



UNIVERSITY OF GOTHENBURG
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**The Effect of a Major Catastrophe on Unrelated
Charity Donations**

Tobias Lindé

Supervisor: Christina Gravert
Graduate School

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University of Gothenburg, Department of Economics

Abstract

The 9/11 attacks claimed many lives and caused severe damages, but people responded with great support to the victims, among other things through financial contributions. This paper investigates whether donations to charities supporting the victims came at the expense of donations to other charities, a concern raised but not previously empirically studied. Using detailed panel data on donations to a culture and education charity, a difference-in-difference approach is used to compare donations from people in states with different impact intensity of the attack. While donations to the organization were record low in October 2001, the results show that donations from the more affected states did not decrease more than from less affected following the attack. The findings are robust to different measures of how affected a state was and for different time frames, but an overall effect applicable to all states cannot be rejected.

Keywords: Charity donations, natural experiment, disaster relief, 9/11, Smithsonian
JEL classification: D10, D64

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1. Introduction

Natural disasters and other catastrophes can claim many victims and cause severe damage, but individuals can respond to such events with increased pro-social behavior and generosity, as suggested by the often large financial support to disaster reliefs. However, concerns have been raised that donations to these unplanned events come at the expense of less contributions to other charities, in which case the large amounts of disaster relief only reflects a shift of focus rather than an increase in generosity.

One major catastrophic event occurred on September 11, 2001, when four American planes were hijacked and crashed into the World Trade Centers, the Pentagon and a field in Pennsylvania (The National Commission on Terrorist Attacks Upon the United States, 2004). While the attack was unprecedented in US history, so was the financial response in terms of donations to the disaster relief. Donations quickly reached enormous amounts and already by the end of October 2001 had 9/11-related donations to the American Red Cross surpassed \$500 million and the organization had to stop asking for such donations (American Red Cross, 2002). The total amount of private contributions to the relief and recovery following 9/11 is estimated to \$2.8 billion (The Foundation Center, 2004). The aim of this study is to contribute to the understanding of whether these large amounts came at the expense of less money donated to charities unrelated to the 9/11 attacks.

Previous studies have indicated that when a household increases donations to certain kinds of charities it partly reduces donations to other purposes in the same year (Reinstein, 2011). However, donations to victims of a large disaster in one year have also been found to be positively associated with subsequent donations to other charities (Brown, Harris & Taylor, 2012). Relating to these studies, one could expect that any sign of crowding out of donations to unrelated charities following 9/11 would be temporary and that these donations could eventually increase in the post-disaster period.

Concerns that donors might be reducing their contributions to unrelated charities to allow for donations to those directly supporting the victims of 9/11 have been raised by several actors; in media (Cater, 2002), by important philanthropic organizations (The Foundation Center, 2004) and directly by charitable organizations

(Association of Fundraising Professionals, 2002).¹ However, no empirical evidence of the effect of the attack on donations to unrelated charities has been found. This study thus contributes to a relatively recent body of literature on how donations to different charities interact, by studying the evolution of donations to an unrelated charity organization following 9/11. It addresses the discussed concerns that are of interest to charity organizations specifically and also contributes to a more general understanding of donor and economic behavior.

The Smithsonian Institution is an American non-profit organization that was founded in 1864 with the mission to work for “the increase and diffusion of knowledge”. This is done through activities at the organization’s National Zoo, 19 museums and 9 research centers, most of them located in the District of Columbia. The organization is thus involved in areas of art, culture and education and was hence working for purposes unrelated to the 9/11 and not directly supporting the victims (The Smithsonian Institution, 2017).

This study uses detailed data on donations to the Smithsonian to study how donations to this unrelated organization evolved following the attacks. The dataset contains actual donations to the organization from 1999 to 2015 and as can be seen in Figure 4 and 5 in Appendix A, gift donations were record low in October 2001 (the month following 9/11). The aim of the analysis is to investigate whether this fall in donations was caused by the attacks and how donations were affected in the longer term.

Using a difference-in-differences approach, donations from more affected states are compared to donations from less affected states, before and after the 9/11. Different measures of how affected each state was, capturing the geographical closeness to the attacks and number of victims, are interacted with different time frames. However, none of the interaction terms are statistically significant, neither in the immediate period nor in the post-catastrophe period. This indicates that donations to the Smithsonian did not evolve differently for the more and less affected states

¹ Cater (2002): “Charities unrelated to 9/11 have been affected by some donors switching contributions away”,

The Foundation Center (2004): “In the early aftermath of 9/11, commentary within the nonprofit world understandably alternated between celebrating this charitable response and worrying about its potential negative impact on the revenues of other charitable institutions.”

Association of Fundraising Professionals (2002): “8.3 percent of those surveyed at year-end listed Sept. 11 as the No. 1 challenge facing them in 2001.”

following the attack, suggesting no effect on such donations. However, the results cannot reject any possibly homogenous overall effect applicable to all states.

The rest of the paper is structured as follows. Section 2 provides a review of the previous literature related to the subject of donor behavior. In section 3 the underlying theory is discussed and applied to the subject and the hypotheses are presented. Section 4 describes the data used, defines the variables and presents descriptive statistics. Section 5 discusses the empirical strategy and section 6 provides the results along with robustness checks. Finally, section 7 contains the conclusion and discusses limitations and future research.

2. Literature Review

This section provides a review of the relevant literature, starting with empirical studies of why people donate money and then focusing on papers examining the interaction of donations to different charities.

The body of literature concerning donor behavior provides implications for charities and governments and contributes to the understanding of behavioral economics in general. The many mechanisms driving charity donations are discussed in an extensive review by Bekkers and Wiepking (2010), where the authors focus on a set of key mechanisms that often interact with each other. First of all, potential donors need to be aware that there is a need for donations. This awareness can come primarily from either interaction directly with the beneficiaries or through actions by the charity organizations. But media coverage is also an important channel for awareness and Brown and Minty (2008) show that donations to the victims of the tsunami in the Indian Ocean in 2004 were increased by television and newspaper reports of the disaster. A second channel is solicitations, asking for donations directly or through a fundraising letter, and this has been found to be a mechanism driving a majority of donations (Bryant, Jeon-Slaughter, Hang & Tax, 2003).

