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Are Political and Charitable Giving Substitutes? Evidence from the United States
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ABSTRACT

Using micro data from the American Red Cross (ARC) and Federal Election Commission (FEC) in two natural experiments, we provide evidence that political and charitable giving are substitutes. In the first natural experiment, we estimate the effects of a positive shock to charitable donations to the ARC: foreign natural disaster events. We find that while charitable donations to ARC increase by 34.9% in the six weeks following a disaster, political donations decline by 18.8% in the same period. Put differently, each 1% increase in the charitable giving to ARC is accompanied by a 0.53% drop in political donations. At the average county-week level donations, the implied effect of a $1 increase in charitable giving is a $0.42 decline in political donations. In the second natural experiment, we estimate the effects of a positive shock to political giving: advertisements for political campaigns. Using a designated market area (DMA) boundary approach, we find that political advertisements increase political giving while they decrease charitable donations to ARC. Our estimates imply that each 1% increase in the political giving is accompanied by a 0.59% drop in charitable donations to ARC. At the average county-week level donations, the implied effect of a $1 increase in political giving is a $0.33 decline in charitable donations. The crowding out elasticities suggest that political and charitable giving are relatively close substitutes. We provide a number of robustness checks, and we discuss potential causal mechanisms.

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An online appendix is available at http://www.nber.org/data-appendix/w26616

1 Introduction

Philanthropy has increased threefold over the past four decades, with close to 73% of Americans making charitable donations annually (Jones, 2021; Askright.com, 2020). Individuals in the U.S. donated $450 billion to charity in 2019, an amount corresponding to 2.2% of GDP (Giving USA, 2018; Albrecht, 2020). Meanwhile, political giving has seen an unparalleled growth, with individual contributions to politics doubling in the 2020 election from the previous cycle (Federal Elections Commission, 2021). These two types of giving, political and charitable, have a lot in common. For example, the average donation can be significant relative to the household's budget, yet too small to achieve much on its own. Despite their similarities, research on charitable giving and political giving are treated as two separate topics. In this paper, we provide evidence that preferences for political and charitable giving are connected with each other.

To study the substitutability between political giving and charitable giving, we use data on charitable donations from the American Red Cross (ARC) and data on political contributions from the Federal Election Commission (FEC). For causal identification, we leverage two natural experiments. First, we study a positive shock to the demand for charitable giving. Under the hypothesis that political and charitable giving are substitutes, the increase in charitable giving should crowd out some of the political giving. In the second natural experiment, we study a positive shock to the demand for political giving. If political and charitable giving are substitutes, the increase in political giving should crowd out some of the charitable giving. Moreover, the degree of crowding out between the two types of giving can shed light on their degree of substituability.

In the first natural experiment, we measure how charitable donations to ARC and political donations to electoral candidates respond to foreign natural disasters. Foreign natural disasters arrive unexpectedly, and do not directly impact donors' means to give. They receive ample media coverage in the United States (Eisensee & Stromberg, 2007) and serve as reminders to donate to disaster relief, similar to reminders cited in the literature for financial payments (Cadena & Schoar, 2011; Karlan et al., 2012). We find that, in the six weeks following a foreign natural disaster, donations to the ARC increase by 34.9% (p-value<0.001). We observe a corresponding 18.8% decline (p-value<0.001) in political donations during this period. These two findings imply a crowding-out elasticity of 0.53 ($=\frac{18.8\%}{34.9\%}$, p-value <0.001): i.e., each 1% increase in charitable donations is associated with a 0.53%

drop in political donations. This crowding out number implies that, at the mean county-week political donation levels, a \$1 increase in disaster relief donations results in a \$0.42 decline in political donations.

In the second natural experiment, we take advantage of political advertising as a source of positive information shocks, increasing the salience of the need to donate to political candidates. We follow the identification strategy in Spenkuch & Toniatti (2018) and Shapiro (2018), and exploit geographic discontinuities in advertising markets to isolate the effect of political advertising. Specifically, using county-month level data and pairing each county to neighboring counties along Nielsen's designated market area (DMA) boundaries, we estimate the effect of political advertising on both types of donations. We estimate that a 10% increase in political ad spending leads to a 0.20% (p-value$<$0.05) increase in political giving, but also results in a 0.14% (p-value$<$0.05) decline in charitable donations. These two findings imply a crowding out elasticity of 0.59 ($= \frac{0.14\%}{0.20\%}$, p-value 0.066): i.e., each 1% increase in political giving results in a 0.59% drop in charitable donations. The implied effect at mean county-week donation is a \$0.33 decline in charitable donations in response to a \$1 increase in political donations.

Overall, these crowding out elasticities for donations (0.53 and 0.59) suggest that charitable and political giving are close substitutes, albeit far from perfect substitutes. These magnitudes are comparable to the crowding out elasticities reported in related contexts. For example, Reinstein (2007) uses an experiment to estimate how individuals substitute between different charities and finds a crowding out elasticity of 0.37: i.e., a 1% increase in donation to one charity results in a drop in donations to other charities by 0.37%. While these results are short-term elasticity estimates, they are economically significant because both charitable organizations and political candidates race against time. For humanitarian relief organizations, timely arrival of donations is instrumental to saving lives and protecting disaster victims (Van Wassenhove, 2006; Balcik & Beamon, 2008; Ergun et al., 2010) and for political candidates, early arrival of political donations matter since all financial resources must be put to use by a primary or an election deadline to secure a win.

We report a number of robustness checks throughout. To test the robustness of the substitutability pattern, we use several alternate data sets. While ARC offers in many ways an ideal setting to test the relationship between political and charitable donations, using data from a single organization raises questions about whether the observed substitution can be replicated using other charitable giving data.[1] To address this concern, we show that the results hold when using data on *all* charitable deductions reported to the Internal Revenue Service (IRS). We also show that the results hold when using proprietary data from the Catholic Relief Services (CRS), which is another large charitable organization focusing on humanitarian relief. We find that increasing political ad spending by 10% leads to a 0.14% decrease in aggregate charitable donations when using the IRS data and to a 0.17% decrease in charitable donations when using the CRS data, which are not only statistically significant but also remarkably similar in magnitude to the estimated effect of 0.14% documented with the ARC data.[2] Since the deductions reported to IRS are aggregated and reported annually, with this exercise we can also test for evidence of intertemporal substitution, that is, whether the substitution pattern observed in the short-term is fully offset by the donations arriving later. The results from the analysis with IRS data show that this is not the case, and that from the CRS data show some evidence of intertemporal substitution that is an order of magnitude smaller than the short-term substitution effect. These findings are consistent with what the charitable giving literature reports regarding intertemporal substitution: it is unlikely, or of second order importance (Falk, 2007; Maréchal & Thöni, 2019; Gee & Meer, 2019).

