Does Price Matter in Charitable Giving? Evidence from a Large-Scale Natural Field Experiment

By DEAN KARLAN AND JOHN A. LIST*

We conducted a natural field experiment to further our understanding of the economics of charity. Using direct mail solicitations to over 50,000 prior donors of a nonprofit organization, we tested the effectiveness of a matching grant on charitable giving. We find that the match offer increases both the revenue per solicitation and the response rate. Larger match ratios (i.e., \$3:\$1 and \$2:\$1) relative to a smaller match ratio (\$1:\$1) had no additional impact, however. The results provide avenues for future empirical and theoretical work on charitable giving, cost-benefit analysis, and the private provision of public goods. (*JEL* D64, L31)

There is an extraordinary amount of money available. The lack is of good ideas on how to get the basket under the apple tree.
—Fundraising consultant Tony Kneer, *The Economist,* July 31, 2004

Private giving to charitable causes has significantly grown in the past several decades. Recent figures published by *Giving USA* show that in the United States, charitable gifts of money have been 2 percent or more of GDP since 1998, and more than 89 percent of Americans donate to charity (Aline Sullivan 2002). Experts predict that the combination of increased wealth and an ageing population will lead to an even higher level of gifts in the coming years (see, e.g., *The Economist,* July 31, 2004, 57). Such trends have left fundraisers, who are typically long on rules of thumb and short on hard scientific evidence, divided as to the most efficient means to attract these dollars. Indeed, even though the economics of charity has been well studied on the "supply" side, critical gaps remain on the "demand" side (James Andreoni 2006).

In an effort to better understand the economics of charity, we make use of a large-scale natural field experiment.[1] Specifically, we use a direct mail solicitation to explore whether, and to what extent, "price" matters in charitable fundraising. There is a rich and interesting literature that examines price effects via rebate mechanisms (such as changes in tax deductions) through which charitable contributions can be used to reduce one's tax burden (see, e.g., Charles T. Clotfelter 1985; William C. Randolph 1995; John Peloza and Peirs Steel 2005).[2] Overall, it is fair to say that the four decades of empirical estimates of these supply-side effects vary widely, and it is difficult to make strong inference from the various price effect estimates obtained (Gerald Auten, Holger Sieg, and Clotfelter 2002).[3] Laboratory experiments, on the other hand, typically find that the level of giving to

* Karlan: Department of Economics, Yale Univeristy, P.O. Box 208209, New Haven, CT 06520, Innovations for Poverty Action, and MIT Jameel Poverty Action Lab (e-mail: dean.karlan@yale.edu); List: Department of Economics, University of Chicago, Chicago, IL 60637, Innovations for Poverty Action, and NBER (e-mail: jlist@uchicago.edu). The editor and four anonymous reviewers provided quite useful remarks that considerably improved the study. The authors thank James Andreoni, Ian Bateman, Richard Carson, Bill Harbaugh, Donald Green, Glenn Harrison, Sendhil Mullainathan, David Reiley, Alois Stutzer, Lise Vesterlund, and participants at the University of Canterbury workshop on field experiments for comments. We thank Tomoko Harigaya, Sarojini Hirshleifer, and May Rostom from Innovations for Poverty Action for excellent research assistance. We thank the organization for working with us on the field experiment and the anonymous donors for providing the matching pledge.

[1] The term "natural field experiment" follows the classification scheme outlined in Glenn W. Harrison and List (2004).

[2] The charitable donation tax deduction was enacted in the United States in 1917 and has become quite important to taxpayers: the aggregate amount of these deductions in the United States from 2001 to 2005 is estimated to be \$145 billion (John Colombo 2001).

[3] Yet, the creative work of Auten, Sieg, and Clotfelter (2002) significantly advanced our understanding of the price effects, delivering persistent price elasticity estimates

others increases as price decreases (Andreoni and John Miller 2002). Nevertheless, 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 exists that suggests such framing matters (Catherine C. Eckel and Phillip J. Grossman 2003). In this study, we combine the attractive features of each of these lines of research by collecting data from a controlled field experiment in an actual fundraising effort.

We use a natural field experiment to explore the importance of price on charitable giving by measuring the comparative static effects of large changes in rates of matching gifts, a commonly employed tool used by fundraisers. A matching gift is a leadership gift that is a *conditional* commitment by a donor(s) to match the contributions of others at a given rate, up to the maximum amount the leadership donor is prepared to give. While the rate of matching is typically the result of an agreement between the fundraiser and the leadership donor, fundraising consultants ubiquitously note that increases in the matching ratio have noticeable power to influence future contributions. For instance, Kent E. Dove (2000, 15) reminds us that one should "never underestimate the power of a challenge gift" and that "obviously, a 1:1 match—every dollar that the donor gives is matched by another dollar—is more appealing than a 1:2 challenge … and a richer challenge (2:1) greatly adds to the match's attractiveness."

Such strong claims have lead fundraisers to make use of the perceived "extra" power of larger matching ratios. For example, a recent $50 million challenge grant gift to Drake University, which was among the 40 largest gifts in US history to an institution of higher education by an individual, was used to spur further gifts through 2:1 and 3:1 matching solicitations (Dove 2000). Such rules of thumb are largely anecdotal, as little scientific study has been completed to examine such demand side claims.

We take advantage of a capital campaign in which more than 50,000 prior donors to a US organization received direct mail solicitations seeking contributions. Individuals were randomly assigned to either a control group or a matching grant treatment group, and within the matching grant treatment group individuals were randomly assigned to different matching grant rates, matching grant maximum amounts, and suggested donation amounts.[4]

We find that simply announcing that match money is available considerably increases the revenue per solicitation—by 19 percent. In addition, the match offer significantly increases the probability that an individual donates—by 22 percent. Yet, while the match treatments relative to a control group increase the probability of donating, larger match ratios—$3:$1 (i.e., $3 match for every $1 donated) and $2:$1—relative to smaller match ratios ($1:$1) have no additional impact. The elasticity estimate of the price change from the baseline to the treatment groups, −0.30, is near the lower range of the elasticity of giving with respect to transitory price changes reported in Auten, Sieg, and Clotfelter (2002). Elasticity estimates over the price range of the matching treatments are roughly zero, however.

An important characteristic of our chosen charity is that it is politically oriented, and thus giving might be a form of political activism. Hence, the local political environment, or any other of a myriad of social factors that influence political activism, may affect the decision to contribute. For this reason, we explore whether treatment effects are spatially heterogeneous. We find that the matching gift result is driven by agents in states that voted for George W. Bush in the 2004 presidential election: the match increases the revenue per solicitation by 55 percent in "red" states whereas there was little effect observed in "blue" states. This result suggests that an individual's political environment also has the capacity to influence not only the level of giving, but also her responsiveness to different treatments.

Overall, these results have potential implications for practitioners in the design of fundraising campaigns, and provide avenues for future empirical and theoretical work on charitable giving. For instance, they suggest that the effect

of −0.79 to −1.26; the elasticity of giving with respect to transitory price changes is much smaller, −0.40 to −0.61.

[4] The matching challenges were made by anonymous supporters of the organization, and were conditional—not unconditional—agreements to contribute, as per the terms of this experiment.

of price is not as straightforward as believed, and call into question the accepted wisdom of fundraisers. The results could also provide insights into certain areas of policymaking, although clearly further work and replications are necessary. Practically, it speaks to state-of-the-art methods used to measure nonmarket values for cost-benefit assessments. The contingent valuation method (CVM), for example, is a survey technique commonly used to measure the economic value of a good or service. While hotly debated, some evidence in the CVM literature (e.g., Daniel Kahneman and Jack Knetsch 1992) suggests that individual values from CVM do not pass a "scope test": the value to a representative agent of saving 100 Peregrine falcons is not different from that of saving 100,000 (see also Peter A. Diamond and Jerry A. Hausman 1994). Of the dozens of studies that report data that pass or fail the scope test, we are unaware of any that use real stakes; rather they all ask "contingent" or hypothetical questions. In this light, our data might be viewed as a useful test of scope using an approach consistent with natural provision of a real public good.