Another channel concerns the costs and benefits of donations. Receiving benefits from the charity, such as access to exclusive events or unconditional gifts might increase donations (Falk, 2007), although it has also been found that conditional gifts can actually reduce donations (Newman & Shen, 2012). List (2011) shows that donations are positively associated with household income and also

closely related to changes in the stock market. He also illustrates that when donations are tax deductible, an increase in the marginal tax rate might increase contributions as the “price” for donations decreases (although the income effect of a lower after-tax-income to some extent offsets this effect). The government can thus affect donations by changes in tax policies, but studies have also found that government grants can affect private contributions to charities. Andreoni and Payne (2011) show that government grants to charities crowd out private donations, mostly because charities respond to these grants with less fundraising efforts.

A second set of mechanisms concerns the psychological effects of making donations. Altruism, caring about the purpose that the charity serves, is an obvious motivation for donors. But people may also donate to charities simply because they enjoy giving, it makes them avoid feelings of guilt or because it makes them feel like socially responsible people. Similarly, donating money may improve an individual’s reputation as an altruistic person or make one appear generous. Finally, people may contribute to charities that work for purposes that are in line with the values of the donors (Bekkers & Wiepking, 2010). Several papers (e.g. Hoffman, McCabe & Smith, 1996; Small & Loewenstein, 2003) have shown that being able to identify or having things in common with the beneficiaries tends to increase gifts. These studies provide experimental evidence that a decreasing social distance increases giving.

While charities and governments can use many of the discussed channels to influence donors’ behavior, there are also other things, unrelated to these actors, which might affect donations. Experiencing a natural disaster or a catastrophe like the 9/11 attacks, or knowing someone affected by these events, can increase altruistic feelings of donors without any action from the charities or the government. To better understand altruism and why people give away money without getting anything in return, studies of how such exogenous events are related to donations can provide valuable insights.

Brown et al. (2012) study donations to charities supporting the victims of the tsunami in the Indian Ocean in 2004 and how this unexpected disaster is related to donations to other organizations. Using household survey data the authors find that donations to the tsunami victims are positively associated with donations to all other categories of charities in the subsequent year and conclude that donations to the tsunami victims had no crowding out effect on future donations to other organizations. However, it is possible that a positive trend in the level of generosity or

higher charity activity in general had a positive impact on donations in both years, explaining the positive association. As pointed out by the authors it is also possible that the event had an immediate temporary effect of crowding out that evolved to a crowding in effect in the longer run, but their analysis using annual data is not able to capture this possibility.

A study that does find patterns of immediate crowding out is Reinstein (2011), who studies to what extent a donation to one charitable organization reduces donations to other organizations. Using annual survey data on households' charity donations to different categories, the author finds that in a year when a household increases charity donations to one purpose it simultaneously decreases donations to some other charity categories. The strongest pattern of expenditure substitution is found between charities within health care and education, whereas no effect is found for religious charities. Recording donations per category rather than charity simplifies the survey procedure but could also cause problems for the analysis if people interpret the categories differently. One could imagine some people placing Smithsonian under "Education" while others might refer to it as "Art and culture" in "Other", and it is not completely clear under what category donations to disaster relief would be placed either. Furthermore, with such data it is not possible to study patterns of substitution between charities within the same category, where substitution would perhaps be more present.

Thus, while Brown et al. (2012) find no evidence that donations to an unexpected disaster crowd out *future* donations to other charities, Reinstein (2011) does find that donations to certain categories do crowd out other donations within the *same* year. It is thus plausible that if the 9/11 attacks had an effect on donations to unrelated charities, an immediate effect could differ from a later effect.

People's reactions to 9/11 are reviewed in Morgan, Wisneski and Skitka (2011) and in addition to the large amounts of donations to the disaster relief, documented positive effects have been found on pro-social behavior (Poulin, Silver, Gil-Rivas, Holman & McIntosh, 2009), blood donations (Glynn et al., 2003) and volunteerism (Penner, Brannick, Webb & Connell, 2005). While concerns have been raised that unrelated charities could have suffered a loss of donations following 9/11 (Cater, 2002; The Foundation Center, 2004; Association of Fundraising Professionals, 2002), no empirical study that either confirms or rejects these concerns have been found.

As opposed to the annual survey data used in Reinstein (2011) and Brown et al. (2012), the dataset used in this study contains actual donations on monthly level, which allows a careful evaluation of the short and long term effects of the 9/11 attacks on donations to the Smithsonian. This study thus contributes to a relatively recent body of literature trying to assess whether donations to different charities crowd out each other.

3. Theoretical Background and Hypotheses

In this section the underlying theory related to donor behavior is discussed, the theoretical framework is illustrated and the hypotheses that will later be tested are defined.

3.1 Theoretical Framework

Economic theory explaining charity donations often start from a utility function of the donor, which includes one or several elements reflecting donations. In simple models, donations to charity purposes can be thought of as contributions to a public good and several extensions and modifications of the public goods models that are more specific to charity donations have evolved over time.

In a famous paper on social interactions, Becker (1974) models how the utility of one individual may depend on the utility of others. Starting with an application on the family, the utility function of one family member includes the utility of the rest of the family and a similar model is later applied to charity. While he discusses that charitable contributions not necessarily only induce utility through the wellbeing of the recipient but could also reflect social acclaim from donating, it is not until later that models that incorporate both of these effects are developed.

Andreoni (1989) builds on a model of public goods and add an element capturing a feeling of “warm glow”. In his model of impure altruism, donors care not only about the total amount that is donated to a charity purpose but also receive additional utility from their own donation from the act of giving.

With inspiration from the model by Reinstein (2011) the theoretical considerations of this paper are presented below. Extending the model of warm glow to include several warm glow elements for different charity purposes, the utility U_i of

individual i could be written as a function of own consumption x_i , and characteristics of warm glow, w_i^A and w_i^B , from donating to organization A and B :

$$U_i = U_i(x_i, w_i^A, w_i^B), \quad i = 1, \dots, n,$$

As is discussed in Schokkaert (2006), the elements of warm glow could be generalized to reflect many different channels increasing the utility of the donor, like material self-interest, reputation and other mechanisms discussed in the previous section. These warm glow characteristics could be modeled as the products of the individual's gift g_i^j to charity j and the parameters or utility shocks μ_i^j measuring the effectiveness with which donations to charity j are transformed into utility:

$$w_i^j = g_i^j \mu_i^j, \quad j = A, B$$

The simplified budget constraint of each individual, where y_i is income that can be spent on either own consumption or donations to the different charities, can be written as:

$$y_i = x_i + g_i^A + g_i^B$$

Thus, the individual maximizes the utility function by choosing how much to donate to each charity:

$$\max_{g_i^A, g_i^B} U_i(y_i - g_i^A - g_i^B, g_i^A \mu_i^A, g_i^B \mu_i^B)$$

The optimal donation to purpose j by individual i can hence be modeled as a function of the exogenous variables income and the utility shocks:

$$g_i^j = g_i^j(y_i, \mu_i^A, \mu_i^B), \quad j = A, B$$

Receiving a fundraising letter from charity A can thus be seen as a positive shift in parameter μ_i^A for individual i . Similarly, if the purpose that organization A is serving

is highlighted in media this would increase the parameter value for that organization and individuals would receive a higher utility of donating to charity *A*. How this would effect donations to charity *B* depends on to what extent donations to different charities are substitutes.

