954 to 1959 and 1961 to 1965, averaging the log of aggregate monthly donations for these classes over each six month period. Figure 4 plots the difference: that is, 1960-class giving minus giving from other nearby classes.

There is a clear spike in the amount donated by the 1960 class relative to the nearby classes during the period of the match, especially in the first half of 2010. After the match switches off, donations from the 1960 class once again resemble donations from the nearby classes. We now use this response to estimate a match-price elasticity.

In Table 7 the dependent variable is the log checkbook amount of each separate donation. The first row estimates are the matching-treatment dummies, δ , from equation (6). The second row converts the δ into an elasticity as described in Section 2C, equation (7). Each regression includes class, year, and month dummies. Moving left to right across the columns adds state dummies and different controls for trends. The standard errors are clustered by graduating class. The baseline $\hat{\delta}$ in column 1 suggests about a 15 percent increase in the size of any gift made when the match is available. The implied elasticity is $-.227$ (.074). Column 2 adds month-by-year dummies, column 3 adds state-of-residence dummies, and column 4 adds state-by-year dummies; in each specification the estimates are similar to baseline. Column 5 includes class-specific year trends. Unsurprisingly, given the identifying source of variation in our data, the estimates are quite similar with these controls.¹⁸

In Table 8 the dependent variable is the total number of donations made by each class in a state, month, and year. Aggregating the unit of observation from the separate donation level into class x month x year cells loses no identifying variation because the variation in the match is class x month x year. The

¹⁸ The estimated elasticity is similar upon adding fixed effects for the individuals making the donations ($-.202$; s.e. = $.068$). Estimation using the donation amounts in levels, rather than logs, produces a similar elasticity, but less precisely estimated: $-.200$ (s.e. = $.200$). Checking the sensitivity of the results by adjusting for inflation, including non-alumni, including donations above a quarter of a million dollars and including non-joint donations produced a range of $\hat{\delta}$ estimates. The smallest was $\hat{\delta} = .019$ but imprecisely estimated (s.e. = $.076$); the implied elasticity is $-.029$. The largest was $\hat{\delta} = .191$ (s.e. = $.034$); the implied elasticity is $-.287$.

baseline elasticity estimate, .059 (.125), is small, wrong-signed, and insignificantly different from zero. The results are similar across the specifications, suggesting that the match causes little extensive margin response. The results thus suggest that the match was ineffective at encouraging “cold donors” (alumni who after approximately 50 years were not giving to their alma mater) to start giving.

In Table 9 the dependent variable is the aggregated donation amount (logged) made by each class, again by state, month and year. Because the unit of observation is the total amount coming from each class in state x month x year cells, the estimates capture both extensive and intensive margin changes. The estimates are the proportional change in the total amount donated in response to the match. The baseline elasticity estimate is $-.170$ (.093). The results are similar across the specifications. In the last column we investigate how the total donations of the 1960 class change after the match switches off. The model is the month-by-year specification from column 2, plus a new dummy variable that equals one in the 12-month period following the match. The estimate is essentially zero, and precisely estimated. After the match switched off, the donation behavior of the 1960 class cannot be distinguished from that of other classes. As seen in Figure 4, there does not appear to be a long-term increase, or decline, in donations after the match ends.¹⁹

5. Discussion of tax- and match-price elasticities

Using kink-based methods to estimate the tax-price elasticity of giving to a nationally-recognized higher education institution we find that the elasticity is about $-.2$. All the kink-based methods we use—the two new methods and the two standard methods—produce reasonably similar estimates, and all of the estimates are smaller in magnitude than the $-.5$ to very elastic range in the extant tax-price literature. In parallel we estimate a directly comparable match-price elasticity. The checkbook match-price elasticity

¹⁹ When the post-match dummy is added to Table 7 the results are essentially unchanged: the baseline coefficient is .150 (s.e. = .049) and the post-match dummy is .034 (s.e. = .018). However, adding the post-match dummy to Table 8 suggests perhaps a small decline in the number of donations in the year after the match: the baseline δ is $-.118$ (s.e. = .084) and the post-match dummy is $-.190$ (s.e. = .060). Even so, Table 9 indicates there is not a decline in the total amount donated by the 1960 class in the year after the match.

estimate is also about $-.2$. Hence, there is no disparity between our tax-price and match-price elasticities, implying that in the giving environment we investigate the checkbook match-price elasticity is directly informative for tax policy.