We see our study making three distinct contributions for practitioners, scholars, and policymakers. First, the substitution pattern between charitable and political giving informs managers of non-profits about the magnitudes by which charitable donations decline in response to political information shocks, and managers of political campaigns about the magnitudes by which political donations decline due to events that generate charitable donations. The substitution implies that a change in the regulations (e.g., tax breaks for charitable donations, caps on political donations, etc.) and promotional and information campaigns (e.g., promotional mails, advertising) that impact one type of giving are likely to impact the other type of giving, too. Crowding out of donations can have severe consequences,

[1] FEC and ARC data are ideal to test the relationship between political and charitable giving, because charitable giving to disaster relief and political contributions are roughly comparable in magnitudes: donations to disaster relief averaged $1.2 billion per year (Rooney, 2018), while individual contributions to political campaigns average $1.55 billion per year (Federal Elections Commission, 2017). Moreover, FEC provides comprehensive data on *all* political contributions in the US and ARC is the largest disaster relief organization based on the number of donations and revenue (Drucker, 1989; McCaslin et al., 2005) and is among the most recognizable charitable organizations overall (Smith & Grove, 2017; Armengol, 2014).

[2] In the online Appendix Section A.6, using a series of experiments, we test if these results can be replicated if the donations went to American Cancer Society or to Feeding America. We obtain qualitatively similar results to our baseline, with close magnitudes. In the experiments, for every additional dollar allocated to a charitable cause, donors reduced their political donations by $0.40–0.59, and for every additional dollar allocated to political giving, they reduced their charitable donations by $0.68-$0.80. These results mitigate the concern that the substitution observed with the ARC data is an artifact of the idiosyncratic organizational factors.

such as failing to provide timely help to disaster victims, or failing to enter or losing a political race. Such outcomes can generate further negative economic, social, and political outcomes (Petrova et al., 2021).

Second, our study informs scholars about the relationship between political and charitable donations. The relationship between the two types of giving has been of interest to scholars in business, management, economics, and political science. Yörük (2015) studies household surveys of donations between 1990 and 2001 and uses variations in income and itemized deductions in taxes across states to identify the relationship between charitable and political giving. Using imputed household-specific tax rate as an instrument for charitable giving, he finds that charitable donors are more likely to give money to politics too, and concludes that the two types of donations are complementary. The main challenge for these results is that taxes could be correlated with a host of unobservable household-specific factors, such as wealth and altruism. Our study contributes to the literature by advancing the causal identification in estimating the relationship between the two forms of giving, and reaches exactly the opposite conclusion: we show that charitable and political giving are substitutes, not complements. Thus, our paper represents a reversal of the earlier findings in the literature. A recent working paper by Cage & Guillot (2021) reaches the same conclusion as we do, confirming substitution between charitable and political giving, using a completely different methodology (reforms in charitable giving deductions) and administrative data from a different country (France). Our results complement these findings by employing the variation in donors' willingness to donate by directly measuring the degree of substitution using US micro data.[3] Finally, in a related paper, Bertrand et al. (2018) document that charitable giving can be used as a means of political influence. For example, grants to charitable organizations in a congressional district increase when that district's representative can influence relevant policies (e.g., by sitting on certain committees). In contrast to Bertrand et al. (2018), the focus of our paper is measurement of the degree of substitution between political and charitable donations.

Third, the substitution between the two types of giving offers scholars and policymakers a tool to

[3]While the literature on charitable giving emphasizes altruism (Andreoni, 1989; Ottoni-Wilhelm et al., 2017; Becker, 1974; Gee & Meer, 2019), warm glow (Andreoni, 1989), responding to peer pressure (DellaVigna et al., 2012; Andreoni et al., 2017), and earning bragging rights (Glazer & Konrad, 1996; Harbaugh, 1998; Montano-Campos & Perez-Truglia, 2019) as potential motivations for giving, there is no consensus on the drivers of political giving. Some studies attribute political donations to instrumental motives such as to influence the policies that benefit the donors the most (Snyder Jr, 1990; Grossman & Helpman, 1996; Mian et al., 2010; Bouton et al., 2018). Others claim that political donations are driven by a consumption motive (Ansolabehere et al., 2003). The substitution we document between the two types of donations raises questions for future research about whether there are shared motivations between the two types of giving.

assess the effects of either type of giving on other outcomes. Sources of exogenous variation for political giving can be used to study the causal effects of charitable giving on another outcome, and vice versa. In the online Appendix accompanying this paper, we provide a proof of concept demonstrating an example. Specifically, we estimate whether "more money in politics" (higher total political contributions) increases the vote share of the incumbent vs. challenger politicians in elections. We regress the vote share of politicians on total political donations, instrumented with foreign natural disasters as a source of exogenous variation. We find that, consistent with the arguments in Ansolabehere et al. (2003), a decline in total contributions improves the electoral prospects of the incumbents. This exercise is useful for scholars and informative for policymakers who are thinking about the effects of past and future regulations which restrict or promote political and charitable giving, such as caps on individual political contributions or tax incentives that increase the appeal of giving. The finding that lower total political donations can exacerbate the incumbency advantage in the U.S. is directly relevant to regulatory changes, such as the Citizens United ruling (Petrova et al., 2019), as well as the regulatory limits on campaign spending (Avis et al., 2017). In a similar fashion, political advertising can be used when thinking about the effects of charitable donations on other outcomes.[4]

2 Empirical Strategy and Results

2.1 Data

We describe the data used to test the relationship between political and charitable donations, starting with the data sets and variables used in the analysis. Tables 1–2 and Table A.1 in the online Appendix provide detailed summary statistics on all the variables used for the analysis.

Charitable Contributions to American Red Cross. We use proprietary data from the American Red Cross (ARC).[5] ARC is a humanitarian organization that provides emergency assistance, disaster relief, and disaster preparedness education in the U.S. The data consist of records of individ-

[4]However, as noted in Moshary et al. (2021) as well, for the variation in political advertising, the instruments could be weak, and one needs to use the appropriate methods and report weak instrument-robust confidence sets (see e.g., Chaudhuri & Zivot, 2011; Andrews, 2017; Sun, 2018; Andrews et al., 2019).

[5]Focusing on disaster relief as a form of charitable giving is ideal because, similarly acute, unexpected information shocks that are exogenous to donors' ability to give rarely exist for other charitable causes (e.g., curing cancer, fighting poverty, etc). We also do not anticipate religious charitable organizations to provide a good testing ground because they may draw donations from a small set of individuals who regularly donate as they attend, say, church services, rather than donating based on need. Moreover, ideally, the proprietary data should come from an organization with high name recognition and ARC is noted to be recognizable (Briones et al., 2011) and not close to a political party.

ual donations made to the organization, with donor information anonymized. For each donor, we have info on their zip code, the date and amount of donations, and any appeals or fundraising materials sent to them by the ARC. The data are available for 2006-2011. Since the ARC data are available for this period, in the rest of the paper, to make sure that we keep macroeconomic shocks, political situation, other important events comparable throughout the analysis, we run all analysis focusing on the period 2006-2011.[6]

Political Contributions. Political contributions data come from the Federal Elections Commission (FEC). The data comprise all individual level donations, and the name and addresses of the individuals are listed along with the date of the donation. We aggregate the data at the county level. The contributions are recorded and made public when an individual's contributions (over one or multiple giving occasions) exceed \$200. Regulations require all donations \$200 and above to be reported by political candidates, but donations below \$200 are reported on a voluntary basis and most remain unreported.[7]

Foreign Natural Disasters. Since domestic disasters may result in negative economic shocks and therefore influence donors' income and ability to donate, we focus on the information shocks associated with disasters that took place outside the United States. We collect data on those disasters using the International Disasters Database (EM-DAT).[8] We focus on large disasters resulting in 400 or more deaths, but also carry out robustness checks with other fatality thresholds. The natural disasters include earthquakes, floods, storms, and volcano eruptions. Throughout the paper, we provide controls for the tropical storms which originate abroad, but affect the US directly (hits of homeland) or indirectly (close call, Mexico, Cuba, Haiti, Dominican Republic, Puerto Rico, Bermuda). Figure 1-panel (a) presents a timeline for the instances of foreign disasters on a weekly basis.