Donations to the Smithsonian could thus be affected by utility shocks related to other charities or by shocks related to the Smithsonian. First, the 9/11 could be seen as a utility shock increasing the value of donating to charities directly supporting the victims and any increase in such donations would have to come at the expense of either less donations to other charities or less consumption. If donors dedicate a fixed amount for charity donations in general, then the considerable amounts donated to the charities supporting the victims would decrease total donations to other charities by the same amount. More reasonable is perhaps that donations are not completely perfect substitutes and different kinds of charities have different degree of substitution, as previous studies have found (Reinstein, 2011). While contributions to the victims and to the Smithsonian can both broadly be defined as charity donations, it is plausible that donations to different purposes to some extent yield different kinds of utility. Hence one could expect some but not complete crowding out of donations to other purposes.

Secondly, if 9/11 made people act more pro-socially and increased the overall level of generosity and altruism then this could be modeled as an increase in all the utility shock parameters for donations to all charitable organizations, including the one for the Smithsonian. One can imagine that being encouraged to donate to one purpose could also make people reflect more about donations to other purposes as well. Any crowding out effect caused by donations directly to the victims could be assumed to be immediate and then diminish with time as such donations quickly reached high levels and were eventually not even asked for by some organizations (American Red Cross, 2002). This could be modeled as a subsequent decrease in the utility parameter for donations to the victims or simply as those donations not being an alternative any more. If the increase in the overall level of altruism or generosity is more permanent and people still want to donate more, then this would increase donations to unrelated charities, like the Smithsonian, in the post-disaster period.

To empirically examine the effect of 9/11 on charity donations to the Smithsonian, donations from people living in the more affected states will be compared to donations from people living in less affected states. Since other,

unobserved shocks could affect donations to the Smithsonian following 9/11 the comparison of donations from people in states with different impact intensity is necessary and this is discussed more in the empirical strategy part in section 5. As discussed in the previous section, the social distance to the victims is an important determinant of donations and it is likely related to the actual distance to the victims. If one has more in common with or is more able to identify with the people in one's state then one is more likely to donate if people nearby, in the state, are affected by the attacks. Thus, the shift in the utility parameters can be assumed to be stronger in states that were directly affected, had more victims or that are geographically closer to the disaster, and donations would then be more affected in these states.

3.2 Hypotheses

Based on the previous discussion the hypotheses can be specified. The null hypothesis is that donations to the Smithsonian evolved in the same way in more and less affected states after the 9/11 attacks. The first alternative hypothesis is that donations to the Smithsonian decreased more in the more affected states in the immediate period after the attacks. The second alternative hypothesis is that donations to the Smithsonian increased in the more affected states relative to the less affected in the later post-disaster period.

4. Data and Variables

In this section the data and the variables used in the subsequent analysis are presented. The first subsection describes the data sources, the second defines the dependent variable and in the third the different independent variables are explained. Finally, the fourth subsection contains descriptive statistics of the presented variables.

4.1 Data

The main dataset used for the analysis in this study contains all donations to the Smithsonian during the period from October 1999 to December 2015 made by individuals that were or had been a *Friends of the Smithsonian* member. Anyone can become such a member, with the annual membership fee starting at \$75, and the

dataset contains these dues and additional gifts made by each individual. Since the dues are paid on a regular basis, once a year as long as the individual wants to be a member, these transactions are assumed to be rather persistent over time. The additional gifts on the other hand, are voluntarily donations that the members can make at any time they want and these transactions are more likely to be affected by shocks. If donations to organizations directly supporting the 9/11 victims did crowd out other donations, it is more likely that the additional gifts were affected than that a member canceled the membership, and hence this study will focus on the evolution of these additional gifts to the Smithsonian.

Since the analysis will be based on the location of the donors, individuals that do not report a US home address are excluded and for individuals that provide several addresses the home address is used. Of these 290,690 individuals, 156,656 made at least one gift donation during the whole period, while the others contributed through the dues only. The total number of gift donations is 498,578 with the average gift being approximately \$71. Since each gift-donating individual only donates approximately three gifts during the whole period, treating the data as individual panel data leads to a strongly unbalanced panel that can cause potential problems (Wooldridge, 2010). A positive (negative) shock on donations would likely both increase (decrease) the size of planned donations but also increase (decrease) the number of donations which would have an ambiguous effect on the mean donation and lead to inaccurate results. Even if donations were aggregated at monthly individual level, more than 95% of the observations would be zero-observations, which would cause other distributional problems (Wooldridge, 2010).

To solve these issues the data is aggregated on state-month level to create a balanced panel dataset suitable for the subsequent analysis. Thus, in the end, the dataset contains 9945 state-month observations for the 51 US states (including District of Colombia) and the descriptive statistics for this data are provided in the subsection 4.4.

To evaluate the effect of 9/11 on these donations, several measures of how affected each state was by the attack will be used and these measures are discussed in the next subsection. The number of victims from one's state following the 9/11 attacks can be used as one such measure. The memorial site² lists all the names of the

² <http://names.911memorial.org>

victims and allows searches of victims per residence but does not provide any list or table of the aggregate number of victims per state. StateMaster (2002) does however provide such an aggregated list (see Table 10 in Appendix C) based on the website memorial in 2002 and this data will be used as a measure of how affected each state was. The numbers largely correspond to those in a table of victims from the World Trade Center only, provided by the Center for Disease Control and Prevention (2002), which verifies its accuracy, but also include all the victims from all the plane crashes (also those that crashed into the Pentagon and in Pennsylvania).