In addition, our inelastic $-.2$ match-price elasticity is qualitatively similar to the inelastic checkbook match-price elasticities produced by almost all previous match-price papers. In their seminal paper, Eckel and Grossman (2003) estimated a checkbook elasticity of $-.067$; in a field experiment replication they estimated a nearly identical $-.045$ (Eckel & Grossman, 2008). Davis' (2006) lab experiment produced $-.114$ (for a match-price = $.50$ and an endowment = $\$12$). In the first match-price field experiment, Karlan and List (2007) estimated an elasticity of about $-.3$. In the U.K. where a match is in place as a part of the tax incentive for donations, Scharf and Smith's (2015) analysis of taxpayers' intended responses to an increase in the match rate from $.25$ to $.30$ indicated a checkbook elasticity of $-.39$. The similarity of previous checkbook elasticities to ours, combined with the similarity of our checkbook elasticity to our parallel tax-price elasticity, suggests that previous checkbook elasticities also may be informative to tax policy.

With a price match, one can also consider amount-received elasticities that combine the checkbook amount donated by the individual plus the match it generates. In line with our interpretation of the checkbook elasticity, in previous match-price papers that have included a non-governmental tax-like rebate as part of the experiment (Eckel & Grossman, 2003, 2008; Davis, 2006), or have included intended responses to hypothetical governmental tax-price changes (Scharf & Smith, 2015), it is the checkbook match-price elasticities, rather than the elasticities of the amount received by the institution ($e_{amt\ received} = e_{checkbook} - 1$), that are closer to the tax-price elasticity estimates of $-.340$, $-.112$, $-.220$, and $-.50$ (respectively from Eckel & Grossman, 2003, 2008; Davis, 2006; Scharf & Smith, 2015).²⁰ Here we

²⁰ This even extends to the few cases where match-price estimates have suggested elastic demand. Eckel and Grossman (2006) and Scharf and Smith (2015; for one specific donor type: donors who are in a high marginal tax bracket and who also do adjust their checkbook donations in response to the match) both estimate a checkbook elasticity that is elastic, but in each of these instances the corresponding tax-price elasticity also is elastic. Also, Huck and Rasul (2011) estimate a *positive* checkbook elasticity (e.g., for a match-price = $.50$, the elasticity is $+.211$), and argue that the difference compared to prior experiments' negative checkbook elasticities is because their design

demonstrate that this pattern suggested by these laboratory- and hypothetical-scenario-based studies extends to high-stakes match decisions and a government tax policy currently in effect.

Finally, although we have focused on checkbook match-price elasticities in the present investigation and found those elasticities to be inelastic and small, when we repeat the baseline specification from Table 9 but change the dependent variable to be the (log) amount received by the institution (checkbook donation plus the match it generates), the elasticity estimate is $\hat{e}_{amt\ received} = -1.13$. Hence, as in prior work our amount-received match-price elasticity is elastic. But our tax-price elasticity is inelastic, implying that the government could direct substantially more funds to higher education institutions at no additional cost to itself by replacing the tax credit with a match.

6. Conclusion

There are separate literatures in economics investigating tax-price and match-price incentives for giving. The match-price literature has produced well-identified small price elasticities, but scholars have questioned whether these estimates can inform tax policy. The tax-price literature has produced a large range of estimates.

In this paper we have taken a first step toward reconciling these literatures. We estimate the tax-price elasticity of giving to higher education institutions using identifying assumptions that differ from, and we argue are weaker than, identifying assumptions used by the previous tax-price literature. Using two new and two standard kink-based methods we find that the tax-price elasticity is about -2 . This estimate is in the small-magnitude inelastic range of previous checkbook match-price elasticities. When

nets out a leadership gift effect: simply knowing that there is a leadership gift, even if it is not a matching grant, encourages others to donate. The design in prior experiments produces elasticities that would implicitly include any leadership gift effect. When Huck and Rasul replicate this prior design so that their elasticity estimate also includes the leadership gift effect (they have evidence in their giving environment that there is such an effect), the estimates are comparable with the negative checkbook elasticities from prior experiments (see the discussion of Table 4 in their paper). Like prior match studies, the match-price elasticity we estimate implicitly includes any leadership effect, so our estimates are compatible with the prior studies. Additional understanding about how leadership effects and matches work is an important objective for future research.

we estimate a match-price checkbook elasticity in parallel to our tax-price elasticity, we find that it also is about $-.2$

To our knowledge this is the first evidence that a privately-offered matching grant generates a price response similar in magnitude to the price response generated by a corresponding tax policy. The implication is that, at least in some giving environments, match-price elasticities are informative for tax-price elasticities. We consider this a “first step” toward reconciling the tax- and match-price literatures because we have estimated giving to only one type of charitable purpose: higher education. The next steps toward reconciliation are to investigate different types of charitable giving, using different identification methods with parallel sources of price variation, including variation driven by overhead costs (Meer, 2014). That many previous match-price studies have produced checkbook match-price elasticity estimates that match up well with both the checkbook elasticity and the tax-price elasticity estimates we find suggests that future work may produce further evidence that match-price elasticities are informative for tax-price elasticities.