The remainder of our study proceeds as follows. The next section summarizes the experimental design and places the contribution in relation to the literature. Section II provides the results. Section III concludes.

I. Experimental Design

Exploring the "demand side" of the economics of charity remains in its infancy. Yet, a recent flurry of work (Daniel Rondeau and List 2006; Stephan Meier 2006; Yan Chen, Xin Li, and Jeffrey K. Mackie-Mason 2006) that examines the effects of matching gifts on charitable giving has arisen simultaneously with our research. A matching gift is a *conditional* commitment by the leadership donor to match the contributions of others at a specific rate.[5]

Rondeau and List (2006) make use of a natural field experiment, dividing 3,000 direct mail solicitations to Sierra Club supporters into four treatments, comparing gifts across a seed money and matching treatment, where the matching gift is a promise to match at a 1:1 rate. Similar to List and Lucking-Reiley's (2002) natural field experiment, they find that the announcement of seed money worked well, but the 1:1 match worked less well. Yet, even though both generated greater contributions than the baseline, imprecise estimates prevented strong inference. Chen, Li, and Mackie-Mason (2006) implement four donation solicitation mechanisms similar in spirit to Rondeau and List (2006) in a natural field experiment on the Internet. Due to a very small number of contributors (24 people contributed in total), they cannot make strong inference across treatments, but they do find that the seed and matching mechanisms each generate significantly higher user click-through response rates. Meier (2006) makes use of an interesting experiment with Zurich undergraduate students to explore matching rates below 1:1 (0.25:1 and 0.5:1). Meier (2006) allocates roughly 265 subjects to each of the treatment conditions and examines their dichotomous decision (students can chose to contribute a certain amount or nothing). He finds that the 50 percent match increases the propensity to contribute, but estimate imprecision precludes strong inference across the 0.25:1 and 0.5:1 treatments. Interestingly, analyzing the trajectory of long-run giving rates, Meier finds that those who received the match give less than the control group.[6]

[5] This contrasts with a different use of leadership gifts—seed money—which is an *unconditional* commitment by a donor, or set of donors, to provide a given sum of money to the cause. List and David Lucking-Reiley (2002) found that increased seed money sharply increased both the participation rate of donors and the average gift size received from participating donors. They did not, however, explore the influence of matching rates.

[6] Field experiments that explore other aspects of the economics of charity have also witnessed a nice surge, and include, but are not limited to, Bruno Frey and Meier (2004), Jen Shang and Rachel Croson (2005), Catherine C. Eckel and Phillip J. Grossman (2006), Armin Falk (forthcoming), and Craig Landry et al. (2006). Although not a study of matching, Shang and Croson (2005), in particular, is of interest to the questions we pose. They examine the intensive margin by working with phone banks that receive inbound calls from public radio campaigns. Thus, they have a sample of individuals who have already decided to give during the current round of soliciting, and then examine which treatments alter the amount the individual chooses to give. Their results are quite intriguing in the sense that they report that reference points from "recent donors" matter greatly, particularly when the recent donor is of the same gender as the caller.

MATCHING GRANT
NOW IS THE TIME TO GIVE!

Troubled by the continued erosion of our constitutional rights, a concerned member has offered a matching grant [4 treatments: of $25,000; of $50,000; of $100,000; blank] to encourage you to contribute to at this time. To avoid losing the fight to defend our , this member has announced the following match: [3 treatments: $1; $2; $3] for every dollar you give. So, for every [3 treatments: HPC*1.00; HPC*1.25; HPC*1.50] you give, will actually receive [$x]. Let's not lose this match—please give today!

Figure 1

Our experimental approach differs from these previous efforts in that we employ a large-scale natural field experiment that affords us the opportunity to use several treatments and sub-treatments that span the range of design parameters that fundraisers are most likely to utilize. For example, we examine a set of match rates (those equal to and above 1:1) that are more commonly cited by fundraisers as dramatic and effective. In addition, we cross these price changes with variations in matching grant maximum amounts and suggested donation amounts. In doing so, we are able to provide deeper insights than heretofore have been possible. Furthermore, by conducting the experiment through a communication channel commonly used by large charities in the United States (direct mail), we are ensured that the results are of practical interest while providing a glimpse of behavior in the realm of decision-making that theorists' models purport to explain. This approach also permits a unique opportunity to introduce an analysis of heterogeneous treatment effects to the charitable giving literature.

The organization that we work with in the natural field experiment is a liberal nonprofit organization in the United States that works on social and policy issues relating to particular civil liberties. The organization is a charity under United States Internal Revenue Service code 501(c)3; hence, donations are tax-deductible for federal income taxes. This organization typically asks prior donors to send tax-deductible donations eight to ten times per year, and our field experiment was one of those fundraising drives. According to a 2002 survey of its donors, 70 percent of members are male, 60 percent are above 65 year of age, 80 percent have a college education, 30 percent are Christian, 25 percent are of no particular religious identity, 15 percent are Jewish, and 85 percent first donated to the organization after 1992. On politics, 85 percent self-reported voting for Gore in the 2000 presidential election, 3 percent for Bush, and 7 percent for Nader.

Our sample frame consists of all 50,083 individuals who have given to the organization at least once since 1991.[7] We assigned individuals randomly to two groups: a treatment "match" group (33,396, or 67 percent of the sample) and a control group (16,687 subjects, or 33 percent of the sample). All individuals received a four-page letter identical in all respects except two: (a) the treatment letters included an additional paragraph inserted at the top of the second page that announced that a "concerned fellow member" will match their donation, and (b) the reply card (see Figure 1) included in bold type the details of the match. For the control group, the reply card match language was replaced with a large logo of the organization.

The remainder of the letter, written and designed by the organization, conformed to their typical fundraising practices. The letter, sent in August of 2005, discussed a pressing national issue (Supreme Court nominations) that the organization was facing that particular month.

The specifics of the match offer were then randomized along three dimensions: the price ratio of the match, the maximum size of the matching gift across all donations, and the example donation amount suggested to the donor. Each of the subtreatments (ratio, maximum size of match, and example amount) was assigned with equal probability. Table 1 provides summary statistics that demonstrate the assignment to treatment and control was orthogonal to observable demographic information and prior giving history.

A. *Price Ratio*

As illustrated in Figure 1, we use three treatments for the price ratio (hereafter "ratio") of the match, $1:$1, $2:$1, and $3:$1. A $1:$1 ratio means that for every dollar the individual donates, the matching donor also contributes $1; hence, the charity receives $2. The $2:$1 ratio means that for every dollar the individual

[7] Individuals who have requested to be removed from mailing lists were excluded from this experiment, and large (over $1,000) prior donors were excluded.

TABLE 1—SUMMARY STATISTICS—SAMPLE FRAME
(*Mean and standard deviations)*

	All (1)	Treatment (2)	Control (3)
Member activity			
Number of months since last donation	13.007	13.012	12.998
	(12.081)	(12.086)	(12.074)
Highest previous contribution	59.385	59.597	58.960
	(71.177)	(73.052)	(67.269)
Number of prior donations	8.039	8.035	8.047
	(11.394)	(11.390)	(11.404)
Number of years since initial donation	6.098	6.078	6.136
	(5.503)	(5.442)	(5.625)
Percent already donated in 2005	0.523	0.523	0.524
	(0.499)	(0.499)	(0.499)
Female	0.278	0.275	0.283
	(0.448)	(0.447)	(0.450)
Couple	0.092	0.091	0.093
	(0.289)	(0.288)	(0.290)
Census demographics			
Proportion white	0.830	0.831	0.830
	(0.172)	(0.171)	(0.173)
Proportion black	0.062	0.061	0.062
	(0.123)	(0.122)	(0.125)
Proportion aged between 18 and 39 years	0.297	0.297	0.298
	(0.132)	(0.132)	(0.132)
Average household size	1.994	1.999	1.986
	(1.001)	(0.998)	(1.006)
State and county			
Red state–proportion that live in red state	0.404	0.407	0.399
	(0.491)	(0.491)	(0.490)
Red county–proportion that live in red county	0.510	0.512	0.507
	(0.500)	(0.500)	(0.500)
State-level activity of organization			
Nonlitigation	2.474	2.485	2.453
	(1.962)	(1.966)	(1.953)
Cases	1.500	1.499	1.502
	(1.155)	(1.157)	(1.152)
Observations	50,083	33,396	16,687

Notes: Nonlitigation is the count of incidences relevant to this organization from each state reported in 2004−2005 (values range from zero to six) in the organization's monthly newsletter to donors. "Court cases" is the count of court cases from each state in 2004−2005 in which the organization was involved (values range from zero to four).

donates, the matching donor contributes $2, etc. (subject to the maximum amount across all donations, as discussed above).