4.2 Dependent Variable

The amount of gift donations to the Smithsonian per state per month is the outcome of interest in the analysis. Figure 2 in Appendix A provides the distribution of state-month gift donations and as can be seen it is very positively skewed. The log of state-month gifts is more normally distributed and will be used as dependent variable in the analysis. Since factors affecting the monthly donations in each state are likely to have an effect that is relative to the level of donations rather than an absolute effect, the log transformation is suitable. A small share (less than one percent) of the state-month observations has zero donations and the log of zero is not defined, hence this issue needs to be solved. Common techniques to account for this is to either take the log of the variable plus a small constant or to simply replace zero-observations with a zero after the transformation (Wooldridge, 2010). The latter alternative has been used in the previous literature on the subject (Brown, 2012) and (since there are no observations between zero and one) will be applied in this study.

4.3 Independent Variables

While the 9/11 attacks can be seen as an attack on the U.S., some states were more affected than others. To be able to compare donations from states that were more and less affected by 9/11, measures of how affected each state was are presented and defined in Table 1. The states where the hijacked planes took off, where they were scheduled to land and where they crashed are defined as the *Directly affected* states as they represent the actual locations of the event. People living in these states can be expected to be even more emotionally attached, have a smaller social distance and be

more likely to know (someone who knows) some of the victims, although this also depends on the population size. Furthermore, exposure to news and media reports of the attacks could be even larger in these states, which has been shown to increase donations (Brown & Minty, 2008). New York, California and New Jersey are all among the *directly affected* states and also the top three states in terms of total foundation and corporate donations to the 9/11 disaster relief (The Foundation Center, 2004), which presumably is correlated with donations from private individuals. Based on this, donations from the *directly affected* states can be expected to be most affected and hence this will be one of the preferred measures.

Another direct measure of how affected a state was by the attacks is the number of victims that were from the state. As was shown in Table 10 in Appendix C, a majority of the states had at least one victim and this measure thus involves more states while also allowing a more heterogeneous impact intensity. Since the distribution of victims is very skewed and to capture a proportionate rather than an absolute change in the number of victims, $\ln(\text{victims})$ will be used as a second measure of how affected each state was. Since some states have zero victims and some have one, the log of the number of victims plus one will be used to include all states and keep the relative order (Wooldridge, 2014). As a comparison, results using *Victims* without the log transformation are also provided in the robustness section.

While the number of fatalities caused by disasters like the tsunami in the Indian Ocean in 2004 and the Haiti earthquake in 2010 by far exceeded the number of victims following 9/11 and hurricane Katrina, U.S. private giving for disaster relief was higher for the domestic catastrophes (Center on Philanthropy, 2005; Gannon, 2014). This suggests that, among other things, the geographical closeness of a catastrophe could be a determinant of donations, and hence people living in states closer to the attacks can be expected to donate more to the victims. To capture this, two alternative measures of how affected each state was, based on the geography of the state are also used. The states *Bordering NY* are included in the first and then a wider measure of the 14 states most *Close to NYC* is also used. Several of the presented measures coincides with each other and some states are included in all measures, but they will all be used in the analysis as a robustness check and to possibly distinguish what channel is the most important.

Table 1. Measures of how affected each state was by the 9/11 attacks

Variable name	Definition	States affected
<i>Directly affected</i>	Dummy equal to one for states where the planes departed, were scheduled or crashed	NY, PA, DC, VA, MA, NJ, CA
<i>Bordering NY</i>	Dummy equal to one for states bordering NY (including)	NY, PA, NJ, CT, MA, VT, RI
<i>Close to NYC</i>	Dummy equal to one for the 14 states closest to NYC	NY, PA, NJ, CT, MA, VT, RI, ME, NH, DE, MD, DC, WV, VA
<i>Victims</i>	Number of 9/11 victims	NY, NJ, MA, VA, CT, MD, CA, PA, DC, NH, IL, TX, FL, RI, ME, GA, AL, LA, NC, OH, KS, DE, MO, HI, MI, OK, CO, TN, NE, NV, MS, KY, NM, AZ, OR, IA, UT, ID, WI
<i>Ln(victims)</i>	Ln(number of 9/11 victims+1)	

4.4 Descriptive Statistics

Table 2 provides the descriptive statistics for the presented variables. Between October 1999 and December 2015 there are 9,945 state-month observations. The average state-month gift donation is \$3,572 and the maximum \$71,273. The number of 9/11 victims per state ranges from zero to 1747 and more detailed number on this are provided below. Approximately 14 percent of the states are included in the measures of *Directly affected* and *Bordering NY* and 27 percent of the states are included in the broader measure *Close to NYC*.

While the Smithsonian has most of its museums and activities in the District of Columbia, donations to the organization come from all over the U.S. The total amounts of gifts donated per state during the whole period (October 1999 to December 2015) are shown in Figure 3 in Appendix A. As can be seen, the most populous states (like California, Texas, Florida and New York) naturally contribute with higher total amounts. Similarly, states closer to District of Columbia (such as

Maryland and Virginia), where the majority of the Smithsonian’s museums are located, tend to donate more gifts.

Figure 4 and 5 in Appendix A provide the distribution of donations over time and captures a slightly positive trend with high monthly variation. As mentioned earlier, the lowest month of all in terms of donations was October 2001. In Figure 6 in Appendix A, the monthly patterns of donations are provided and as can be seen, October is usually the lowest month in terms of donations whereas December is the highest.

Table 10 in Appendix C provides the number of victims per U.S. state. Although the 9/11 attacks only took place in a few states, with New York being the most affected area, the victims came from many different areas. Ten states had at least ten victims and 39 states had at least one victim.

Table 2. Descriptive statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
<i>Donation</i>	9945	3571.855	5261.557	0	71273.05
<i>Ln(donation)*</i>	9945	7.349288	1.478508	0	11.17427
<i>Victims</i>	9945	56.88235	257.9909	0	1747
<i>Ln(victims)*</i>	9945	1.492687	1.658042	0	7.466228
<i>Directly affected</i>	9945	0.137255	0.344134	0	1
<i>Bordering NY</i>	9945	0.137255	0.344134	0	1
<i>Close to NYC</i>	9945	0.274510	0.446289	0	1

* As mentioned in the section Dependent/Independent Variables, the log transformation causes some missing values, but after the discussed adjustments this is solved.