Political Advertising. The data for political advertising are obtained from Wisconsin Advertising Project (for the years before 2010) and its successor Wesleyan Media Project (for 2010 and later years). We refer the reader to Fowler et al. (2015) for a detailed description of the data (as well as the basic

[6] As the donations are made in response to fundraising campaigns, we control for the intensity of ARC mailings. In addition, we do not replace missings with zeroes if no account in a county-week was a target for ARC fundraising campaign in the past three months.

[7] As all donations of \$200 and above should be reported to FEC, we replace missings with zeroes to make a balanced panel. In the online appendix Table A.17, we check if our results are robust to using donations of various dollar amounts, including below \$50, \$50-\$200, \$200-\$1000, \$1000-\$3000, \$3000-\$5000, above \$5000.

[8] According to the site, the database includes all disasters starting from the year 1900 until present, conforming to at least one of the following criteria: 10 or more people dead; 100 or more people affected; the declaration of a state of emergency; or a call for international assistance.

descriptive statistics). The source of ad data is Kantar Media/CMAG, which is a commercial firm specializing in serving corporate and political clients. These data represent the most comprehensive data collection on the content and targeting of political advertisements, and include information on the date, time, market, and cost of each ad that aired. The Wesleyan Media Project processes and codes the CMAG ad tracking data from all 210 media markets in the United States. The data are available for even (election) years between 2004 and 2012 except for the year 2006. To carry out the analysis during a period comparable to the period of the ARC data (i.e., 2006 to 2011), we use data from 2008 and 2010 election years.[9]

2.2 The Effects of Foreign Natural Disasters

We start our analysis by testing how charitable and political donations respond to foreign disaster information shocks. Foreign disasters arrive unexpectedly, receive media coverage in the U.S., and therefore act as reminders for the need to donate to disaster relief. More importantly, these disasters take place in other parts of the world, so they are unlikely to impact donors' financial means of giving directly. We use the following specification:

$$Y_{c,t} = \alpha_1 \cdot I_t^{+0/+6} + \alpha_2 \cdot I_t^{+7/+8} + \alpha_3 \cdot I_t^{-2/-1} + \mathbf{X_{c,t}}\beta + \varepsilon_{c,t}, \tag{1}$$

where the dependent variable $Y_{c,t}$ stands for either the total dollar contributions to ARC in county c and week t, or the corresponding total dollar contributions to political campaigns. These dependent variables can take the value of zero, so we use inverse hyperbolic sine (arcsinh) transformation (Burbidge et al., 1988a; Bellemare & Wichman, 2020).[10] The variable $I_t^{+0/+6}$ takes the value of 1 during the week of the disaster t and the following 6 weeks. Thus, α_1 captures the effect of a natural disaster on

[9] We also looked into the full Kantar Media database to identify ARC TV advertising and identified only 76 instances in all DMA-day-level markets.

[10] At the county level, the donation data are skewed, with many zeros. This is a common problem, and the classic solution to it is using some concave transformation of the respective variables, e.g., $log(1 + x)$ transformation. This solution could lead to biased inference due to the additive constant (N'Guessan et al., 2017). Inverse hyperbolic sine transformation, i.e., $arcsinh(x) = log(x + \sqrt{(}x^2 + 1))$ (Burbidge et al., 1988b) is a transformation which is defined for all values of x, including $x = 0$. Moreover, it takes a value close to $log(2x) = log(2) + log(x)$ for most x values. This transformation has been well adopted in modern applied microeconometrics (e.g. Card et al., 2020; Depetris-Chauvin et al., 2020). Bellemare & Wichman (2020) notes that for models with arcsinh–arcsinh transformed variables, the coefficient of the independent variable ($\hat{\beta}$) should be interpreted as elasticity. For models with arcsinh–dummy transformed variables, the percentage change in the LHS variable due to the change in the RHS variable can be approximated by $(exp(\hat{\beta}) - 1)$. Our elasticity derivations follow these guidelines by Bellemare & Wichman (2020). For robustness, we also report all main results using $log(1 + x)$ transformation of the key variables in the online Appendix Tables A.5 and A.6.

giving. We use a window of 6 weeks after the week of the disaster because of the abundant anecdotal evidence that the effects of disasters on donations are concentrated in that time period. For example, Schwab (2010) claims that "disaster donations are typically (...) made within the six weeks following a disaster." And Rooney (2018) argues that "most Americans who donate to support disaster relief (...) make these donations within six weeks of a big disaster." This anecdotal evidence is also consistent with the findings from Eisensee & Stromberg (2007), who show that news media keep reporting about major foreign disasters during the first 40 days after the event.[11] For some specifications, we also include the binary variable $I_t^{+7/+8}$, which takes the value 1 during the seventh and eighth weeks after the disaster week. Thus, α_2 measures if there are any substantial effects beyond the initial 6-week post-disaster period. Lastly, $I_t^{-2/-1}$ takes the value 1 during the two weeks before the start of the disaster. The coefficient α_3 provides an event-study falsification test to check for the presence of other, possibly unobserved, events which may create a spike in the outcome variable in the period leading to the time of shock and tests whether disasters were unanticipated. If the timing of the disasters is truly exogenous, we should expect α_3 to be close to zero. $\mathbf{X_{c,t}}$ is a vector of controls: month-of-the-year dummies, year dummies, the time until the next election (to control for the fact that donations to politics are more likely to arrive closer to the election date), the number of mailings sent out by the ARC in the previous three months (for the charitable giving specification), and county fixed effects.[12] We double cluster standard errors by state (to account for the potential spatial correlation) and week (to account for the fact that disaster shocks are common for the whole country).

The results of estimation of equation (1) are presented in Table 3. Based on Bellemare & Wichman (2020), the coefficient on $I_t^{+0/+6}$ from column (1) suggests that the charitable donations to ARC increase by approximately 34.9% during the 6 weeks after a disaster. This effect is statistically significant at 1% level. In column (2), we add the variable $I_t^{-2/-1}$ for the event-study falsification test. The coefficient on $I_t^{+0/+6}$ remains similar in magnitude and statistical significance. On the contrary, the coefficient on $I_t^{-2/-1}$ is closer to zero and not statistically significant. This evidence supports the premise that the timing of the disasters is indeed good as random. Column (3) also includes the variable $I_t^{+7/+8}$. The coefficient $I_t^{+7/+8}$ is not statistically different from the coefficient $I_t^{+0/+6}$, but

[11] In the online Appendix Table A.7, we show that the results are robust to slightly longer or shorter post-disaster window definitions.