In addition to the conceptual importance within economics of reconciling the tax- and match-price literatures, reconciliation would have significant policy implications. Almost everything known about the price elasticity of charitable giving from the tax-price literature is about an aggregate giving construct—total charitable deductions—that combines giving to many disparate purposes: poverty relief, family and youth services, sports leagues, health research, education, religious congregations, the arts, etc. A long-standing policy question concerns how giving responses to tax policy are distributed across these different purposes, but because tax policy applies to the aggregate there is little hope that situations will arise allowing the use of variation in tax-price specific to one of these disaggregated purposes. However, match-price experiments naturally can be used to identify price elasticities for giving to each of these purposes. If further reconciliation indicates that the match-price elasticities from those experiments are informative for tax policy, this long-standing question of distribution could be answered.

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Figure 1. Compensated price effects from a kink.

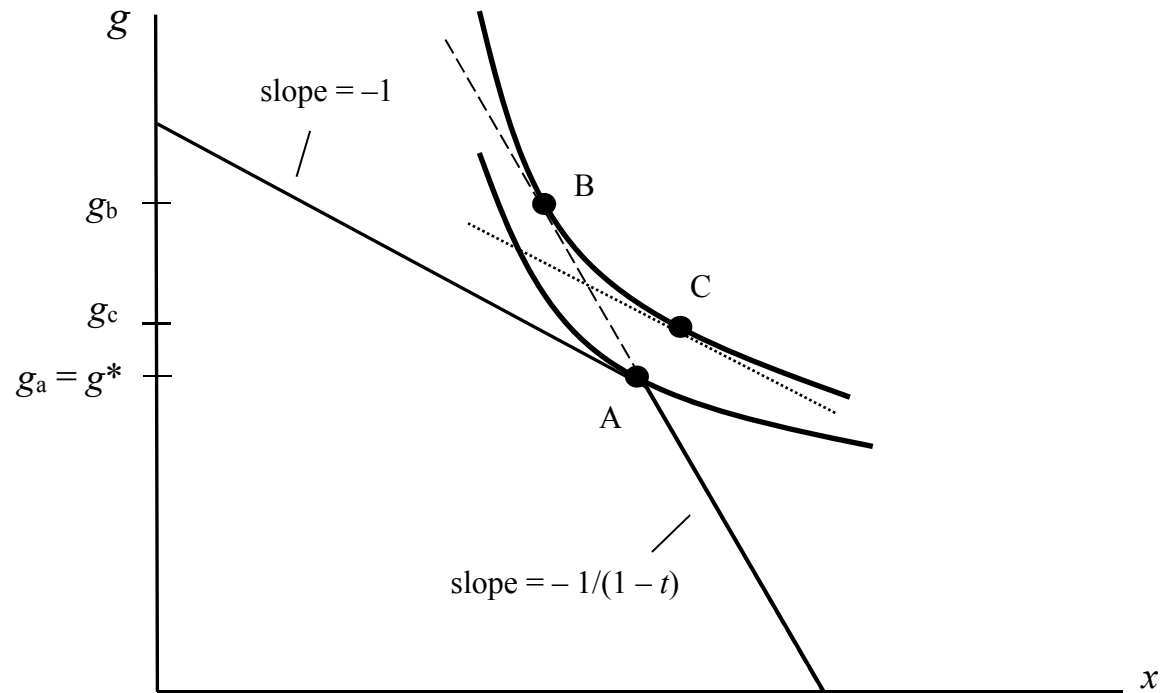


Figure 2. Uncompensated price effects from a kink.