5. Empirical Strategy

To study the effect of 9/11 on charity donations to the Smithsonian, one would ideally like to observe donations from an individual both in the absence and the presence of the attacks and then simply compare these values. Unfortunately, this is obviously not possible and hence some other approach is necessary.

Simply studying the evolution of total donations before and after 9/11 is not enough to claim any causal effect, since we do not know the counterfactual pattern;

how would donations have evolved in the absence of the attack. It is possible that some other unobserved factor caused the record low gift donations in October 2001. Similarly, comparing donations from more and less affected states after the attack does not account for possibly different levels of donations from the two groups before the attack.

The difference-in-difference (DID) approach combines the two approaches just mentioned and takes advantage of the two-dimensional panel data (a cross-section of state observations over several time periods). By comparing the difference before and after the attack in the difference in donations from the two groups the method controls for unobserved time-specific and group-specific effects. The approach has been widely used to study policy and law changes but is applicable to study natural experiments (where an exogenous event affects one group of individuals, firms or states more than another) in general. The main assumption that the DID method relies on is that of a parallel trend in the outcome variable for the two groups in the absence of treatment. Since one group acts as the counterfactual, the difference in the difference in the outcome variable should only reflect the effect of the attack and not by some other factor. While this assumption cannot be tested directly, a careful examination of the trends before the treatment can contribute to whether the assumption is likely to hold or not (Wooldridge, 2010, 2014; Angrist & Pischke, 2008). A graphical analysis of the trends will be provided below and the parallel trend assumption will be further tested in the robustness subsection (6.2).

The 9/11 attacks provide a natural experiment setting (not implying that the attack was an experiment but simply using the analytical terminology) where individuals in some states were more affected than others (although from the perspective that the attack was on the U.S., all individuals were to some extent affected). People living in states that were directly affected by the attacks, where the victims lived, or that are geographically closer to NYC are likely to be more emotionally affected by the attacks, more likely to know (someone who knows) some of the directly affected victims and are likely to be even more exposed to news reporting and media regarding the attacks, compared to those living in other states. While the traditional DID model compares one group that was exposed to the natural experiment treatment with a control group that was not affected at all, the comparison of donations from more and less affected states will capture any additional effect for the more affected states.

Applying the DID approach to estimate the effect of 9/11 on donations to the Smithsonian, the baseline model can thus be specified as:

$$\ln(\text{donation}_{st}) = \beta_0 + \beta_1 \text{post}_t + \beta_2 \text{affected}_s + \beta_3 \text{post}_t * \text{affected}_s + \varepsilon_{st} \quad (1)$$

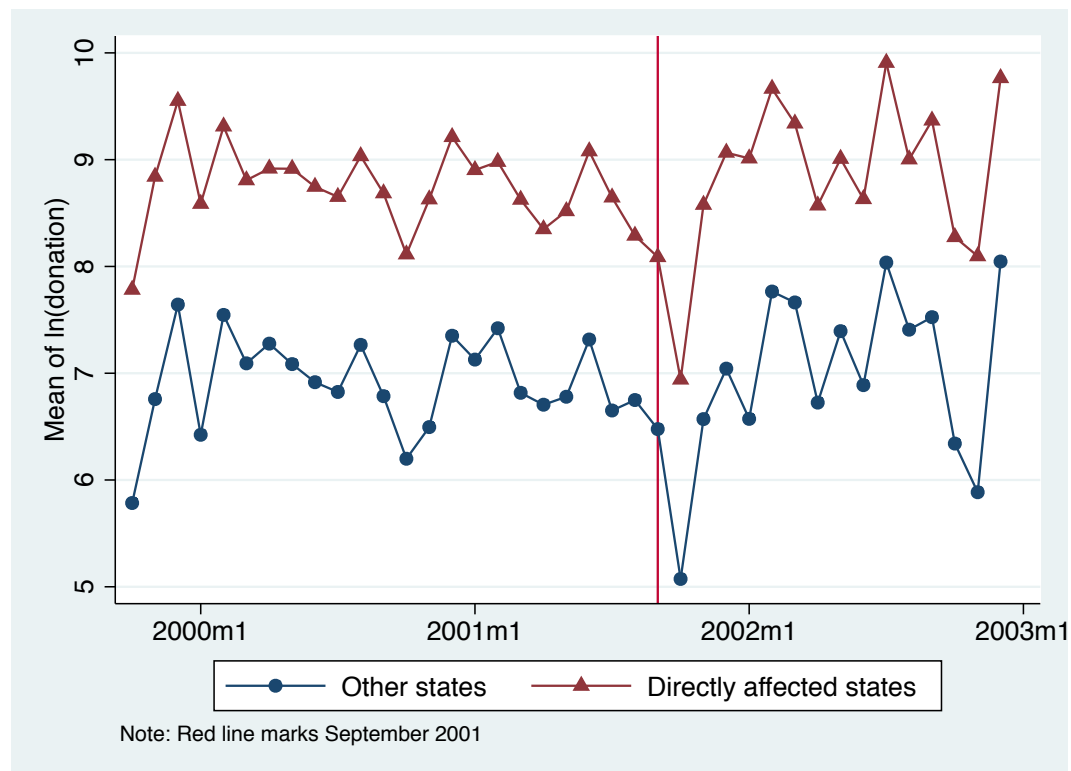
In Equation 1 the dependent variable $\ln(\text{donation}_{st})$ is the log of gift donations to the Smithsonian from state s at time t . Post_t is a dummy equal to unity for observations in and after September 2001 and its coefficient captures state-invariant time effects on donations before and after this date. As is shown in List (2011) the stock market is closely related to charity donations and it applies to all states. Similarly, annual events like Christmas and the end of the year, when people tend to give more (Figure 6 in Appendix A shows that donations to the Smithsonian are highest in December), vary over time but not across states. affected_s is a dummy equal to unity for states that were more affected by the 9/11 attacks (represented by the different measures defined subsection 4.3) and the coefficient captures time-invariant state effects. As was previously shown (in Figure 3 in Appendix A), the more populous states and states closer to DC tend to give more, but the population of a state can be considered rather constant, at least in the shorter run, and the geography of a state is definitely time-invariant. The interaction term of Post_t and affected_s is the variable of interest and the coefficient captures any additional effect on donations from the more affected states following September 2001.