[12] The variation in the key variable of interest ($I_t^{+0/+6}$) comes at week level, and there is no cross-sectional variation in this variable. Thus we cannot use week fixed effects as they would be perfectly collinear.

the point estimates are about a third smaller and the coefficient is not significant.[13]

Columns (4)–(6) of the table show that foreign natural disasters have a negative and significant effect on political contributions. The coefficient on $I_t^{+0/+6}$ from column (4) indicates that, in the six weeks after a disaster hits, the average decline in political giving is 18.8%. The coefficients on $I_t^{-2/-1}$ from columns (5)–(6) indicate that the shocks are unanticipated: these coefficients are closer to zero and statistically insignificant. Overall, the results in columns (4)–(6) show that natural disasters negatively affect political donations, suggesting that charitable donations crowd out political donations. In sum, Table 3 indicates that the foreign natural disasters increased the ARC donations, but at the same time decreased political giving. We can combine the estimates to quantify the degree of crowd out. The estimates imply that charitable donations crowd out political donations by a factor of 0.53 ($= \frac{18.8\%}{34.9\%}$). We estimate the p-value (<0.001) and the confidence intervals of this elasticity using a seemingly unrelated regression approach.[14] At the bottom of the table, we also report the results from two F-tests that check if the event shocks are unanticipated (if α_2 and α_1 are statistically similar) and if the short term substitution pattern reverses (if there is evidence of intertemporal substitution) in the long term (if -α_3 and α_2 are statistically similar). In all specifications, we fail to find evidence of either. We report similar tests in other tables as well.[15]

Our findings are also robust to a series of tests and changes to the baseline specification (please see the online Appendix), including using different time windows after a disaster hits (Table A.7 and A.8), excluding disaster controls (Table A.10) or ARC's solicitation as mailing controls (Tables A.11), using alternative fatality thresholds for the definition of large natural disasters (Table A.13), and controlling for odd-year and even-year month fixed effects (Table A.14). Political advertising by candidates does not respond to foreign natural disasters (Table A.16), suggesting that the findings are not driven by a change in political advertising strategy following large foreign natural disasters. Substitution pattern

[13] We include a separate control for tropical storms close to the U.S., which could affect the country directly. Our results are robust to the exclusion of these controls (see online Appendix Table A.10) or to repeating the analysis using only observations without zero donations (results can be obtained from the authors).

[14] Specifically, we use a delta method for nonlinear combination of parameters, following seemingly unrelated regressions estimation and its nlcom implementation in STATA.

[15] In Figure A.1, we report the results for different amounts: below \$50, \$50-\$200, \$200-\$1,000, \$1000-\$3,000, \$3,000-\$5,000, and above \$5,000. We find the strongest results for donations above \$200 and below \$3,000. We expect the results to be stronger for smaller donations, as they are closer to a typical individual political donation. However, per FEC guidelines, political candidates are required to itemize only the political contributions which are \$200 and over. Contributions less than \$200 are only voluntarily reported. As a result, the data for donations under \$200 are often missing and the results for these donations should be interpreted with caution. With all the caveats above, our coefficients are negative and significant at 10% level for the reported donations in the \$50-\$1,000 range. Moreover, the estimated coefficients are negative for donations greater than \$1,000. We report the coefficients for the different donation amounts in Table A.17 of the online Appendix.

is still statistically significant if we break the donations down by political party (Tables A.18 and A.19).[16]

2.3 The Effects of Political Advertising

Next, taking advantage of a second natural experiment, we test how charitable donations respond to political information shocks. Ideally, to estimate the elasticity of giving with respect to political information shocks, we would like to conduct an experiment where we expose some randomly selected group of individuals (treatment group) to political information, but not the rest of the group (control group). We would then compare the lift in the donations of the treated group to that of the control group. While running such an experiment at the national level is not feasible, political advertisements across DMAs resemble such an experiment where individuals in one DMA are exposed to political information (ads) of the same type and quantity, while others in another DMA may not be.

Unlike natural disasters, spending on political advertising and consequent political information shocks are endogenous. A host of correlated unobservables determine both a candidate's advertising spending and political contributions in any geographic area. An OLS approach, even after including time and location fixed effects, is unlikely to yield unbiased estimates as counties are more likely to be exposed to political ads during specific stages of political campaigns, timing of which may coincide with a candidate's visit, news reports, volunteer field efforts, etc. Thus, unobserved factors correlated with the error term may, in part, explain our results.[17] An alternative approach is using cross-sectional variation, comparing similar counties at the same stage of the campaign.

To address this endogeneity issue, we follow the papers that take advantage of border discontinuity approaches in general (e.g., Card & Krueger, 1994; Spenkuch & Toniatti, 2018; Shapiro, 2018), and use DMA borders, in particular. A significant portion of the political ads are purchased at DMA level, and households within a DMA are exposed to similar TV content and ads. DMA maps do not necessarily overlap with administrative and political maps, as they were determined according to

[16] A notable difference between Table A.7 and Table 3 is that, in A.7, the point estimates for weeks 6-8 (column 3) or week 8 only (column 6) for charitable contributions are statistically significant, in contrast to the coefficient for the weeks 7-8 in columns 3 and 6 of Table 3. This suggests that charitable donation response to natural disasters likely extends beyond the [0, +6] week window. In contrast, the point estimates for the weeks 6-8 and week 8 for political contributions (columns 3 and 6 of Table A.8) are at least two times smaller than that in column (6) of Table 3, which suggests that political donation response to natural disasters is likely more short-lived.

[17] At the same time, holding everything constant, there does not seem to be a sharp increase in the volume of political ads at a particular time or in a particular geography that we could use for identification.

the television stations consumers of cable or satellite dish have access to.[18] For illustrative purposes, Figure 1-b presents a map with the DMA and the county boundaries. The cross-sectional variation due to discontinuous DMA borders allows individuals across these boundaries to be exposed to different levels of political ads, resulting in a quasi-random source of variation in political advertising shocks. Put differently, two similar counties along a common DMA border would be exposed to similar levels of political advertising had the DMA boundary not fallen between the two.[19] Note that, for political donations, we impose the additional requirement that the compared counties belong to the same congressional district to avoid differences in outcomes being driven by different characteristics of the electoral races faced in different districts.

To account for the possibility of delayed advertising effects, in what follows, our key independent variable covers two months of political ads preceding political donations. Specifically, we regress the outcome variable $Y_{c,t}$ representing the total contributions in county c in month t on the dollar value of advertising spending in county c for period t, employing arcsinh transformation:

$$Y_{c,t} = \gamma_{p,bt} + \eta_t + \alpha_1 A_{c,t}^{-1/+0} + \alpha_2 A_{c,t}^{+1} + \varepsilon_{c,t} \tag{2}$$

Here, $A_{c,t}^{-1/+0}$ represents the total political advertising spending in county c in the previous month, $t-1$, and current month, t. As in equation (1), we include the falsification term $A_{c,t}^{+1}$, which accounts for future advertising spending in county c and in month $t+1$.[20]

We focus on monthly as opposed to weekly data for two reasons. First, and more importantly, campaign ads have a high degree of auto-correlation in weekly data. Thus, we cannot argue that political ads constitute abrupt information shocks with weekly data as we did for natural disasters. Second, while some papers suggest that campaign advertising effects are short-lived (Gerber et al., 2011), others argue they can last as long as six weeks (Hill et al., 2013), with Urban & Niebler (2014), similar to us, using monthly frequency. Note also that we include county pair fixed effects rather than county fixed effects. This is because we study a fairly short time period (2006-2011), and the political

[18]See Shapiro (2018) and Spenkuch & Toniatti (2018) for details on the historical development of DMAs.