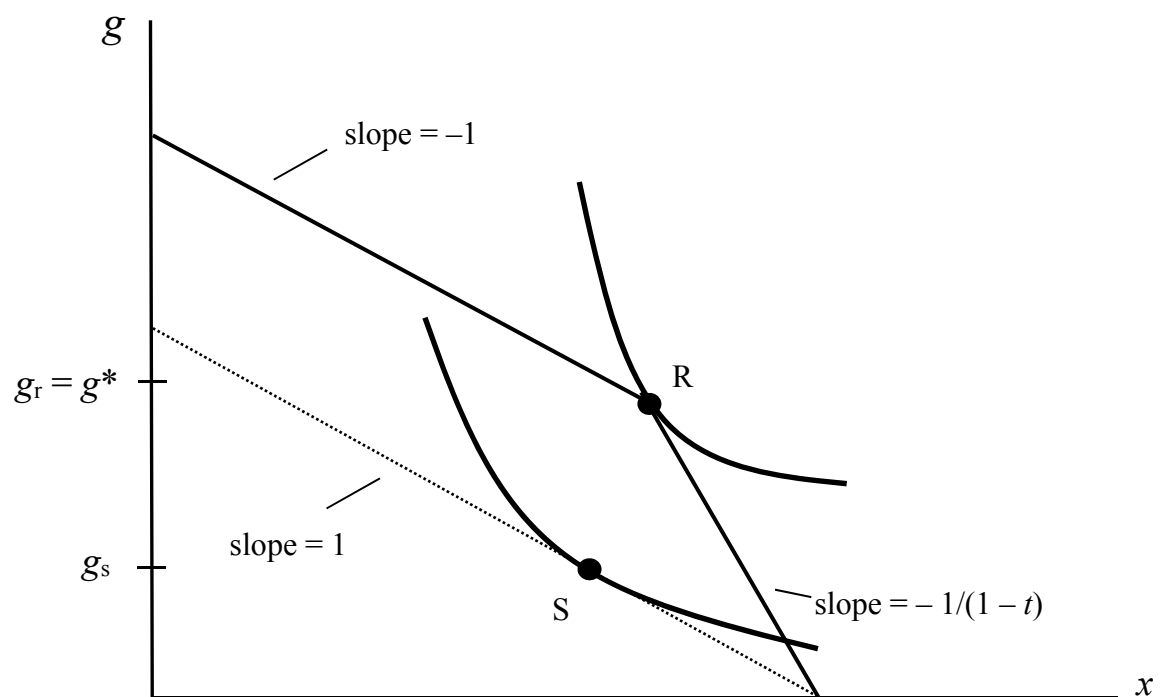
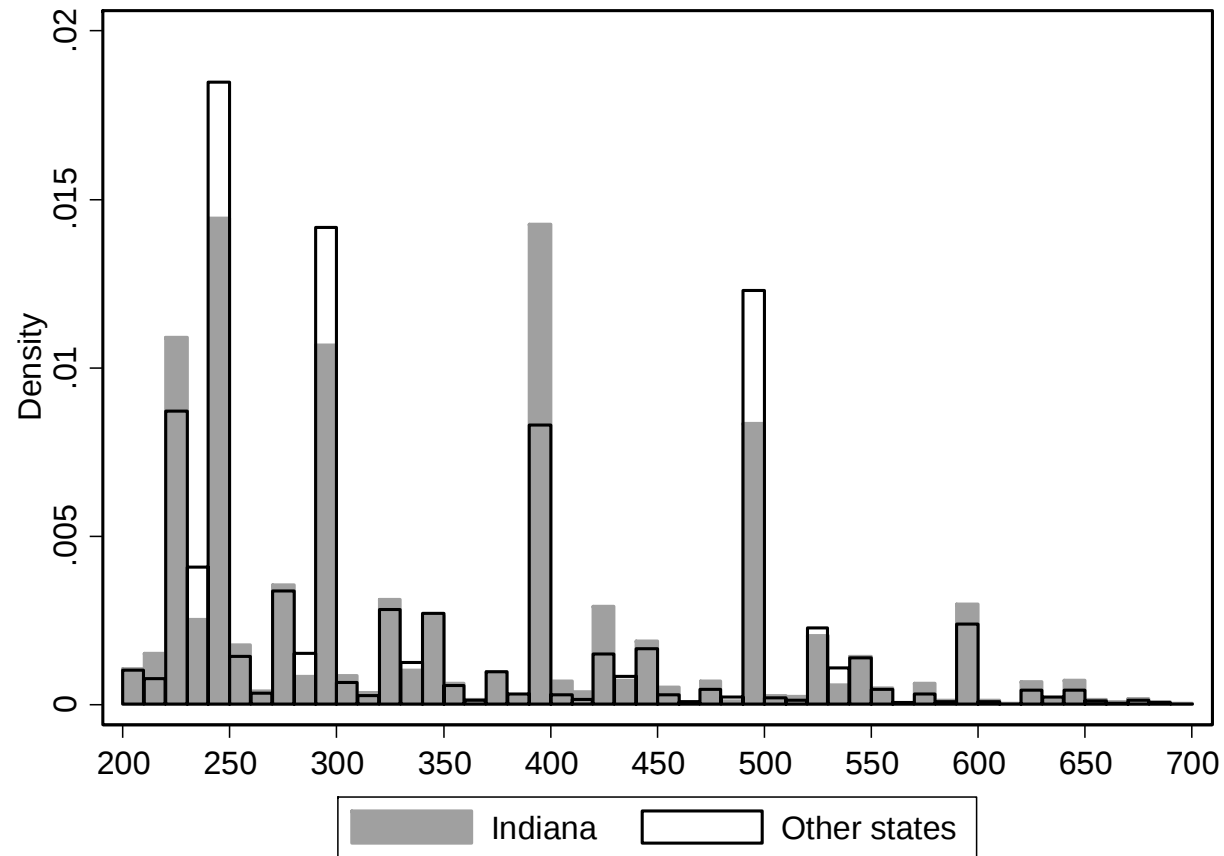