The error term ε_{st} is assumed to be uncorrelated across states, but errors for each state in different periods may be correlated (an issue common in DID models), and this can lead to incorrect standard errors, which will make the statistical inference invalid. To account for this within-cluster error correlation, Ordinary Least Squares (OLS) estimations with cluster robust standard errors will be used throughout the analysis. These errors should be clustered at the state level and are also robust to potential issues of heteroskedasticity in the error term. An alternative solution is to use Generalized Least Squares, but that requires more strict assumptions and the former approach is a common solution in applied work. Nonetheless, clustered standard errors are generally larger than normal ones, which can lead to fewer rejections of the null hypotheses and this issue will be addressed more in the

robustness section (Bertrand, Duflo & Mullainathan, 2004; Angrist & Pischke, 2009; Cameron & Miller, 2015).

As previously mentioned, the parallel trend is crucial for the DID estimator to be a valid estimator of the causal effect and an examination of the pre-trends of the outcome variable can indicate whether the assumption is likely to hold or not. In the absence of the treatment (in this case being “directly affected” by 9/11 or having victims from the state) the trend in donations should be parallel for the two groups or for states with more and less victims. Figure 1 shows the evolution of average log of donations per month for the directly affected and for other states. While the directly affected states have a constantly higher level of donations, the overall trends for the two groups prior to 9/11 are highly related suggesting the parallel trend assumption is likely to hold in general, although the change in some individual months differ slightly. This assumption is further tested in the robustness section, where the measures of how affected each state was are interacted with the months prior to the attack to more carefully detect any pre-attack trend differences.

Figure 1. The trends of the average gift donations to the Smithsonian for directly affected and other states



6. Results and Analysis

In this section the results are presented and analyzed and the robustness of these are checked and discussed. The first subsection contains the baseline results for the two preferred measures and time frames, provides a discussion of these, and also addresses potential alternative explanations. In the second subsection the parallel trend assumption is further investigated, the effects for individual months are provided and the alternative measures and specifications are tested.

6.1 Baseline Results

The baseline results of this study are presented in Table 3. Column 1 provides the coefficients from an OLS estimation of the difference-in-difference model specified in Equation (1) in the previous section. The coefficient of “After 9/11” is positive and statistically significant indicating that state donations were approximately 0.2 log points, or 22 percent, higher after 9/11 compared to before. Since these coefficients come from an estimation based on the full sample this positive coefficient likely reflects the positive long-term trend in donations (see Figure 4 in Appendix A) rather than an immediate increase following 9/11. The coefficient for “Directly affected” is also positive and statistically significant with donations from the directly affected states being on average 1.8 log points higher than from other states. Since the directly affected states (where the planes departed, were scheduled or where they crashed) are also some of the more populous states (e.g. New York and California), this difference is reasonable. After controlling for these time and group differences, the interaction-term “After 9/11 × Directly affected” captures any change in the difference between the two groups before and after 9/11. Thus the coefficient of the interaction term could be interpreted as the causal additional effect of 9/11 on donations from the directly affected states, assuming the parallel trend assumption holds. This coefficient is positive but not statistically significant, indicating that donations did not change in the directly affected states relative to the other states following 9/11.

Table 3. Baseline results of the effect of 9/11 on donations to the Smithsonian**Results. Dependent variable: Ln(donations)**

Independent Variables	(1)	(2)	(3)
After 9/11	0.208** (0.029)	0.050 (0.030)	
Directly affected	1.833** (0.295)	1.833** (0.295)	1.833** (0.295)
After 9/11 × Directly affected	0.010 (0.058)	0.036 (0.039)	
September-December 2001			-0.623** (0.055)
September-December 2001 × Directly affected			0.044 (0.091)
2002			0.274** (0.030)
2002 * Directly affected			0.033 (0.041)
Constant	6.913** (0.171)	6.913** (0.171)	6.913** (0.171)
Sample	Full	<2003	<2003
Year-month fixed effects	No	No	No
State fixed effects	No	No	No
Observations	9,945	1,989	1,989
R-squared	0.19	0.18	0.21
Adjusted R-squared	0.19	0.18	0.21

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors clustered at state level in parentheses. The full sample contains all state-month observations from October 1999 to December 2015. In column 2 and 3 the sample is restricted to observations before 2003 only.

In column 2 the sample is restricted to only include observations before 2003. If 9/11 did affect donations, the effect is likely to be largest in the immediate period after the attack and then diminish with time. With the restricted sample, the period after 9/11 is shorter and the coefficient for donations in the post period is no longer statistically significant, but the general difference between the directly affected and other states remains. The interaction term is still positive and larger than in column 1, but again not statistically significant.

Column 3 provides the estimates of an extended model, where the immediate effect is separated from the later effect to allow for an effect to change over time. As discussed in the theory section, it is possible that 9/11 had an immediate negative

effect on donations to the Smithsonian if people were shifting donations to charities directly supporting the victims at the expense of less money donated to other charities. However, in the longer run any negative effect is likely to diminish, as a substitution effect from donations to the victims would decrease with time, and possibly turn positive if the attacks had an overall positive effect on charity donations. The coefficients show that donations were statistically significantly lower during September to December 2001 (compared to the period before September 2001) and then statistically significantly higher in 2002. The interaction terms for both periods is positive but not statistically significant providing no evidence of any immediate or post-disaster effect of the 9/11 attacks on donations from more affected states compared to donations from other states. Thus, the null hypothesis that donations to the more affected states evolved in the same way as donations from less affected states after 9/11 cannot be rejected.

While one could expect any effect of 9/11 on donations to be larger in the directly affected states than the rest of the country, it is plausible that the effect is not homogenous. New York, which suffered the most in terms of number of victims and damage, and Pennsylvania, where one of the planes crashed into a field, were both directly affected by the attacks but the effect on donations would presumably be larger in the former. Similarly, Connecticut (which borders to New York and where 65 of the victims lived) and Alaska (far away on the other side of the country and with no residents among the victims) are neither in the group of directly affected states but one could expect donations from the former to be partly affected (at least more than donations from the latter).