[19]DMA boundaries control for the content and type of ads, which are not observable to the researcher, in a way that the state boundaries cannot. Because advertising is purchased at DMA level, and some DMAs span across state boundaries, comparing counties across state boundaries does not allow for comparing different levels of political information shocks.

[20]Since there is considerable variation in the prices of advertising between TV channels, day time vs. prime time advertisement options, and between TV shows within a given channel, we use dollar spending (as opposed to the number of ads aired or gross rating points) to better approximate the number of individuals which are exposed to the information shock. These variables show a strong positive correlation.

advertising data in this period are observed for one midterm election year (2010) and one presidential election year (2008). 2006 data are not made accessible by the data source. Thus, we do not have enough power to estimate the county fixed effects since for each county and election we have a cross section of observations.

Thus, to take into account the possibility of a delayed response, our key variable of interest combines ad spending from the previous and current months (a time frame of 8 weeks), which makes the windows of analysis comparable to the 6-8 week time frame used in specification (1) with natural disasters. The other controls are bi-monthly county pair p fixed effect for months $t-1$ and t ($\gamma_{p,bt}$) to compare the ad spending in the county pair at the same time period, and month fixed effect (η_t) to account more precisely for the state of a political campaign. Following Spenkuch & Toniatti (2018) and Shapiro (2018), every county is included as many times as it enters county pairs for the pairs sharing a DMA border. We double cluster standard errors by DMA and bi-monthly time period accordingly. In the sample, we retain only counties with DMA borders and match each county to any other neighboring county in the same state with which it shares a DMA border. Moreover, for the specification using political donations, we require that border county pairs are within the same congressional district to control for election and campaign timing and differences between candidates.[21] Our identifying assumption is that the differences in the advertising spending between a county pair are uncorrelated with the differences between the time-varying unobservables in these counties. Similar to the natural disasters analysis, we transform both the donation and the political advertising variables using arcsinh transformation. For our baseline specification (Table 4), we provide estimates for the aggregate period (2006-2011) over which the ARC data are available, which makes donor responses to charitable and political information shocks more comparable. Similar to our analysis with natural disasters, to make sure that our results are not driven by the intensity of ARC fundraising campaigns, we control for the charitable appeal mailing activity of ARC. However, our results are very similar in the absence of this control (see Table A.12 in the online Appendix). In all specifications, we control for the weeks to election date to account for the intensity of political campaigning.

The results from the estimation of equation (2) are presented in Table 4 and indicate that, in response to political information shocks, political contributions increase and charitable contributions

[21] Specifically, we identify all counties in the U.S. which share a physical border, but belong to different DMAs. For the analysis of political donations, we exclude county pairs that belong to different congressional districts, since differences between congressional districts contribute to the differences in political donation levels. After these steps, we identify 2123 (1522 in the political donations sample) distinct cross border county pairs.

decline. Columns (1) and (2) show that political ads lift political donations and the estimates for coefficient α_1 range from 0.020 to 0.022 (significant at 5% level). Put differently, if political advertising spending in a county goes up by 10%, holding everything else constant, the political donations in this county go up by 0.20-0.22%. This estimate is consistent with the relatively low persuasion rates reported in studies on political advertising (e.g., Spenkuch & Toniatti, 2018), who report that political advertising increases vote shares by less than 1%. The ads aired in the future do not affect current political (column (2)) and charitable (column (4)) donations, supporting our identifying assumption. Next, we also observe that political ads negatively affect charitable donations (columns (3) and (4)), with the magnitude ranging from -0.014 to -0.013 at the statistical significance of 5% and 10%, respectively. The magnitudes imply that a 10% increase in the political ad spending leads to a 0.13-0.14% decline in charitable giving. Based on these estimates, the crowding out elasticity between political and charitable donations is 0.59 ($= \frac{1.3\%}{2.2\%}$): i.e., for each 1% increase in political donations, there is a drop of 0.59% in ARC donations. This crowding out elasticity is remarkably close to the corresponding elasticity we estimated using the disaster shocks (0.53, from Section 2.2).

In the online Appendix of the paper, we test and find that these results are robust to small changes in the estimation window size (Table A.9). Table A.15 in the online Appendix tests if ARC reduced the number of solicitations sent to donors anticipating that increased political advertising will steal some donor's attention away, and does not find support for this explanation in the data.

At the bottom of Table 4, we report a test checking if the coefficients of the ad spending from the previous and current month (α_1) and future month (α_2) are statistically different from each other. If these coefficients are similar, this can rule out the possibility of intertemporal reversal in donations. We find that, for political donations, the coefficient for future ad spending is positive and significantly smaller than the coefficient of the past and current month ad spending (column (2)). For charitable (ARC) donations (column (4)), we find that the coefficients of the main effect and future dollar spending are statistically similar, and have the same sign, again suggesting no reversal of the effect in the near term.

2.4 Magnitudes

Our estimates are informative about the percentage changes in donations of one type in response to the information shocks of own and other donation types. While the elasticities calculated earlier are informative about the relative percentage changes in donations, they should be interpreted carefully, keeping the base levels in mind. To calculate the magnitude of the impact, we fix the values of charitable/political giving and make back-of-the-envelope calculations at the mean values of county-week level donations.

The implied total donations to disaster relief during the data period can be calculated as follows. Total individual donations to disaster relief in 2018 in the U.S. was approximately $3 bln (The Non-Profit Times, 2020). Assuming a constant rate of growth for disaster relief during 2006-2018, and adjusting for time trends, the average predicted disaster relief would be $2.07 bln for 2006-2011.[22] The average annual donations to ARC in our sample is approximately $8 million per year. The average donation to ARC per county-week in our sample is $65. Thus the implied overall disaster relief donations per county-week approximately equals to $65/$8 million * $2.07 bln = $16,801. Since our data capture all reported political donations, the average political donation per county-week can be calculated in a straightforward fashion and equals $13,451.

To compute the effect of a marginal dollar at these mean values, imagine that the average county-week disaster relief donations increase from $16,801 to $16,802. The difference between the arcsinh transformations of these variables is $0.258 * 10^{-4}(= log(16,802 + (16,802 + 1)^{1/2}) - log(16,801 + (16,801^2 + 1)^{1/2}))$. The implied effect, from our crowding out elasticity estimate 0.53, is a decline by $0.137 * 10^{-4}(= 0.53 * 0.258 * 10^{-4})$ in transformed values. The corresponding change for the political donations would be a decline from $13,451 to $13,450.58. Put differently, a $1 increase in disaster relief donations results in $0.42 decline in political donations at the week-county means.

Similarly, the effect of political advertising-driven crowding out of charitable donations can be drived as follows. If the average county-week donations increase by $1 (from $13,451 to $13,452), this implies a growth of arcsinh transformed variables by $0.322 * 10^{-4}$. The implied effect for the charitable donations from our elasticity estimate is a decline by $0.19 * 10^{-4}(= 0.322 * 10^{-4} * 0.59)$. The corresponding value for the total disaster relief would be $16,800.67. Thus, we conclude that a $1 increase in political donations leads to approximately a $0.33 decline in charitable donations at the mean values of county-week donations.