A theoretical framework in the spirit of Andreoni's (1989, 1990) impure altruism model provides ambiguous predictions as to the direction of the price effect in this setting (see Karlan and List 2005).[8] The simplest prediction is that

[8] We also provide a sketch of the model and how it relates to our empirical findings on the *AER* Web site (http://www.e-aer.org/data/dec07/20060421_data.zip). Such a model can be traced to a footnote in Gary S. Becker (1974). Richard Cornes and Todd Sandler (1984) and Richard Steinberg (1987) develop rich models of cases of mixed public/private goods. Andreoni (2006) provides an excellent overview of the general model as well as supply-side elasticity estimates. There are important alternative modeling approaches to this framework. For example, some have considered moral or group interested behavior (see, e.g., Jean-Jacques Laffont 1975; Amartya K. Sen 1977; and Robert Sugden 1984). In Sugden (1984), for instance, agents adhere to a "moral constraint," whereby they compare themselves to the least generous person when making their contributions. Relatedly, in B. Douglas Bernheim's

the matching gift effectively lowers the price of the public good and thus demand for the public good increases. Other explanations make the same prediction, however. For instance, the announcement of the availability of a leadership gift might reduce or eliminate any uncertainty about the credibility and value of a charitable organization or the particular task at hand, increasing rates of giving and the level of public good provision. Similarly, a match announcement may represent a timing-signalling effect, effectively changing the perception of the importance of the gift *now* to the nonprofit. These associative mechanisms fit under "signalling" models of sequential giving (Lise Vesterlund 2003).[9]

Alternatively, individuals may perceive the matching grant as a marketing trick, either believing the money will be paid regardless of the amount raised, or perhaps not paid at all. In either case, this would cause the match to have no influence on giving (or perhaps even a negative effect if it harms the reputation of the charity) and thus would cause an underestimate of the elasticity of giving with respect to price. It does not, however, generate an underestimate of the impact of a matching grant utilized in the field, which is directly relevant for charities as they raise funds for their public goods. Yet, even in the case where subjects find the matching-grant story credible, a matching grant can decrease donations because it might place the individual on a different portion of their utility function, potentially reducing the marginal utility of the public good. Such a decrease in giving depends on the match ratio, the agent's belief about others' giving, and the maximum size of the grant.

Another important alternative prediction arises when the number of agents grows large. As David C. Ribar and Mark O. Wilhelm (2004) show, as the agent pool is expanded, the relative importance of one's utility from altruism diminishes and, in the limit, choices are driven solely by "warm glow." Andreoni (2006) shows that similar results can be achieved by allowing the size of the charity to grow. In this case, in the limit individuals might gain no marginal utility from the actual provision of the public good, but simply purchase "moral satisfaction" when contributing. An empirical example of this variant of the model is described in Kahneman and Knetsch (1992). In their study, and in several subsequent studies (see, e.g., Jonathan Baron and Joshua D. Greene 1996, and the citations therein), a recurrent finding of hypothetical valuation exercises (contingent valuation) is that the value assigned to a public good does not depend on the quantity, or "scope," of the good in question. For example, Kahneman and Knetsch (1992) report that agents have a similar willingness to pay to improve sport fish stocks in British Columbia fresh water as they do for all of Canadian fresh water. Likewise, they report that famine relief in Ethiopia is valued similarly to famine relief across the whole continent of Africa. In effect, agents are insensitive to quantity, or "price," changes.

Such results are fiercely debated in the literature[10] and certainly could be due to the hypothetical nature of the exercise, but they do provide an important alternative prediction: if utility is solely a function of one's own contribution, then a stark prediction is that there should be an insensitivity of individual contributions

(1994) conformity model, agents value status, and behavioral departures from the social norm impair status. George A. Akerlof (1982) obtains similar conformity results by assuming that deviations from social norms have direct utility consequences.

[9] Intuitively, it is important to keep in mind that under a match the total provision of the public good is the product of gifts and the match rate. This contrasts with using leadership gifts as seed money, where the level of public good results from the summation of gifts and the level of seed monies available. Since seed money unconditionally increases the existing provision level of the public good, marginal utility may be reduced, leading to lower individual contributions (see Craig E. Landry et al. 2006). Andreoni (1998) uses a neat theoretical construct to explore a different effect of seed money in threshold public good provisioning: his model of charitable giving has multiple equilibria, and in the absence of seed money there exists a Nash equilibrium with zero charitable giving. The zero-contribution equilibrium can be eliminated, however, by initial commitments of seed money, which lower the remaining amount needed to be raised in the public fundraising campaign. Thus, in his model seed money is used as an elimination device rather than as a credibility device.

[10] Indeed, the Kahneman and Knetsch (1992) study remains one of the most highly cited papers ever published in the *Journal of Environmental Economics and Management*.

to changes in the matching rate.[11] Recent theories of social preferences refine this prediction by suggesting that agents are "conditionally" cooperative, or might be willing to contribute more to the public good if they learn that others have contributed, regardless of the magnitude of these previous contributions. The underlying mechanisms at work in such behavioral patterns include models of conformity, social norms, and reciprocity (see the discussion in Bruno S. Frey and Meier 2004). In this light, the presence of *any* matching ratio might influence a warm glow effect from giving, leading to higher individual contributions in the matching treatments compared to the controls.

B. *Maximum Size of the Matching Grant*

We test four treatments for the maximum matching grant amount: $25,000, $50,000, $100,000, and unstated. For similar reasons, as discussed above, a simple theoretical sketch provides ambiguous predictions as to whether a larger maximum amount (which makes the matching grant more likely to be relevant for the donor) will lead to a higher response rate, contribution level, and greater provision of the public good.

C. *Ask Amount*

At the top of the reply card, the organization includes three individual-specific suggested amounts equal to (a) the individual's highest previous contribution, (b) 1.25 times the highest previous contribution, and (c) 1.50 times the highest previous contribution (all appropriately rounded). In the matching grant paragraph, we randomly chose one of the three suggested amounts from the reply card and used that as an example to illustrate the effect of the grant on the amount the charity would receive.

Again, our theory provides ambiguous predictions. For instance, a higher suggested amount may influence the nature of the warm glow effect, increasing giving. Yet, if the "moral satisfaction" is deemed too costly, and the individual does not consider giving less than the example amount, then a higher example amount may make individuals less likely to contribute. In fact, Mal Warwick (2003) finds that the net effect of lowering the ask amounts on the reply card typically increases the revenue (response rate typically increases, and amount given rarely changes).[12]

D. *Heterogeneous Treatment Effects*

Because we are fundraising for a liberal politically motivated group and sending solicitations to all 50 states, it is possible that the observed treatment effects are heterogeneous across different solicitees and different environments. For example, some researchers have argued that solicitee income level is a key determinant of the price elasticity of charitable donations (see, e.g., Auten, Sieg, and Clotfelter 2002; Donna M. Anderson and Ruth Beier 1999). Further, W. E. Lindahl (1995) identifies the length of relationship as a key variable in charitable fundraising. In addition, it is possible that utilitarian effects of contributing to our politically motivated charity are different spatially due to the local political environments. To test for these effects, we merge our charitable giving data with:

- Demographic data from the Census, aggregated at the zip code level;
- State and county returns from the 2004 presidential election; and
- Data from the organization on frequency of their activities within each state.