To allow for the possibility of heterogeneous effects, Table 4 contains the results from estimations with a more continuous measure of treatment, the log of victims per state. While one should not expect that e.g. 10% more victims following the attack automatically decreases or increases the amount of donations from the state with an exact proportion, the measure can be an indicator of how affected the residents were by the attacks. People living in a state with more victims can be assumed to be more emotionally attached, more likely to know (someone who knows) some of the victims and even more exposed to media reports of the attacks. Since the treatment is now different for each state, it is necessary to control for state fixed effects to allow each state to have individual initial levels of donations. The inclusion of time fixed effects controls for state-invariant time effects and yields the same

estimates as if dummies for the post periods had been used, like in Table 3, but might reduce the standard errors (Wooldridge, 2010). The coefficient of the interaction-term “After 9/11 \times ln(victims)” now captures any effect on donations following 9/11 related to the number of victims in each state. The coefficient for the interaction-term is neither statistically significant in column 1 with the full sample nor in column 2 with the restricted sample. Similarly, when allowing for an immediate and a later effect in column 3, the coefficients are not statistically significant.

Table 4. Baseline results of the effect of 9/11 on donations to the Smithsonian

Results. Dependent variable: Ln(donations)			
Independent Variables	(1)	(2)	(3)
After 9/11 \times ln(victims)	-0.002 (0.014)	0.004 (0.013)	
September-December 2001 \times ln(victims)			0.023 (0.023)
2002 \times ln(victims)			-0.002 (0.013)
Sample	Full	<2003	<2003
Year-month fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Observations	9,945	1,989	1,989
R-squared	0.73	0.70	0.86
Adjusted R-squared	0.73	0.69	0.85

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors clustered at state level in parentheses. The full sample contains all state-month observations from October 1999 to December 2015. In column 2 and 3 the sample is restricted to observations before 2003 only.

The results from these two tables are thus in line and show that donations from the more affected states did not change relative to donations from less affected states following 9/11. These results contribute to the existing literature on the subject adding to the understanding of donor behavior. Similar to Brown et al. (2012) this study finds no evidence of crowding out of donations to unrelated charities (the Smithsonian) the year after a severe catastrophe (although no positive relation to subsequent donations is found either). The results of this study also complements to a question raised in the aforementioned study, finding no evidence of any immediate, short term crowd out either (for donations to the Smithsonian). Reinstein (2011) found that donations to

some, but not all, kinds of charities display patterns of being substitutes. Thus while the presented results suggest that donations to the Smithsonian were not affected by the 9/11, it is still possible that some other kinds of charities were indeed affected. It could be the case that following the attack donors decrease donations to some unrelated charities but do not cut down on contributions to the Smithsonian, which to some extent preserves American culture.

Before moving on to check the robustness and sensitivity of the results, some alternative explanations for the results are discussed. First, it is likely that the 9/11 attacks to some extent affected people living in all states and this overall effect would not be distinguished in these estimations comparing more and less affected states. The lack of evidence of any additional effect on donations for more affected states makes it less likely, but it could still be the case that there was a homogenous effect on donations for all states.

Another potential explanation as to why no immediate negative effect on donations from the more affected states was found could be that the Smithsonian responded to the attacks. It is for example possible that the organization sent out additional solicitations to counter a fear of losing donations to other charities. As previous studies have shown, a large share of donations come in response to solicitations (Bryant et al., 2003) and if the Smithsonian sent extra solicitations to the more affected states following the attack then this would be a serious threat to the validity of the results. A benefit of this paper is that the dataset contains aggregate data on solicitations sent to the members as well, allowing a partial control for this potentially confounding explanation. As can be seen in Figure 7 in Appendix A, the numbers of solicitations sent were not remarkably high in the months following the attack, which mitigates this threat. Furthermore, the organization usually sends solicitations based on type of membership rather than the geographical location of the donors (personal correspondence via E-mail, March 27, 2017), which further mitigates the potential threat of additional targeted solicitations following the attack.

6.2 Robustness and Sensitivity

In this section the robustness of the results and the sensitivity of changing the specification is presented and discussed. As mentioned earlier, the main assumption

that the difference-in-difference technique relies on is that of a parallel trend in the outcome variable in the absence of treatment. The graphical examination of the trends in the previous section supported a parallel trend, but to further evaluate the assumption and to allow for monthly short-term effects, Table 5 and 6 in Appendix B contains results from regressions with the treatment variables interacted with monthly dummies. In Table 5 (Appendix B) the coefficients for dummies of three months prior to and three months after September, along with September 2001 are interacted with “Directly affected”. The interaction terms with the three months prior to September are not statistically significant, supporting the likelihood of a parallel trend in absence of the treatment. Neither for September, October nor November 2001 are the coefficients of the interaction terms statistically significant, meaning that even in the short run was there no evidence that the more affected states decreased donations relative to the less affected states. This suggests that some other factor could explain the record low donations to the Smithsonian in October 2001, although it does not rule out the possibility that there was an overall homogenous effect that affected donations from all states. The interaction term of “December 2001” and “Directly affected” is however positive and statistically significant. While the previous estimations found no overall effect, this result indicates that there was a short term effect in December 2001, when donations from directly affected states increased relative to donations from less affected states. In Table 6 in Appendix B the results of a similar regression are provided, with the only difference being that the monthly dummies are instead interacted with the log of victims. The results are in line with those of Table 5, with the treatment interacted with December being positive and the only statistically significant one, suggesting that in December people living in states with more victims of 9/11 increased their donations compared to those in states with fewer victims.

Although illustrative to find any short-term effects, this multiple hypotheses testing increases the likelihood of committing at least one Type I error (falsely rejecting a null hypothesis that is true) and this higher likelihood of finding at least one of the coefficients statistically significant should be taken into account (List, Shaikh & Xu 2016). Nonetheless, the coefficient for the interaction with December is statistically significant with both measures making it more likely that it reflects an actual short-term effect rather than a Type I error.

While the variables “Directly affected” and “ln(victims)” are considered the most appropriate measures of how affected a state was following 9/11, Table 7 in Appendix B provides the results from the other potential measures discussed in subsection 4.3. Column 1 and 5 contain the results with the preferred measures (included again to simplify a comparison of the different measures) and all models control for time fixed effects. In column 2 the measure of treatment is a dummy equal to unity for the 14 states that are located closest to NYC, in column 3 the measure is a dummy indicating whether the state borders New York or not and in column 4 the treatment variable is number of victims in the state (without the log transformation). Similar to the previous results with the preferred measures, the coefficients of the interaction terms are not statistically significant for the alternative measures either. Thus, the conclusion of no evidence for any immediate negative effect, or positive effect in the longer run, on donations to the Smithsonian from the states that were more affected by 9/11 compared to the other states is robust to these different measures of treatment.