[22] Unfortunately, we could not find the raw data or a reported amount for the disaster relief donations for 2006-2011. To estimate this number, we use the data on total giving trends from Giving USA and Chronicles of Philanthropy. According to these sources, the total charitable giving for 2018 was $427 bln, while the average charitable giving for 2006-2011 was $294 bln. Using a proportion rule and the $3bln total disaster relief for 2018 reported by the NonProfit Times, the predicted average disaster relief expenses in 2006-2011 is $2.07 bln.

2.5 Political Ads and Charitable Tax Deductions

Our findings suggest that charitable and political donations resemble close substitutes within a time frame of up to two months. It is not clear if the pattern of substitution can be generalized to donations to other charitable organizations, and if the pattern would hold over a longer time frame. In particular, one may be concerned about intertemporal substitution such that the decline in the short term period can be offset by more donations in the long run. In this subsection, we address these two questions using data on annual charitable deductions in tax returns reported by the Internal Revenue Service (IRS). IRS data include aggregate annual donations to all charities on a select set of income and tax items, classified by zip code and adjusted gross income bracket.[23] With these data, we can study the effect of political ads on aggregate charitable donations reported by the IRS, and test if the substitution replicates.

Table 5 shows the estimates from a variation of specification 2, collapsing data at two-yearly congressional cycle level.[24] As before, every county is included as many times as it enters the county pairs, and we cluster standard errors by DMA-election cycle. We include county pair, congressional cycle (time), and income group fixed effects. The results show that the total aggregate donations decline with higher levels of political advertising spending in a county. The numerical coefficient is -0.014, significant at the 5% level, implying that a 10% increase in political ad spending leads to a 0.14% decrease in charitable contributions in a county over the congressional cycle, a very comparable magnitude to those reported in Table 4.

These findings suggest that the substitution pattern we document for ARC extends to aggregate donations made to all charitable organizations reported to IRS. Moreover, they suggest lack of evidence for intertemporal substitution: the negative effects of political information shocks on the charitable donations are not fully offset by a future increase in donations, even looking at an aggregate two-year data period. This finding is consistent with the findings from a number of studies in donation and gift-giving literature which test intertemporal substitution and report small and insignificant effects (e.g., Falk, 2007; Hungerman & Wilhelm, 2016; Meer, 2017) or find intertemporal substitution to be of second order importance (Maréchal & Thöni, 2019). At the same time, because any delay in the

[23] For more information on the data set, please visit https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi. Data are based on individual income tax returns filed with the IRS. We use data from 2006-2011, compatible with the timeline of the ARC data. Income brackets are: under \$1, \$1 to \$10,000, \$10,000 to \$25,000, \$25,000 to \$50,000, \$50,000 to \$75,000, \$75,000 to \$100,000, \$100,000 to \$200,000, and \$200,000 or more.

[24] Alternatively, in the online Appendix Table A.2, we collapse data at annual level and show very similar results.

arrival of both political and charitable donations can result in significant outcomes, such as delays in helping disaster victims or winning an election, lifts in short-term donations are economically and socially significant (e.g., Van Wassenhove, 2006; Balcik & Beamon, 2008; Ergun et al., 2010).

2.6 Replication with Data from Catholic Relief Services

We also replicate our key tables using proprietary data from the Catholic Relief Services (CRS). CRS is an international humanitarian agency of the Catholic community in the U.S., whose aim is to provide relief at times of disaster, civil conflict, and disease and poverty. While its aid efforts resemble that of ARC, CRS is different in its religious affiliation. Table A.3 in the online Appendix presents the results from running specification 1, similar to Table 3, using data from CRS. The coefficient on $I^{+0/+6}$ from column (1) suggests that the charitable donations to CRS increase by approximately 11.8% during the 6 weeks following a disaster, and this effect is significant at the 5% level. In column (2), we add the variable $I^{-2/-1}$ for the event-study falsification test and the coefficient on $I^{+0/+6}$ remains very similar in magnitude. The coefficient on $I^{-2/-1}$ is close to zero and is statistically insignificant, suggesting again that the timing of the disasters is indeed good as random. In column (3), the coefficient on the variable $I_t^{+7/+8}$ is -0.098: small, negative, but significant, indicating while charitable contributions are concentrated in the first six weeks, some intertemporal shift that is an order of magnitude smaller compared to the increase in the first six weeks may be possible. Overall, the results from the CRS data are largely consistent with the results from the ARC data.

We also relate CRS data to political ads (Table A.4). Our results are consistent with the results reported in columns (3) and (4) of Table 4, as political ads seem to significantly decrease donations to CRS. Point estimate (column (2)) is 0.017, remarkably similar to the numbers with ARC (0.013) and IRS (0.014) data. Overall, the results in Tables A.3 and A.4 suggest that our main results are likely to be generalizable to other disaster relief charities. Yet, we interpret these results with caution, since in contrast to ARC and IRS, we know less about the data generating process for CRS donations and because the data are more patchy in comparison to the other two.

2.7 Discussion on LATE

Since foreign natural disasters arrive unexpectedly and the DMA border strategy allows us to estimate the causal effect of ads, we expect our estimates to be unbiased. Nonetheless, the information shocks we study impact only the individuals who become exposed to them and select into donating. So the crowding out estimates we calculate correspond to a local average treatment effect (LATE) that gives more weight to the individuals who react more strongly to the information shocks that we study, and may not be identical to the average treatment effects (ATE).

While the LATE may differ from the ATE in magnitude, it is still informative. First, the crowding out elasticities we derive—the change in charitable giving in response to a change in political giving (and vice versa)—are obtained from the same "complier" sample in each natural experiment. Second, to assess whether the ATE is far from the LATE, one approach in empirical research is to estimate the same parameter using different natural experiments. Estimated parameters varying largely across different experiments is indicative that one or more estimates differ from the ATE. The crowding out estimates from the first and the second natural experiments of our study, as well as those from the replications, are remarkably similar in magnitude.[25] Thus, while not conclusive, this partially alleviates the concern that the LATE we estimate differs largely from the ATE.

3 Discussion and Implications

In this paper, based on the findings from two natural experiments, we argue that political and charitable donations are closer substitutes than previously thought (Ansolabehere et al., 2003). In particular, a 1% marginal increase in charitable donations leads to a 0.53% decline in political donations and a 1% marginal increase in political donations crowds out charitable giving by 0.59%. Additional evidence from the IRS and the CRS data suggests that the observed substitution can be generalized to charitable donations and organizations beyond ARC. Therefore, our findings are in line with the earlier literature which states "political giving should be regarded as a form of consumption not unlike giving to charities, such as the United Way or public radio" (Ansolabehere et al., 2003, pg.118).