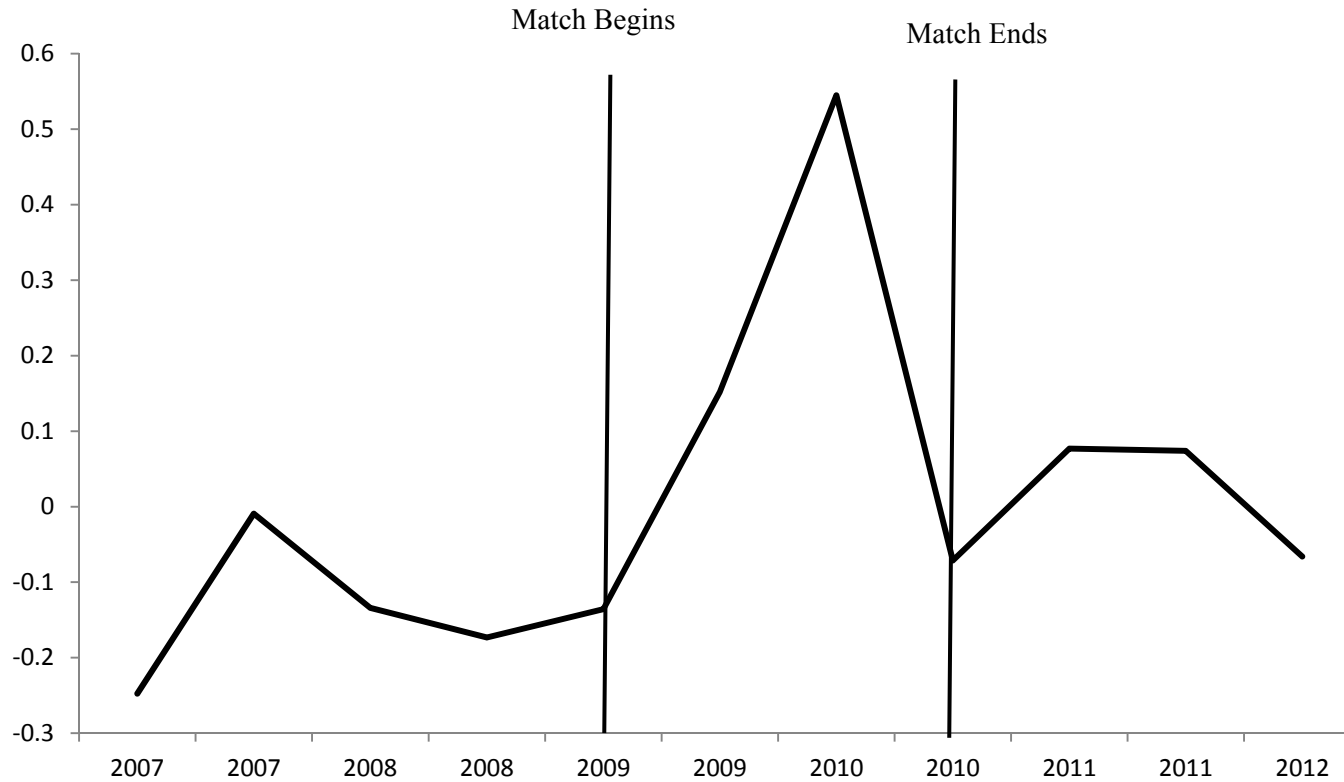