II. Experimental Results

Tables 2A and 2B present summary statistics and provides the core experimental results. In the table we focus on two measures: (a) a binary variable equal to one if any charitable contribu-

[11] Even in cases where individuals gain marginal utility from the actual provision of the public good, under a set of reasonable assumptions, the Karlan and List (2005) model yields the possibility of a matching grant decreasing donations via the interplay between the match ratio, the agent's belief about others' giving, and the maximum size of the grant.

[12] The "ask amount" refers to the amounts on the reply card, whereas we have tested the example amount within the matching grant offer language (holding constant the ask amounts). Hence, we have not tested exactly what is reported in Mal Warwick (2003), yet the similarities warrant comparing the results.

Table 2A—Mean Responses
(Mean and standard errors)

	Control	Treatment	Match ratio 1:1	2:1	3:1
Implied price of $1 of public good:	1.00	0.36	0.50	0.33	0.25
Panel A	(1)	(2)	(3)	(4)	(5)
Response rate	0.018	0.022	0.021	0.023	0.023
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Dollars given, unconditional	0.813	0.967	0.937	1.026	0.938
	(0.063)	(0.049)	(0.089)	(0.089)	(0.077)
Dollars given, conditional on giving	45.540	43.872	45.143	45.337	41.252
	(2.397)	(1.549)	(3.099)	(2.725)	(2.222)
Dollars raised per letter, not including match	0.81	0.97	0.94	1.03	0.94
Dollars raised per letter, including match	0.81	2.90	1.87	3.08	3.75
Observations	16,687	33,396	11,133	11,134	11,129
Panel B: Blue states					
Response rate	0.020	0.021	0.021	0.022	0.021
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Dollars given, unconditional	0.897	0.895	0.885	0.974	0.826
	(0.086)	(0.059)	(0.102)	(0.110)	(0.091)
Dollars given, conditional on giving	44.781	42.444	42.847	44.748	39.635
	(2.914)	(1.866)	(3.356)	(3.456)	(2.838)
Dollars raised per letter, not including match	0.90	0.89	0.88	0.97	0.83
Dollars raised per letter, including match	0.90	2.66	1.77	2.92	3.30
Observations	10,029	19,777	6,634	6,569	6,574
Panel C: Red states					
Response rate	0.015	0.023	0.021	0.024	0.026
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Dollars given, unconditional	0.687	1.064	0.987	1.103	1.101
	(0.093)	(0.085)	(0.157)	(0.148)	(0.135)
Dollars given, conditional on giving	47.113	45.490	47.667	46.110	43.161
	(4.232)	(2.607)	(5.848)	(4.392)	(3.507)
Dollars raised per letter, not including match	0.69	1.06	0.99	1.10	1.10
Dollars raised per letter, including match	0.69	3.23	1.97	3.31	4.40
Observations	6,648	13,594	4,490	4,557	4,547

tion is made within one month after the direct mail solicitation, and (b) a continuous variable for the amount given. As panel A indicates, in total we raised \$45,860 in the fundraising drive: \$13,566 in the control groups and \$32,294 in the matching treatments. (Note that twice as many matching letters were sent, so a simple comparison is misleading.) In the matching treatments, we raised \$10,431, \$11,423, and \$10,439 in the \$1:\$1, \$2:\$1, and \$3:\$1 treatments, respectively (not including the match amount). This amounted to \$0.813, \$0.937, \$1.026, and \$0.938 in terms of revenue per solicitation in the control, \$1:\$1, \$2:\$1, and \$3:\$1 treatments, respectively.

In terms of the other treatment variables, the figures suggest that neither the match threshold nor the example amount had a meaningful influence on behavior. One could posit that the matching ratio should be *less* effective when the match threshold is lower (because the higher match ratio makes the threshold more likely to be reached, *ceteris paribus*). Although our estimates are imprecisely measured, after interacting the match ratios and threshold amounts fully, we do not find systematic patterns for the interaction effects. In fact, the point estimate indicates the opposite: in a probit regression of giving regressed on threshold, ratio, and the

Table 2B—Mean Responses
(*Mean and standard errors*)

	Match							
		Threshold				Example amount		
	Control	\$25,000	\$50,000	\$100,000	Unstated	Low	Medium	High
Implied price of \$1 of public good:	1.00	0.36	0.36	0.36	0.36	0.36	0.36	0.36
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Response rate	0.018	0.022	0.022	0.022	0.022	0.021	0.022	0.023
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Dollars given, unconditional	0.813	1.060	0.889	0.903	1.015	0.914	1.004	0.983
	(0.063)	(0.109)	(0.091)	(0.084)	(0.106)	(0.080)	(0.091)	(0.084)
Dollars given, conditional on giving	45.540	49.172	39.674	41.000	45.815	43.107	45.239	43.251
	(2.397)	(3.522)	(2.900)	(2.336)	(3.475)	(2.557)	(2.932)	(2.542)
Dollars raised per letter, not including match	0.81	1.06	0.89	0.90	1.01	0.91	1.00	0.98
Dollars raised per letter, including match	0.81	3.32	2.63	2.65	2.99	2.83	2.92	2.96
Observations	16,687	8,350	8,345	8,350	8,351	11,134	11,133	11,129
Panel B: Blue states								
Response rate	0.020	0.020	0.022	0.022	0.020	0.019	0.022	0.022
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dollars given, unconditional	0.897	0.884	0.912	0.900	0.884	0.796	0.950	0.939
	(0.086)	(0.115)	(0.127)	(0.110)	(0.116)	(0.094)	(0.108)	(0.102)
Dollars given, conditional on giving	44.781	43.204	41.091	41.236	44.469	41.516	43.194	42.503
	(2.914)	(3.716)	(4.227)	(3.093)	(3.806)	(3.283)	(3.364)	(3.063)
Dollars raised per letter, not including match	0.90	0.88	0.91	0.90	0.88	0.80	0.95	0.94
Dollars raised per letter, including match	0.90	2.83	2.72	2.50	2.60	2.38	2.78	2.82
Observations	10,029	5,035	4,954	4,856	4,932	6,574	6,550	6,653
Panel C: Red states								
Response rate	0.015	0.023	0.023	0.022	0.025	0.024	0.022	0.024
	(0.001)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Dollars given, unconditional	0.687	1.330	0.856	0.874	1.206	1.086	1.082	1.023
	(0.093)	(0.212)	(0.127)	(0.124)	(0.199)	(0.141)	(0.158)	(0.141)
Dollars given, conditional on giving	47.113	57.156	37.649	39.584	47.330	44.929	48.097	43.519
	(4.232)	(6.485)	(3.643)	(3.462)	(6.039)	(4.005)	(5.234)	(4.318)
Dollars raised per letter, not including match	0.69	1.33	0.86	0.87	1.21	1.09	1.08	1.02
Dollars raised per letter, including match	0.69	4.08	2.51	2.80	3.57	3.48	3.11	3.11
Observations	6,648	3,309	3,385	3,487	3,413	4,549	4,579	4,466

interaction of the threshold and ratio, we find a negative but statistically insignificant coefficient on the interaction term (results are available upon request).

As a first basic examination of these giving rates, we use a distribution free test to explore whether the contribution amounts vary across treatment. Using a signed-rank Wilcoxon test, we find that the distribution of gifts in the matching treatments is situated to the right of the distribution of gifts in the control treatment at the $p < 0.05$ level. Yet, the gift distributions across the various matching ratios are not significantly different from one another.

Next, we impose parametric assumptions to estimate the effect of the match (and its different features) on the likelihood of giving. Using probit models, we estimate the following two specifications:

$$Y_i = \alpha_0 + \alpha_1 T_i + \varepsilon_i; \tag{1}$$

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 T_i S_i + \beta_3 T_i P_i + \beta_4 T_i X_i + \varepsilon_i, \tag{2}$$

where Y_i is a binary variable equal to one if individual i donated within one month of receiving the solicitation; T_i equals one if individual i received any of the match offers; S_i is a vector of three indicator variables for three of the four match sizes (the omitted category is unstated); P_i is a vector of two indicator variables for two of the three price ratios (the omitted category is \$1:\$1); and X_i is a vector of two indicator variables for two of the three example amounts (the omitted category is the low example amount).