While we cannot explicitly test what drives the observed substitution between political and charitable giving, our results can first be explained by budget constraint considerations, as donating in one category leaves less room in one's budget to donate to another category. However, given that the political donations which we primarily focus on are above $200, and the average ARC donation is $60, there may be additional mechanisms." Another possible explanation consistent with our empirical

[25] We find the same order of magnitudes in auxiliary laboratory experiments (reported in the online Appendix Section A.6).

findings is warm glow, or the private benefits such as feel-good motivations that primarily benefit the self. The literature in economics and political science argues that civic engagement is partially motivated by private benefits such as warm glow (Niebler & Urban, 2017), and related benefits such as complying with social norms or avoiding social pressure (Gerber et al., 2016; DellaVigna et al., 2016). Diamond writes "if someone gets a warm glow from work on an election that results in an increase in some public good level, then such a warm glow seems to be on the same footing as that from a charitable donation" (Diamond, 2006, p.916). When an individual donates in order to obtain private benefits such as warm glow, the benefits to the victims matter less, thus the purpose of the donations matters less. As a result, charitable donations can be a substitute to political giving as a source of private benefits.

Our findings have implications for charitable organizations, political candidates, and researchers. For managers of charitable organizations, the findings indicate that events which drive political donations—e.g., political advertising, fundraising events, changes to caps for giving—may crowd out charitable giving in the short term, and delay or shrink the overall resources available for disaster relief, and disaster relief organizations need resources for timely response to help the victims (Balcik & Beamon, 2008; Van Wassenhove, 2006). Similarly, for political candidates, substitution implies that factors which drive charitable donations, e.g., arrival of disasters, may crowd out political donations. This crowding out can play an economically and politically significant role, even if it takes place in the short term, since politicians run campaigns following an election timeline.

For researchers, our findings are informative for two reasons. First, while motivations for donating to others is an area of joint interest to marketers, economists, and political scientists, few papers focused on investigating the relationship between charitable and political giving, with the exception of Yörük (2015), who found that charitable and political giving are complements. Our findings reverse those by Yörük (2015) and allow researchers to update beliefs regarding the relationship between the two types of giving.

Second, researchers can use the relationship between the two types of giving to identify their causal effects on other outcomes. In the online Appendix Section A.5, we provide a proof of concept for how the relationship between the two types of donations can be useful to researchers to estimate the causal implications of donations on other outcomes. More specifically, we investigate the impact of the overall pot of campaign donations on voting outcomes. Naturally, estimating the causal effect of political donations to politics on voting outcomes suffers from endogeneity issues. Unobservable factors can influence both the tendency to donate to candidates and to vote for them, and running an experiment which randomizes donating to candidates to resolve the endogeneity problem is challenging. The proof of concept we provide shows that political donations can be instrumented on foreign natural disasters, and instrumented donations can be used to estimate the causal effect of donations on electoral outcomes. In the exercise, we test how total political contributions impact the incumbent and challenger politicians' likelihood of being elected. The results in Table A.20 suggest that higher total political contributions hurt, rather than help, the electoral prospects of incumbent politicians. If the total political contributions increase by 10%, the vote share of challengers increases by 1.2 p.p. (column (1), coefficient significant at 1% level), and the probability that a challenger wins an election increases by 3.6 p.p. (column (2), significant at 10% level). This proof of concept demonstrates how our findings can become instrumental to other researchers who are interested in establishing the effects of donations on various managerial, political, and social outcomes, but have to tackle endogeneity problems to do so, and can ignite future research in these areas.

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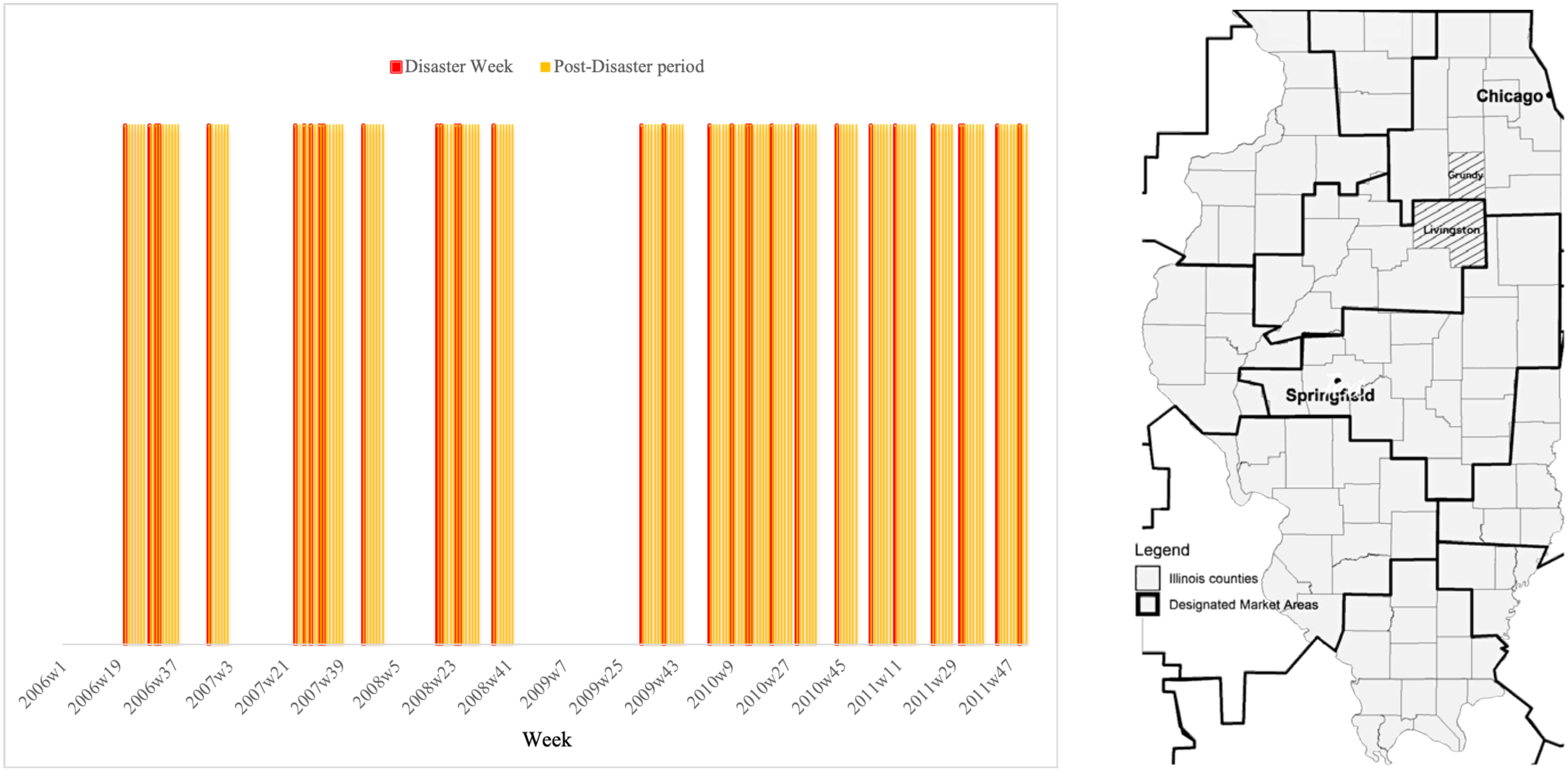
Figure 1: Institutional Context for the Analysis

a. Large Foreign Natural Disasters over Time

b. Counties and Media Markets in the State of Illinois



Notes: Panel (a) shows the weekly time series of large foreign natural disasters for years 2006 to 2011. The darker red bars indicate that there was a disaster in that week, while the lighter yellow bars indicate the post-disaster period. Panel (b) shows the boundaries for counties and designated media market areas in the state of Illinois. Shaded counties are two examples of counties along a DMA border in the same congressional district.