Figure 3. Joint gifts in Indiana and other states: A tax kink exists at \$400 in Indiana.



Note: The figure shows a histogram of joint gifts greater than \$200 and less than \$700 from Indiana (grey bars) and other states (clear bars) between 2004 and 2015 in bins of \$10. There are 75,068 gifts in the picture, of which 7,128 are from Indiana. Not shown is a large mass point for the bin (\$190, \$200] that is more than three times the mass point at the bin (\$290, \$300], for both Indiana residents and residents of other states. We did not show the \$200 mass point to allow the scale of the figure to more clearly display the density around \$400.

Figure 4. Log donation amounts: The 1960 class minus nearby classes.



Notes: The figure plots the difference between donated amounts from the 1960 class and the nearby classes 1954–1959, 1961–1965. For the 1960 class we aggregate the donations from all class members in each month, take the log, and then average the logged donations over six month periods from 2007 through 2012. Because December 2008 is the first month of the match, it is averaged in with the first half of 2009. We do the same thing for the nearby control classes, and take the difference.

Table 1. Summary statistics.

	Just Indiana	All States
1. Number of annual gifts	99,178	651,747
2. Average annual gift	1,192 (20,845)	2,832 (113,359)
3. Number of annual gifts < \$1,000,000	99,169	651,485
4. Average annual gift < \$1,000,000	1,041 (11,133)	1,563 (15,356)
5. Number of joint gifts	41,129	373,994
6. Average joint gift	1,604 (20,501)	3,221 (87,995)
7. Number of gifts between \$200 and \$1,000	12,026	123,556
8. Average annual gift between \$200 and \$1,000	391 (160)	377 (149)
9. Number of joint gifts between \$200 and \$1000	7,585	79,122
10. Average annual joint gift between \$200 and \$1000	383 (155)	374 (149)
11. Number of gifts from the 1960 Class [†]	956	10,793
12. Average gift from the 1960 Class [†]	401 (1,181)	4,568 (122,909)

Notes: The data describe gifts to the university between 2004 and the first five months of 2015.

Standard deviations in parentheses.

[†] Gifts are aggregated to the annual level (that is, all gifts made by a person during the year are combined) except for the last two rows, where non-aggregated gifts are reported. The average annual-level gift for the 1960 class is \$7,867 (sd = 289,914), and in Indiana it is \$852 (sd = 1,841).

Table 2. Educational giving and all giving in the *PSID*.

	Indiana residents who donate to education	Residents of all states who donate more than \$1,000 to all purposes combined
Age of household head	51 (1.60)	56 [†] (.20)
Household head married	0.81 (.05)	0.71 (.01)
Giving to educational institutions	466 (110)	237 [†] (18.9)
Giving to charitable organizations ^a	1,615 (269)	1,673 (43.8)
Giving to congregations	2,787 (538)	2,727 (56.3)
Giving to all purposes combined	4,403 (703)	4,401 (75.6)

Notes: The giving data are from the Philanthropy Panel Study, the generosity module within the *PSID*, waves 2005, 2007, and 2009. There are 68 observations in the first column, 6,407 in the second, and 38 in both columns. The averages are weighted, and standard errors are in parentheses.

The giving data are in nominal dollars. Using inflation-adjusted 2009 dollars produces very similar results: for columns 1 and 2 respectively the averages for education are 488 (117) and 247 (20); for charitable organizations: 1,670 (273) and 1,741 (45); for congregations: 2,888 (557) and 2,845 (59); and for all purposes combined: 4,559 (720) and 4,586 (78.8).

^a Includes giving to educational institutions.

[†] Indicates a statistically significant difference in means using a Wald test conducted with a seemingly unrelated regression that accounts for the covariance between the overlapping samples. The *p* value for equality of educational giving is 0.06, and the *p* value for equality of age is 0.009.

Table 3. Compensated tax-price elasticity estimates.

Method	Lower bound estimate	Point estimate (linear interpolation)	Upper bound estimate
Nearest round neighbor ^a	-.121 (.021)	-.197 (.024)	-.293 (.033)
Nearest neighbor ^b		-.465 (.036)	
Nearest neighbor ^c		-.290 (.019)	
Polynomial ^d		-.259 (.022)	
Nearest round neighbor ^e			
Use two bins below and two bins above the kink to estimate the counterfactual density (i.e., double the bandwidth)	-.111 (.022)	-.177 (.021)	-.257 (.023)
Double the bin width to \$50	.006 (.020)	-.098 (.019)	-.224 (.021)
Mass point at \$250 (instead of \$300)	.045 (.021)	-.136 (.023)	-.293 (.033)
Mass point at \$600 (instead of \$500)	-.121 (.021)	-.231 (.025)	-.477 (.035)

Notes: Bootstrapped standard errors are in parentheses. The estimates use equation (1) where $p_0 = 1/2$ and $p_1 = 1$.

^a Mass points at \$300 and \$500 used to identify counterfactual bunching at the kink. The lower bound is based on the mass point at \$300, the upper bound is based on the mass point at \$500, and the point estimate is based on linear interpolation between the \$300 and \$500 mass points. Amounts are placed in bins of width \$25, centered at round numbers (e.g., \$375, \$400, \$425, etc.). One bin below and one bin above the kink are used to estimate the counterfactual density.

^b Bandwidth = \$25. Therefore, the kink includes amounts given in the interval (\$387.50, \$412.50). One band below the kink (\$362.50, \$387.50] and one band above the kink (\$412.50, \$437.50) are used to identify counterfactual bunching at the kink, and to estimate the counterfactual density. The method is due to Saez (2010).

^c Bandwidth = \$50. Therefore, the kink includes amounts given in the interval (\$375, \$425). One band below the kink (\$325, \$375] and one band above the kink (\$425, \$475) are used to identify counterfactual bunching at the kink, and to estimate the counterfactual density.

^d Third-order polynomial in the amounts from \$200 to \$999 and three dummy variables at round numbers (at 25s, 50s and 100s) used to identify counterfactual bunching at the kink. Amounts are placed in bins of width \$25. One bin below and above the kink are used to estimate the counterfactual density. The method is due to Kleven and Waseem (2013).