Table 3 presents the basic experimental results on the likelihood of contributing, and also examines heterogeneous treatment effects based on whether the individual had given previously in 2005. We find the match is slightly more effective for those who had not yet given in 2005 (columns 3 and 4 versus columns 5 and 6). In results not shown, we find that the match is significantly more effective for small prior donors (below the median \$35 gift) than large prior donors.

We also model the amount given as the outcome of interest. This analysis necessarily confounds two effects: the match may alter the type of person who responds (i.e., those predisposed to give large versus small amounts), and may alter the amount given conditional on giving. We estimate two specifications on both the full sample and the restricted sample of those who gave:

$$A_i = \alpha_0 + \alpha_1 T_i + \varepsilon_i; \tag{3}$$

$$A_i = \beta_0 + \beta_1 T_i + \beta_2 T_i S_i + \beta_3 T_i P_i + \beta_4 T_i X_i + \varepsilon_i, \tag{4}$$

where A_i is a continuous variable equal to the amount donated within one month of receiving the solicitation (we also estimated equations (1) and (2) ((3) and (4)) simultaneously in a two-stage selection model and the empirical results are similar).[13]

In Table 4, for the amount given (A_i), panel A reports results for the full sample, and panel B reports results restricting the sample to the individuals who responded ($Y_i = 1$). Columns 7 and 8 use the change, rather than level, of giving as the dependent variable. Panel A specifications combine the effect on response rate with the effect on amount given, thus providing the aggregate effect on charitable giving. This is particularly important from the fundraiser's perspective in determining optimal demand-side considerations to maximize charitable giving. Panel B specifications allow us to remove the average effect on the response rate from the estimate, but two effects remain: the match may attract individuals with higher (or lower) typical giving amounts, and of course the match may change the amount an individual gives. Thus, the specifications in panel B no longer adhere to the experimental design, since a selection effect confounds the incentive effects on amount given. For this reason, we emphasize results in panel A for drawing inference.

Our data also permit a rough estimation of the price elasticities of giving. When considering price movements from the control to the treatment cells, we estimate that elasticity of giving to be -0.225,[14] and on subsamples the estimate ranges from completely inelastic (states lost by Bush in the 2004 presidential election) to as large as -0.668 (states won by Bush in the 2004 presidential election). A potential comparison to these numbers is the estimated price elasticities of charitable tax deductions in the literature. Ever since Michael K. Taussig's (1967) original estimates of the effect of changes in tax deductibility, four decades of research have provided estimates of the price elasticities.[15] Our estimates are in the range of several previous studies, although certain assumptions must be invoked when transferring from the context of a "match" to the context of a tax system "rebate" (for evidence of the failure of rebates and matches to generate similar results, despite similarity mathematically, see Eckel and Grossman 2003, 2006). For example, Andreoni, William G. Gale, and John K. Scholz (1996) report a

[13] Results are available upon request from the authors.

[14] This is calculated from Table 2A, panel A, columns 1 and 2, on total dollars contributed (not including the match) per letter sent. The dollars raised increased by 19 percent, and the average price to "buy" \$1 of the public good was \$0.36 (hence a decrease of 64 percent), which implies an elasticity of -0.30 before taxes. Assuming a 25 percent marginal tax rate, the elasticity is then -0.225.

[15] For early surveys, see Charles T. Clotfelter (1985) and Steinberg (1990). John Peloza and Peirs Steel (2005) update these surveys and examine price elasticities with a meta-analysis.

TABLE 3—PRIMARY REGRESSION RESULTS
Probit, dependent variable = donated (binary)

	All		Already gave in 2005		Had not given yet in 2005	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.004***	0.002	0.003**	−0.001	0.005**	0.005
	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.004)
Treatment * 2:1 ratio		0.002		−0.001		0.005
		(0.002)		(0.002)		(0.004)
Treatment * 3:1 ratio		0.002		−0.001		0.006
		(0.002)		(0.002)		(0.004)
Treatment * $25,000 threshold		−0.001		0.003		−0.004
		(0.002)		(0.003)		(0.004)
Treatment * $50,000 threshold		0.000		0.004		−0.003
		(0.002)		(0.003)		(0.004)
Treatment * $100,000 threshold		−0.000		0.006**		−0.006
		(0.002)		(0.003)		(0.003)
Treatment * medium example amount		0.001		0.003		−0.001
		(0.002)		(0.002)		(0.003)
Treatment * high example amount		0.001		0.002		0.001
		(0.002)		(0.002)		(0.003)
Pseudo r-squared	0.001	0.001	0.002	0.005	0.001	0.002
Observations	50,083	50,083	26,217	26,217	23,866	23,866

Notes: Omitted subtreatments are 1:1 ratio, unstated threshold, and low example amount. Standard errors in parentheses.
*** Significant at, or below, 1 percent.
** Significant at, or below, 5 percent.
* Significant at, or below, 10 percent.

price elasticity of −0.35, and Bruce R. Kingma (1989) estimates the elasticity for public radio contributions to be −0.43 (although Sonia H. Manzoor and John D. Straub (2005) obtain different empirical estimates than Kingma (1989) using updated data). Likewise, our estimates are consistent with estimates of the elasticity of giving with respect to transitory price changes reported in Auten, Sieg, and Clotfelter (2002). Note that these studies on taxation policy calculate the elasticity using the gross amount given by the donor (thus, *including* the rebate). We similarly use the gross amount given by the donor, which implies we are *not* including the match amount in our calculations.

A. *Heterogeneous Spatial Treatment Effects*

Panels B and C in Table 2 provide summary statistics for blue states (voted for John Kerry in 2004) and red states (voted for Bush in 2004) to provide a sense of the spatial variability of our estimates. Empirical results are stark. Overall, the response rate in blue states is higher than in red states, but is equivalent across the treatment and control groups (treatment = 2.1 percent; control = 2.0 percent). Alternatively, the response rate in red states is significantly higher for the treatment than the control group (treatment = 2.3 percent; control = 1.5 percent). Note that, whereas the level of giving is much higher in the blue states than in the red states (1.5 percent in red versus 2.0 percent in blue) under the control condition, the level of giving is roughly equivalent under the treatment condition (2.3 percent in red versus 2.1 percent in blue). The summary statistics again show insignificant responsiveness for all other treatments—size and suggested amount—across both red and blue states.

Table 5 presents the econometric results by political environment of the individual's state (whereas Table 2 panel B, versus panel C, showed the summary statistics). We employ four measures: the vote share by state for Bush in the 2004 general presidential election, the vote share by county for Bush, the number of court cases between 2002 and 2005 by state in which this organization was either a party to or filed a brief, and the number of non–court case incidents between 2002 and 2005 by state reported in this organization's newsletter to its members.

These measures do not incorporate the intensity or importance of any given court case or incident. Hence they are noisy measures of the level of activity of this organization within each state, and even noisier measures of the perception of the individuals of the local activity of the organization in their state.

Panel A of Table 5 shows clearly that the matching grant treatment was ineffective in blue states, yet quite effective in red states. The nonlinearity is striking, as noted by comparing columns 4 and 5: the differential response rate for states in which Bush narrowly lost (47.5 percent to 49.9 percent) was 0.2 percent points, whereas the differential response rate for states in which Bush narrowly won (50.0 percent to 52.5 percent) was 1.6 percent points. Figure 2 plots the coefficients from the eight regressions in panel A of Table 5. Figures 3 and 4 plot the response rates for each state, where each bubble is sized proportionally to the number of observations in the dataset. Figure 3 plots Bush's vote share on the x-axis and the overall response rate on the y-axis, demonstrating a slight downward slope: individuals in red states, on average, give less. Figure 4 plots Bush's vote share on the x-axis and the differential response rate for the match on the y-axis, demonstrating that no particular outlier states are driving the red/blue state difference.

Given the striking nature of our red/blue state result, it is important to take care to examine the robustness of this result. Analytically, many explanations could be provided for why individuals in red versus blue states are more (or less) likely to give to a liberal organization. The finding here, however, is that individuals in red states are more *responsive* to a matching grant offer, increasing the likelihood of contributing but not the amount given. The level effect for treatment groups is the same for red and blue states, whereas the control group for the red states is lower than the control group for the blue states.