Table 1: Summary Statistics for Variables at the County-Week Level

	Sample	N	Mean	SD	Median	Min	Max	Range
$I^{+0/+6}$	All	740,343	0.50	0.50	0.00	0.00	1	2006-11
$I^{-2/-1}$	All	740,343	0.19	0.39	0.00	0.00	1	2006-11
$I^{+7/+8}$	All	740,343	0.17	0.38	0.00	0.00	1	2006-11
Aggregate ARC Donations, $, county-week	All	740,343	65.30	495.96	0.00	0.00	129352.8	2006-11
Aggregate ARC Donations, $, county-week	Nonzero	206,050	234.62	918.74	55.00	0.01	129352.8	2006-11
Aggregate ARC Donations count, county-week	All	740,343	1.10	6.65	0.00	0.00	1969.00	2006-11
Aggregate ARC Donations count, county-week	Nonzero	206,050	3.94	12.15	1.00	1.00	1969.00	2006-11
Aggregate FEC Donations, $, county-week	All	978,432	6405.37	56225.65	0.00	0.00	9154844.00	2006-11
Aggregate FEC Donations, $, county-week	Nonzero	465,509	13463.16	80929.77	1283.00	1.00	9154844.00	2006-11
Aggregate FEC Donations count, county-week	All	978,432	6.99	44.52	0.00	0.00	4402.00	2006-11
Aggregate FEC Donations count, county-week	Nonzero	465,509	14.69	63.65	2.00	1.00	4402.00	2006-11

Notes: All donation variables are computed with arcsinh transformation.

Table 2: Summary Statistics for Variables, County-Month Level

	Obs	Mean	Std. dev.	Min	Max
Aggregate FEC donations	14,798	8.8066	1.6101	0	15.0984
$A^{-1/+0}$, FEC sample	14,798	4.0910	5.2440	0	16.7204
A^{+1} , FEC sample	12,461	4.8605	6.2326	0	17.0098
Aggregate ARC donations	50,952	2.4080	2.8100	0	11.8055
$A^{-1/+0}$, ARC sample	50,952	3.6007	4.8358	0	16.6295
A^{+1}, ARC sample	50,952	3.5443	5.5526	0	16.8945
CRS donations	21,036	2.48222	3.1816	0	14.9767

Notes: Unit of observations is county-month. All donation and expense variables are reported in arcsinh transformation. The data on Congressional political advertising from Wisconsin Advertising Project is only available for 2008 and 2010 electoral cycles.

Table 3: Disaster Information Shocks & Charitable and Political Contributions

	Charitable Contributions			Political Contributions		
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{+0/+6}$	0.300***	0.288***	0.315***	-0.174***	-0.174***	-0.186***
	(0.0880)	(0.0922)	(0.0924)	(0.0589)	(0.0599)	(0.0604)
$I^{-2/-1}$		-0.0832	-0.0627		-8.14e-05	-0.00737
		(0.127)	(0.126)		(0.0651)	(0.0649)
$I^{+7/+8}$			0.197			-0.0756
			(0.118)			(0.0686)
Observations	740,280	740,280	740,280	978,432	978,432	978,432
R-squared	0.474	0.474	0.475	0.580	0.580	0.580
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Mailing Controls	Yes	Yes	Yes	No	No	No
Disaster Controls	Yes	Yes	Yes	Yes	Yes	Yes
Disaster Fatality Threshold	400	400	400	400	400	400
F-value H: $I^{+0/+6} = I^{-2/-1}$		8.099	8.461		4.742	4.942
p-value		0.00640	0.00540		0.0302	0.0269
F-value H: $I^{+0/+6} = - I^{+7/+8}$			10.03			6.846
p-value			0.00262			0.00932

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. The dependent variable is the county-week level aggregate charitable donations to ARC or political donations from FEC, transformed with arcsinh transformation. $I^{+0/+6}$ is a dummy which equals 1 for the week of disaster and the 6 weeks after that, $I^{+7/+8}$ is a dummy which equals 1 for the weeks 7 and 8 after the disaster to allow for delayed effects, and $I^{-2/-1}$ is a dummy which equals 1 for the two weeks preceding the disaster. Controls include the logged number of mailings sent by ARC in the 3 months preceding a donation and only apply to columns (1)-(3). We include county, year, month, and weeks-to-election fixed effects, as well as controls for the tropical storms that originate outside of the US, but affect it directly (hits of homeland) or indirectly (close call, Mexico, Cuba, Haiti, Dominican Republic, Puerto Rico, Bermuda). The time period for the analysis is 2006-2011. Heteroscedasticity-robust standard errors, adjusted for clusters by state and week, are in parentheses.

Table 4: Political Information Shocks and Contributions

	Political (1)	Political (2)	Charitable (3)	Charitable (4)
$A^{-1/+0}$	0.020**	0.022**	-0.014**	-0.013*
	(0.008)	(0.010)	(0.006)	(0.007)
A^{+1}		0.003		-0.004
		(0.007)		(0.005)
Observations	14,798	11,035	50,952	50,952
R-squared	0.733	0.746	0.723	0.723
Bi-monthly x County Pair FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
F-test H: $A^1 = A^{-1/+0}$		13.343		.988
p-value		.004		.342

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. The specification run is $Y_{c,t} = \gamma_{p,bt} + \eta_t + \alpha_1 A_{c,t}^{-1/+0} + \alpha_2 A_{c,t}^{+1} + \varepsilon_{c,t}$ with variables transformed with arcsinh transformation. The dependent variables are the aggregate political donations from FEC, charitable donations from ARC. Independent variable is the aggregate political ad spending in the county in the corresponding time period. The results for political donations (columns (1) and (2)), ARC donations (columns (3) and (4)) are estimated for the set of counties within the same congressional district, but located on different sides of corresponding DMA border. Heteroscedasticity-robust standard errors, adjusted for clusters by state, are in parentheses.

Table 5: Political Advertising Shocks and Charitable Contributions (IRS)

	Charitable Deducted (1)	Charitable Deducted (2)
Ads (Congressional Cycle)	-0.014**	-0.014**
	(0.007)	(0.007)
Observations	13,486	13,486
R-squared	0.692	0.778
County Pair FE	Yes	Yes
Congressional Cycle FE	Yes	Yes
Income Group FE		Yes

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. The specification run is $Y_{c,2Y} = \gamma_{borderpair} + \eta_{2Y} + \alpha_1 AdsCong_{c,2Y} + \varepsilon_{c,2Y}$ with variables transformed with arcsinh transformation. The dependent variable is the charitable tax deductions, as reported annually to the Internal Revenue Service (IRS), aggregated over the two-year congressional cycle. Independent variable is the aggregate political advertising expenditures in a county in a two-year congressional cycle. Controls include county pair, congressional cycle, and income group fixed effects. Time period of analysis is 2006-2011. Heteroscedasticity-robust standard errors are clustered by the DMA-congressional cycle.