^e Same estimator as in Note ^a but with the indicated modifications to the estimation parameters.

Table 4. Nearest round number estimator applied to placebo kinks.

Placebo kink at	Mass points used for identification	Lower bound estimate	Point estimate (linear interpolation)	Upper bound estimate
\$300	\$250 and \$500	.125 (.015)	.087 (.014)	-.096 (.019)
\$500	\$300 and \$600	.116 (.025)	-.110 (.022)	-.240 (.027)
\$600	\$500 and \$700	.383 (.046)	.119 (.026)	-.166 (.024)
\$700	\$600 and \$800	.334 (.068)	.228 (.055)	.098 (.043)
\$800	\$700 and \$900	-.057 (.020)	-.084 (.020)	-.112 (.023)
\$900	\$800 and \$1,100 ^a	.263 (.068)	.292 (.072)	.379 (.140)
\$1,000	\$500 and \$1,500	.221 (.028)	.063 (.014)	-.059 (.015)
\$1,500	\$1,000 and \$2,000	-.056 (.006)	-.078 (.006)	-.100 (.007)

Notes: Amounts are placed in bins of width \$25. One bin below and one bin above the kink used to estimate the counterfactual density. Bootstrapped standard errors are in parentheses. The estimates use equation (1) where $p_0 = 1/2$ and $p_1 = 1$.

^a The right mass point is set at \$1,100, even though \$1,000 may seem to be a more natural choice. When a right mass point at \$1,000 is used to estimate an “elasticity” at \$900, the quadratic formula that produces the elasticity estimate requires taking the square-root of a negative operand.

Table 5. Nearest round number estimator: Further results.

Sub-sample	Lower bound estimate	Point estimate (linear interpolation)	Upper bound estimate
Baseline	-.121 (.021)	-.197 (.024)	-.293 (.033)
Not eligible for the football lottery with a gift of \$200	-.223 (.051)	-.231 (.051)	-.242 (.063)
2004 – 2006	-.074 (.042)	-.171 (.045)	-.293 (.063)
2007 – 2015	-.133 (.023)	-.204 (.024)	-.293 (.032)
Number of years giving = 1 to 5 ^a	.108 (.100)	-.058 (.143)	-.438 (.276)
Number of years giving = 6 to 12 ^a	-.152 (.023)	-.211 (.023)	-.281 (.028)
Number of years giving = 1 to 5 ^b	-.118 (.113)	-.191 (.176)	-.338 (.408)
Number of years giving = 6 to 12 ^b	-.121 (.024)	-.197 (.025)	-.289 (.031)

Notes. Mass points at \$300 and \$500 used to identify counterfactual bunching at the kink. The lower bound is based on the mass point at \$300, the upper bound is based on the mass point at \$500, and the point estimate is based on linear interpolation between the mass points. Amounts are placed in bins of width \$25. One bin below and one bin above the kink used to estimate the counterfactual density. Bootstrapped standard errors are in parentheses. The estimates use equation (1) where $p_0 = 1/2$ and $p_1 = 1$.

^a The split of the sample into number of years giving “1 to 5” and “6 to 12” uses the only the years the person gave a gift designated as joint.

^b The split of the sample into number of years giving “1 to 5” and “6 to 12” uses the years the person gave a gift designated non-joint as well as years in which the person gave as joint. However, the years used in the estimation of the elasticities are only those in which the person gave as joint.

Table 6: Uncompensated tax-price elasticities: Percentile-based estimates.

Panel A: Baseline estimate and further results.

Baseline ^a	Heterogeneity		Lottery-ineligible ^d	Number of years giving ^e		Number of years giving ^f	
	Lower bound ^b	Upper bound ^c		1-5	6-12	1-5	6-12
-.265 (.042)	0 (.082)	-.429 (.012)	-.310 (.062)	-.288 (.109)	-.265 (.040)	-.417 (.123)	-.266 (.039)

Panel B: Placebo kinks.

	Placebo kink at:			
\$400 (actual kink)	\$401	\$450	\$500	\$550
-.265 (.042)	.069 (.055)	.096 (.068)	0.0 (.005)	-.056 (.027)

Notes: The estimates are from the percentile-based estimator using a range of donations $\Theta = [\$201, \$1,000]$; using other ranges produces similar results (see Appendix Table D). Bootstrapped standard errors are in parentheses. These estimations use equation (5), where $p_0 = 1/2$ and $p_1 = 1$.

^a The baseline estimate is formed by pooling donors in the Θ range from all control states.

^b The heterogeneity lower bound is formed by pooling donors in the Θ range separately in each control state that has at least 100 observations, and then selecting the control state whose marginal donor gives largest amount (see text).

^c The heterogeneity upper bound is formed as in the previous note, except that the control state selected is the one whose marginal donor gives smallest amount (see text).