As noted earlier, some scholars have argued that income level is a key determinant of donor responsiveness (see, e.g., Hamilton Lankford and James Wyckoff 1992; Auten, Sieg, and Clotfelter 2002). In this spirit, our results might be capturing underlying demographic differences between red and blue state contributors. To test this explanation, we merge our data with demographic census data aggregated at the zip code level. Table 6 summarizes these results. Upon interacting treatment with education, income, racial composition, age, household size, home ownership, number of children, and an urban/rural indicator, the coefficient on the interaction term red*treatment remains robust.[16] We also test, and reject, that the red/blue state finding is driven by underlying differences in intensity of prior support for the organization (Table 6, column 1).[17]

[16] Not shown due to space considerations, a regression of amount given regressed on the same treatment, demographic terms, and interaction terms interacted with treatment yield similar qualitative results (i.e., the interaction of red state and treatment does not change). Although few demographic variables are predictive of the binary decision to give (since this is a sample of prior donors, this is not surprising), higher median wealth and home ownership, and smaller household sizes, are correlated with higher giving amounts.

[17] Given the robustness of these results, we empirically explored three specific possible explanations for the success of the matching grant in red but not blue states. First, we examined whether the immediate political environment of the individual (perhaps capturing the political leaning of those they interact with most often) matters, or whether it is indeed the state. We find that the political leaning of the county is irrelevant: individuals living in red counties in blue states behave similarly to individuals living in blue counties in blue states (and do not respond to the match), and individuals living in blue counties in red states behave just like individuals living in red counties in red states (and respond significantly to the match). Perhaps individuals are more responsive to price when they are considering goods for personal consumption. If individuals in red states perceive this organization to be engaged in work that could directly affect their lives, then perhaps they are more responsive because of the private return to this organization's work. This would suggest that the red versus blue state differential was masking an omitted variable, the organization's local activity. In complementary models we examine this hypothesis by including controls and interactions for the organization's activity. Including these variables does not change the core result that the matching grant worked only in red states. Furthermore, it is useful to note that the organization has never solicited individuals differently based on the state in which they live. Lastly, perhaps the red versus blue state merely captures an observable difference in the dedication or passion of the individual donors. With no survey data available on these individuals, the only measure we have available is prior giving. We ran alternative models to examine whether the red versus blue state finding is robust to the inclusion of controls and interactions for prior giving. Indeed, those variables do not matter, and the red/blue state distinction remains the largest determinant of responding to the matching grant offer. A theory from social psychology, untestable directly with our data and experimental design, argues that individuals

TABLE 4—PRIMARY REGRESSION RESULTS
(OLS, dependent variable = dollars donated)

Dependent variable:	Amount given						Change in amount given	
Panel A: Unconditional on giving	All		Already gave in 2005		Did not give in 2005		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.154*	0.118	0.152	0.102	0.157	0.142	6.331	−18.650
	(0.083)	(0.151)	(0.093)	(0.170)	(0.140)	(0.255)	(12.013)	(21.925)
Treatment * 1:1 ratio		omitted		omitted		omitted		omitted
Treatment * 2:1 ratio		0.089		−0.027		0.216		17.945
		(0.117)		(0.132)		(0.198)		(16.985)
Treatment * 3:1 ratio		0.001		−0.114		0.121		18.583
		(0.117)		(0.132)		(0.197)		(16.986)
Treatment * unstated maximum		omitted		omitted		omitted		omitted
Treatment * $25,000 maximum		0.045		0.024		0.068		25.256
		(0.135)		(0.152)		(0.228)		(19.612)
Treatment * $50,000 maximum		−0.126		0.066		−0.337		25.191
		(0.135)		(0.152)		(0.228)		(19.615)
Treatment * $100,000 maximum		−0.111		0.046		−0.289		24.000
		(0.135)		(0.152)		(0.228)		(19.612)
Treatment * low example gift		omitted		omitted		omitted		omitted
Treatment * medium example gift		0.090		0.220*		−0.053		0.315
		(0.117)		(0.132)		(0.197)		(16.985)
Treatment * high example gift		0.069		−0.032		0.179		−18.214
		(0.117)		(0.132)		(0.197)		(16.987)
Constant	0.813***	0.813***	0.423***	0.423***	1.241***	1.241***	−56.893***	−56.893***
	(0.067)	(0.067)	(0.076)	(0.076)	(0.114)	(0.114)	(9.81)	(5.80)
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	50,083	50,083	26,217	26,217	23,866	23,866	50,083	50,083
Panel B: Conditional on giving	All		Already gave in 2005		Did not give in 2005		All	
Treatment	−1.668	0.686	−0.180	15.884	−2.425	−3.419	3.172	4.281
	(2.872)	(5.036)	(6.571)	(11.781)	(3.104)	(5.426)	(1.859)	(3.263)
Treatment * 1:1 ratio		omitted		omitted		omitted		omitted
Treatment * 2:1 ratio		−0.138		4.720		−0.862		2.976
		(3.811)		(8.231)		(4.211)		(2.469)
Treatment * 3:1 ratio		−4.112		−3.393		−3.637		−0.768
		(3.815)		(8.295)		(4.207)		(2.471)
Treatment * unstated maximum		omitted		omitted		omitted		omitted
Treatment * $25,000 maximum		3.711		−12.250		8.181*		−1.268
		(4.385)		(10.464)		(4.695)		(2.841)
Treatment * $50,000 maximum		−6.160		−15.497		−5.443		−2.375
		(4.342)		(10.269)		(4.675)		(2.813)
Treatment * $100,000 maximum		−4.868		−22.391**		−0.946		−4.680
		(4.357)		(10.054)		(4.747)		(2.823)
Treatment * low example gift		omitted		omitted		omitted		omitted
Treatment * medium example gift		2.631		4.429		1.392		1.539
		(3.822)		(8.446)		(4.198)		(2.476)
Treatment * high example gift		0.284		−12.605		5.000		−0.863
		(3.789)		(8.722)		(4.103)		(2.455)
Constant	45.540***	45.540***	49.200***	49.200***	44.309***	44.309***	−1.806	−1.806
	(2.423)	(2.422)	(5.623)	(5.596)	(2.605)	(2.599)	(1.568)	(1.570)
R-squared	0.000	0.008	0.000	0.035	0.001	0.015	0.002	0.009
Observations	1034	1034	280	280	754	754	1034	1034

Note: Standard errors in parentheses.
*** Significant at, or below, 1 percent.
** Significant at, or below, 5 percent.
* Significant at, or below, 10 percent.

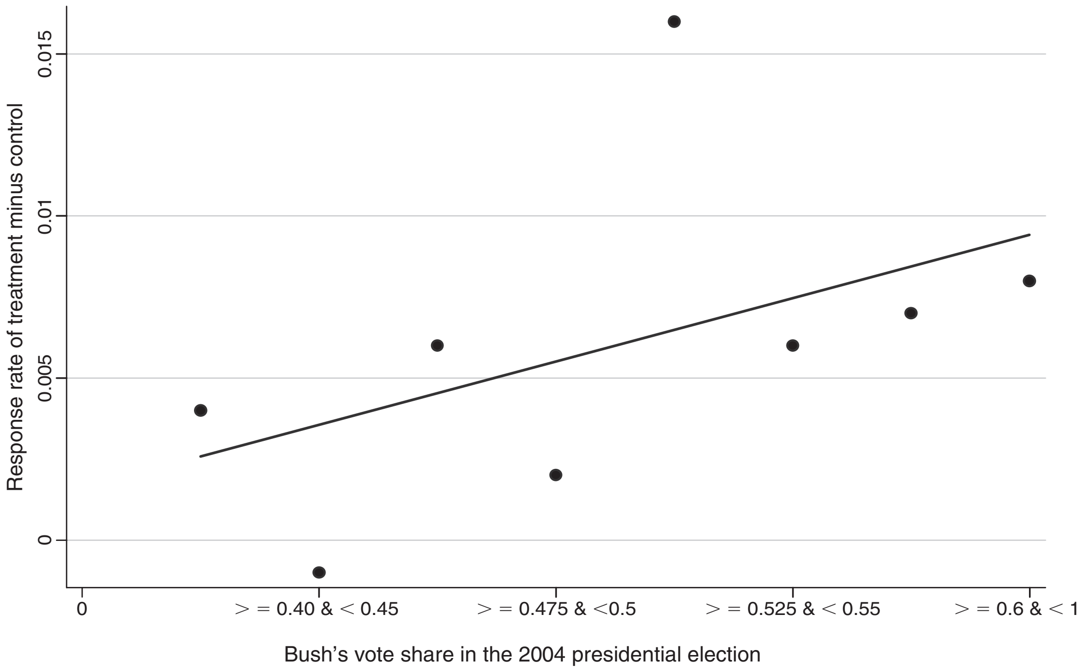

FIGURE 2

These results, coupled with those discussed above, lend insights into the broader applicability of the elasticities reported in the supply-side literature. Even though some studies have found negative and highly price-elastic measures, many researchers have presented estimates that strongly challenge the view that tax incentives are a useful stimulus to giving (Peloza and Steel 2005). The reported results have compelled some leading scholars to argue that the overall evidence on the price effect is decidedly mixed (see, e.g., Steinberg 1990; Auten, Sieg, and Clotfelter 2002). We view our results as providing some confidence in the estimates in the most recent literature, but they also serve to highlight that wide context-specific variation exists, based not only on demographics (e.g., income) but also on the timing and quality signal value of a leadership gift. This insight is consonant with the spirit of recent work identifying the importance of the properties of the situation when interpreting data from lab experiments (see, e.g., List 2006; Steven D. Levitt and List 2007).

in a minority group have a stronger sense of *social* identity, and hence perhaps the peer nature (a social cue) of the matching grant acted as a catalyst to trigger the salience of this identity. This theory suggests that the "signal" generated by the leadership gift is effective as either a quality or timing signal, and that those in the minority political group are more responsive to such signals.

III. Conclusions

The "supply side" of the economics of charity typically utilizes a model of charitable giving that treats donations no differently from any other consumer purchase. In this view, changes in tax deductibility emulate a change in the price of donating. This study pushes this literature in a new direction by focusing on the price effects on the "demand side" of the economics of charity. In particular, we explore large price deviations by liberally changing the match rate in an actual charitable fundraising field experiment that targeted over 50,000 donors.

Several insights emerge. First, we find that using leadership gifts as a matching offer considerably increases both the revenue per solicitation and the probability that an individual donates. This finding supports the anecdotal evidence among fundraising consultants on the

Table 5—Heterogenous Treatment Effects by Political Environment
(Dependent variable = donated (binary))
Probit

Panel A: Subsamples by Bush vote share	Each column restricts the sample frame to respondents in states with the specified Bush vote shares							
	≤ 40%	> 40% & ≤ 45%	> 45% & ≤ 47.5%	> 47.5% & ≤ 50%	> 50% & ≤ 52.5%	> 52.5% & ≤ 55%	> 55% & ≤ 60%	> 60%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.004	−0.001	0.006	0.002	0.016***	0.006*	0.007	0.008**
	(0.005)	(0.002)	(0.005)	(0.004)	(0.006)	(0.003)	(0.005)	(0.004)
Constant	0.015***	0.021***	0.021***	0.019***	0.017***	0.015***	0.014***	0.012***
	(0.004)	(0.002)	(0.004)	(0.003)	(0.005)	(0.003)	(0.004)	(0.003)
Pseudo r-squared	0.000	0.000	0.000	0.000	0.002	0.000	0.001	0.001
Observations	2,522	18,176	3,789	5,319	3,975	7,061	3,903	5,303

Panel B: Analysis by county type	Red county in a red state	Blue county in a red state	Red county in a blue state	Blue county in a blue state
	(1)	(2)	(3)	(4)
Treatment	0.010***	0.007**	0.000	0.001
	(0.002)	(0.003)	(0.003)	(0.002)
Pseudo r-squared	0.005	0.003	0.000	0.000
Observations	13,675	6,553	11,826	17,872

Panel C: Analysis by activity of the organization	Full sample	Full sample	Full sample
	(1)	(2)	(3)
Treatment	0.001	0.003	0.003
	(0.002)	(0.003)	(0.003)
Red state	−0.006***	−0.006***	−0.006**
	(0.002)	(0.002)	(0.003)
Treatment * red state	0.009***	0.008***	0.008**
	(0.003)	(0.004)	(0.004)
Nonlitigation	0.000		0.000
	(0.001)		(0.001)
Treatment * Nonlitigation	0.000		0.000
	(0.001)		(0.001)
Court cases		0.000	0.000
		(0.001)	(0.001)
Treatment * court cases		−0.001	−0.001
		(0.001)	(0.001)
Pseudo r-squared	0.002	0.002	0.002
Observations	49,631	49,631	49,631

Notes: "Nonlitigation" is the count of incidences relevant to this organization from each state reported in 2004–2005 (values range from zero to six) in the organization's monthly newsletter to donors. "Court cases" is the count of court cases from each state in 2004–2005 in which the organization was involved (values range from zero to four). Standard errors in parentheses.

*** Significant at, or below, 1 percent.
** Significant at, or below, 5 percent.
* Significant at, or below, 10 percent.

TABLE 6—CENSUS DEMOGRAPHIC ANALYSIS REGRESSIONS
(Dependent variable = donated (binary))
Probit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.002	(0.001)	0.000	(0.001)	0.010	0.005	0.000	0.005	(0.003)
	(0.002)	(0.007)	(0.002)	(0.005)	(0.008)	(0.004)	(0.005)	(0.003)	(0.027)
Red state	−0.006***	−0.006**	−0.006**	−0.006***	−0.006**	−0.006**	−0.006***	−0.006**	−0.005*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Treatment * red state	0.009***	0.010***	0.010***	0.010***	0.010***	0.009**	0.010***	0.009**	0.007*
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
First donation five or more years ago (binary)	0.004								
	(0.002)								
Treatment * first donation at least five years ago	0.000								
	(0.003)								
Highest previous amount donated	0.002								
	(0.002)								
Treatment * highest previous amount donated	−0.001								
	(0.002)								
Proportion white		−0.003							−0.013
		(0.007)							(0.015)
Treatment * proportion white		0.003							0.017
		(0.008)							(0.019)
Proportion black			−0.003						−0.014
			(0.009)						(0.015)
Treatment * proportion black			0.011						0.028
			(0.010)						(0.019)
Proportion age between 18 and 39 years				−0.026**					−0.036*
				(0.012)					(0.020)
Treatment * proportion age 18 to 39 years				0.005					0.029
				(0.014)					(0.024)
Average household size					0.006*				0.006
					(0.003)				(0.005)
Treatment * average household size					(0.004)				(0.010)
					(0.004)				(0.007)
Median household income Y						0.004			−0.007
						(0.005)			(0.012)
Treatment * median household income Y						−0.007			0.014
						(0.006)			(0.015)
Proportion house owner							0.007		−0.004
							(0.006)		(0.014)
Treatment * proportion house owner							0.002		0.010
							(0.007)		(0.017)
Proportion who finished college								(0.003)	0.010
								(0.006)	(0.014)
Treatment * proportion who finished college								(0.010)	−0.028*
								(0.008)	(0.017)
Proportion of population urban									0.001
									(0.005)
Treatment * proportion of population urban									−0.001
									(0.006)
Pseudo *r*-squared	0.005	0.002	0.002	0.003	0.003	0.002	0.003	0.003	0.005
Observations	50,036	48,251	48,081	48,251	48,255	48,243	48,248	48,249	48,073

Note: The results for urban omitted due to space, and are the same as the other covariates: treatment * red state remains equal to 0.010***.