^d The sub-sample not eligible for the football lottery with a gift of \$200.

^e The split of the sample into number of years giving “1 to 5” and “6 to 12” uses the only the years the person gave a gift designated as joint

^f The split of the sample into number of years giving “1 to 5” and “6 to 12” uses the years the person gave a gift designated non-joint as well as years in which the person gave as joint. However, the years used in the estimation of the elasticities are only those in which the person gave as joint.

Table 7. Response to a matching grant: Gift-level estimates; intensive margin.

	Baseline	Month by year dummies	State dummies	State by year dummies	Class trends
Matching treatment for the 1960 class	.151 (.049)	.149 (.047)	.150 (.047)	.148 (.047)	.152 (.041)
Implied elasticity	-.227 (.074)	-.224 (.071)	-.225 (.071)	-.222 (.071)	-.229 (.062)
Matching treatment placebo for the other classes ^a	Yes	Yes	Yes	Yes	Yes
25 th , 50 th , 75 th graduation anniversary dummies	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Class dummies	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	Yes	No
Month by year dummies	No	Yes	Yes	Yes	No
State by year dummies	No	No	No	Yes	No
Class trends	No	No	No	No	Yes

Notes: The dependent variable is the log of each checkbook donation. The sample are the $N = 471,861$ separate alumni gifts that are less than or equal to \$250,000; among these the average gift is about \$1,340 and the median is \$140. The first row presents the estimates on a dummy that equals one for the 1960 class during the time period of the match; standard errors are clustered by graduating class cohort (in parentheses). The second row converts the row 1 coefficients into elasticity estimates using equation (7) with $p_0 = 1/2$ and $p_1 = 1$; delta method standard errors (in parentheses).

^a A dummy that equals one for the other classes during the 19 month period around their 50th anniversaries.

Table 8. Response to a matching grant: Number of donation estimates; extensive margin.

	Baseline	Month by year dummies	State dummies	State by year dummies	Class trends
Matching treatment for the 1960 class	-.119 (.083)	-.098 (.081)	-.102 (.097)	-.118 (.097)	-.131 (.092)
Implied elasticity	.059 (.125)	.049 (.122)	.051 (.146)	.059 (.146)	.065 (.138)
Matching treatment placebo for the other classes ^a	Yes	Yes	Yes	Yes	Yes
25 th , 50 th , 75 th graduation anniversary dummies	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Class dummies	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	Yes	No
Month by year dummies	No	Yes	Yes	Yes	No
State by year dummies	No	No	No	Yes	No
Class trends	No	No	No	No	Yes

Notes: The dependent variable is the total number of donations by each class in a state, month, and year. There are 156,589 class x state x month x year cells.

The first row presents the estimates on a dummy that equals one for the 1960 class during the time period of the match; standard errors are clustered by graduating class cohort (in parentheses). The second row converts the row 1 coefficients into elasticity estimates using equation (7) with $p_0 = 1/2$ and $p_1 = 1$ and is calculated relative to the sample mean number of gifts; delta method standard errors (in parentheses).

^a A dummy that equals one for the other classes during the 19 month period around their 50th anniversaries.

Table 9. Response to a matching grant: Aggregated donation estimates.

	Baseline	Month by year dummies	State dummies	State by year dummies	Class trends	Post-match control
Matching treatment for the 1960 class	.113 (.062)	.126 (.062)	.127 ^b (.064)	.127 ^b (.064)	.119 (.041)	.126 (.063)
Implied elasticity	-.170 (.093)	-.188 (.093)	-.191 (.096)	-.191 (.096)	-.179 (.062)	-.189 (.045)
Dummy for the 12-month period after the match for the 1960 class	-	-	-	-	-	.002 (.025)
Implied post-match elasticity	-	-	-	-	-	-.003 (.038)
Matching treatment placebo for the other classes ^a	Yes	Yes	Yes	Yes	Yes	Yes
25 th , 50 th , 75 th graduation anniversary dummies	Yes	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Class dummies	Yes	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	Yes	No	No
Month by year dummies	No	Yes	Yes	Yes	No	Yes
State by year dummies	No	No	No	Yes	No	No
Class trends	No	No	No	No	Yes	No

Notes: The dependent variable is the (log of the) aggregated donation amount by each class in a state, month, and year. There are 156,765 class x state x month x year cells. The first row presents the estimates on a dummy that equals one for the 1960 class during the time period of the match; standard errors are clustered by graduating class cohort (in parentheses). The second row converts the row 1 coefficients into elasticity estimates using equation (7) with $p_0 = 1/2$ and $p_1 = 1$; delta method standard errors (in parentheses).

^a A dummy that equals one for the other classes during the 19 month period around their 50th anniversaries.

^b That the estimates in columns 3 and 4 appear to be identical is an artifact of rounding.