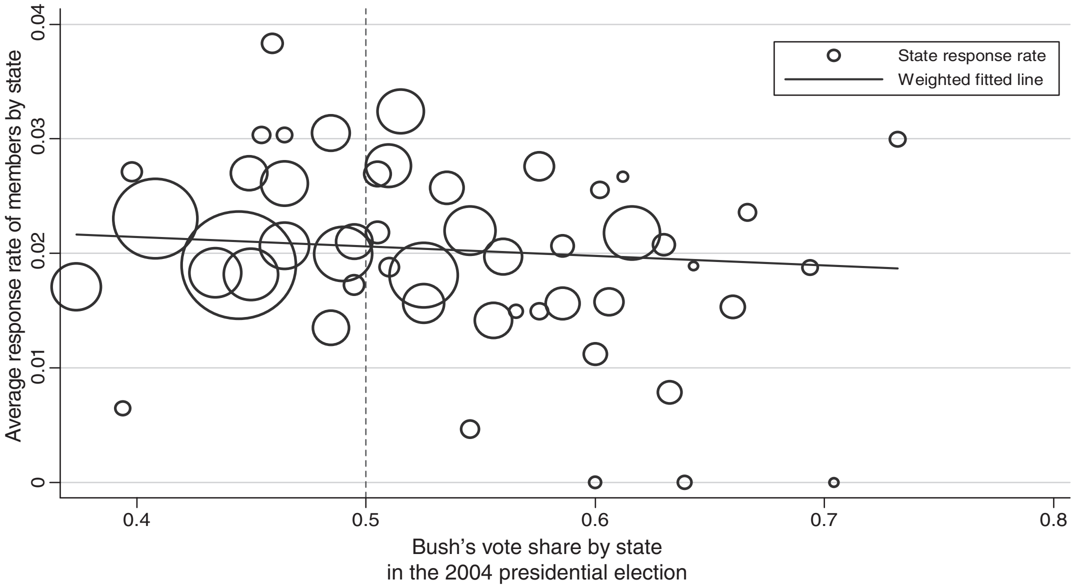

Figure 3

efficacy of a matching mechanism. Second, at odds with the conventional wisdom, we find that larger match ratios (i.e., \$3:\$1 and \$2:\$1) relative to smaller match ratios (\$1:\$1) have no additional impact. This result directly refutes the integrity of using larger match ratios, and stands in sharp contrast to current fundraising practices. In this light, with proper replication our results have practical import.

Our data also provide a test of an important method used in cost-benefit analysis. Cost-benefit analysis remains the hallmark of public policy decision making. Indeed, US President Clinton's Executive Order 12866, which reaffirmed the earlier executive order from the Reagan Administration, requires that federal agencies consider costs, benefits, and economic impacts of regulations prior to their implementation.[18] Estimation of benefits has been controversial, but the state-of-the-art method is a stated preference approach (e.g., contingent valuation) if the total economic value of a nonmarketed good or service is sought. This approach has been criticized for several reasons, but perhaps most importantly for its hypothetical nature and the fact that few contingent studies pass a formal "scope" test (see, e.g., Diamond and Hausman 1994; Kahneman and Knetsch 1992). To the best of our knowledge, our data represent a first attempt to explore the "scope" of a public good that is actually provided in a naturally occurring environment. In this regard, our data are consistent with the insensitivities observed in the CVM literature.

Finally, from a theoretical viewpoint, while extant theory provides insights into some of our results, the size and starkness of the differential response rate suggests that further theory would be useful. Future research in political psychology and social identity can help us better understand why the matching grant works in red, but not blue states.[19] Furthermore, in light

[18] The more than 100 federal agencies issue approximately 4,500 new rulemaking notices each year. About 25 percent of those 4,500 are significant enough to warrant Office of Management and Budget review. Of those, about 50 to 100 per year meet the necessary condition of being "economically significant" (more than \$100 million in *either* yearly benefits or costs). Every economically significant proposal receives a formal analysis of the benefits and costs by the agency.

[19] For example, our finding could be a political analog to the racial "acting white" phenomenon discussed in David Austen-Smith and Roland Fryer (forthcoming). This phenomenon (being socially sanctioned for performing well in school) occurs when blacks are in the minority in a school. Even if minority status makes one's political identity stron-

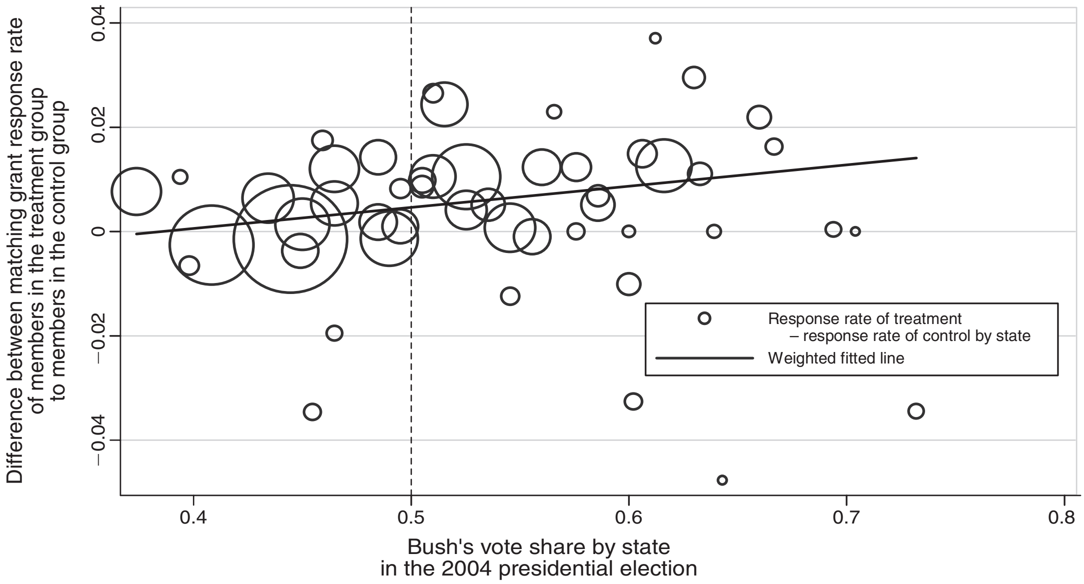

FIGURE 4

of the fact that Martin Feldstein (1975) shows that price elasticities vary among the types of charitable organizations, it is important to explore whether, and to what extent, our results on heterogeneous spatial treatment effects are robust to other charity types such as religious, educational, and environmental organizations. Perhaps the nature of an organization's activities influences whether donors contribute to gain "moral satisfaction" or to increase the provision of the public good. Testing matching grants with organizations that provide local public goods, or goods of smaller units (e.g., food for children in Africa), can further our understanding of whether it is important that the purchased good be tangible, and perhaps even generate a private gain, in order to observe sensitivity to all changes in price above $1:$1.

Overall, these results highlight the usefulness of field experimental research examining the relative strength of nonprice effects. The fact that responsiveness to a matching grant is partly determined by the political environment, rather than the economics of the matching grant itself, is important and consistent with recent work that reveals the relative importance of noneconomic factors in driving decision making in charitable giving (Craig Landry et al. 2006) and consumer credit (Marianne Bertrand et al. 2006). Manipulations that make salient the importance or effectiveness of a gift can generate further donations (Vesterlund 2003).[20] Clearly, further work is necessary to understand which signals generate such effects. Such work will inform both positive and normative issues in economics. Finally, such results will be useful for theorists and empiricists interested in

ger, this is not sufficient to generate our result (in fact, as is, that may argue that those in red states should give more on average than those in blue states, all else equal, but we find the opposite). To fit our setting, one must also argue that this identity is latent (perhaps out of frustration with their local political outcomes, perhaps because individuals have many latent identities and social and environmental triggers are needed to activate them; e.g., see Sen 2006), and then primed by the stimuli of the matching grant offer.

[20] Relatedly, it is interesting that price of giving matters in experimental dictator games (see, e.g., Andreoni and John Miller 2002, Eckel and Grossman 2003). From this perspective, considering the heterogeneous treatment effects that we find, it is possible that price matters if donations are raised, e.g., for a hurricane relief center, while price matters less to agents donating to a political organization. We trust that future research will address this issue.

obtaining deeper insights about the motivations behind the provision of public goods, as well as for nonprofits interested in improving their fundraising practices.

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