As discussed in the previous section, the use of clustered standard errors in DID settings like this is considered appropriate to control for error correlation within the state, but the clustering of errors also tends to increase them, leading to less rejections of the null hypotheses than with conventional standard errors (Bertrand et al., 2004; Angrist & Pischke, 2008; Cameron & Miller, 2015). However, most of the coefficients are positive meaning that it is not due to large standard errors that any significant negative effect on donations is not found (if anything the effect would likely be positive). Nonetheless, to check whether the lack of evidence of any immediate or long term effect is due to the use of clustered standard errors, Table 8 and 9 in Appendix B provide the baseline estimations (same as those in Table 3 and 4) but with conventional standard errors. As can be seen in the tables the coefficients for the interaction terms are again not statistically significant and the conclusion is the same, there is no evidence that more affected states did decrease or increase donations compared to other states following 9/11.

7. Conclusions

This section provides the conclusions of this study, acknowledges the limitations and leaves some suggestions for future research on the subject.

Following 9/11 and the large amounts of disaster relief donated by individuals in response to the attacks, concerns were raised that these donations came at the expense of other donations. The results of this study show that although gift donations to the Smithsonian were record low the month after 9/11 (in October 2001), there is no evidence that donations from states that were more affected by the attack decreased more than donations from less affected states following the attack. These results are robust to different measures of how affected each state was and different time frames and suggest that some other factor could be causing the low donations in October. The results are likely to hold for charities that are similar to the Smithsonian but cannot be generalizable to all kinds of charities and hence the external validity is limited. It is still possible that charities with other purposes are considered more substitutable by donations to disaster relief and donations from more affected states to such charities could show more signs of being crowded out.

The comparison of donations from more and less affected states over time controls for unobservable state-invariant time effects and time-invariant state effects which makes the results more credible, but one limitation of the approach is that it fails to identify any overall effect applicable to all states. Although the lack of evidence of any additional effect on donations from the more affected states makes it less likely, it is possible that the 9/11 had a homogenous effect on donations from all states and this potential effect would not be distinguishable in the results. If the Smithsonian had received enough donations from donors living in other countries this could have been a more suitable control group. However, overseas donors contributing to a charity preserving American culture are also likely to be affected by and respond to the 9/11 (but perhaps less than individuals living in less affected U.S. states). Another alternative approach could be to examine how donations to an international organization (with donors from many different countries) changed after that attack for donors from the U.S. compared to those from other countries.

While the control for time fixed and state fixed effects strengthens the internal validity of the results, another limitation of the study is that the method does not control for unobserved effects that varies both over time and across states. Additional

solicitations sent out following 9/11 to counter a potential loss of donations could be one such channel, but as discussed earlier solicitations did not increase after the attack and are usually not based on the location of donors. However, it is not impossible that other unobserved events or actions by the organization coincide with or were aimed at the more affected states.

Ideally when studying donor behavior, one would like to have data on the exact timing of all charity donations to all different charities made by each individual. But finding such a dataset is challenging and the previous literature has had to compromise, either using annual survey data on donations to different categories (e.g. Reinstein, 2011; Brown et al., 2012) or using detailed data on donations to only one charity like in this study. However, data from “charity accounts” –accounts where individuals deposit money and then distribute to the different charities of interest – could provide data that is more detailed in many ways. With access to such a dataset, with the exact timing of actual donations to different charities, future research could contribute further to the understanding of donor behavior and the interaction of donations to different organizations.

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Appendices

A. Graphical presentation of the outcome variable and number of solicitations

Figure 2. The distribution of state-month gift donations to the Smithsonian from October 1999 to December 2015

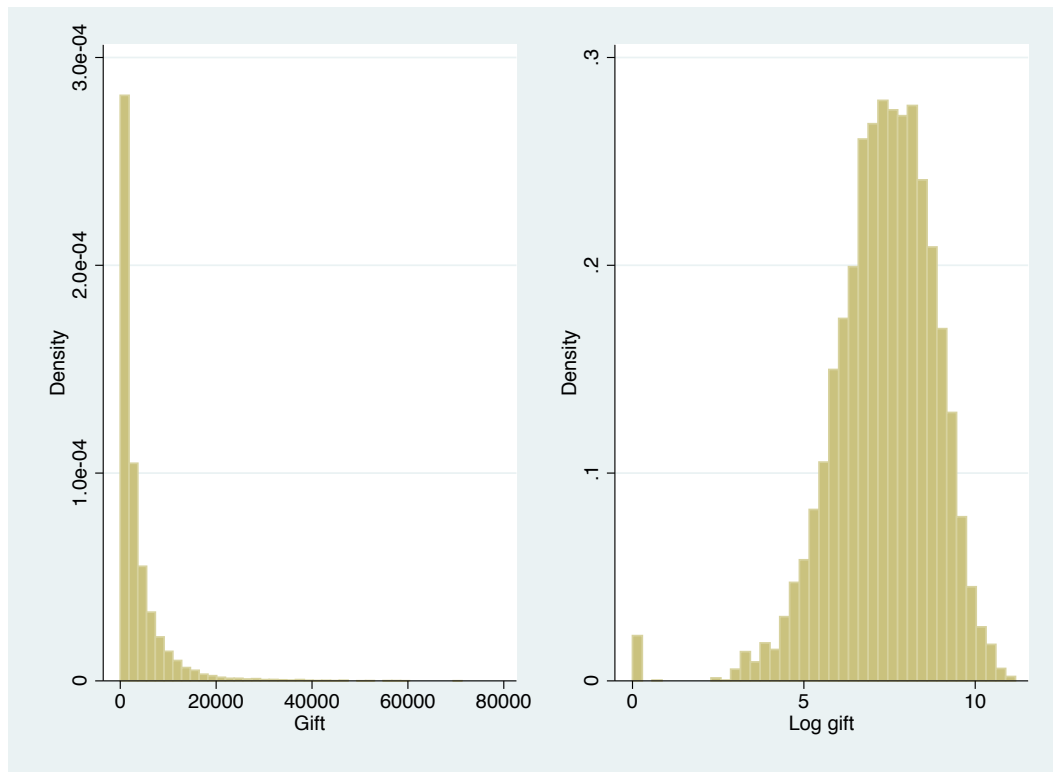


Figure 3. Total amount of gift donations per state to the Smithsonian from October 1999 to December 2015

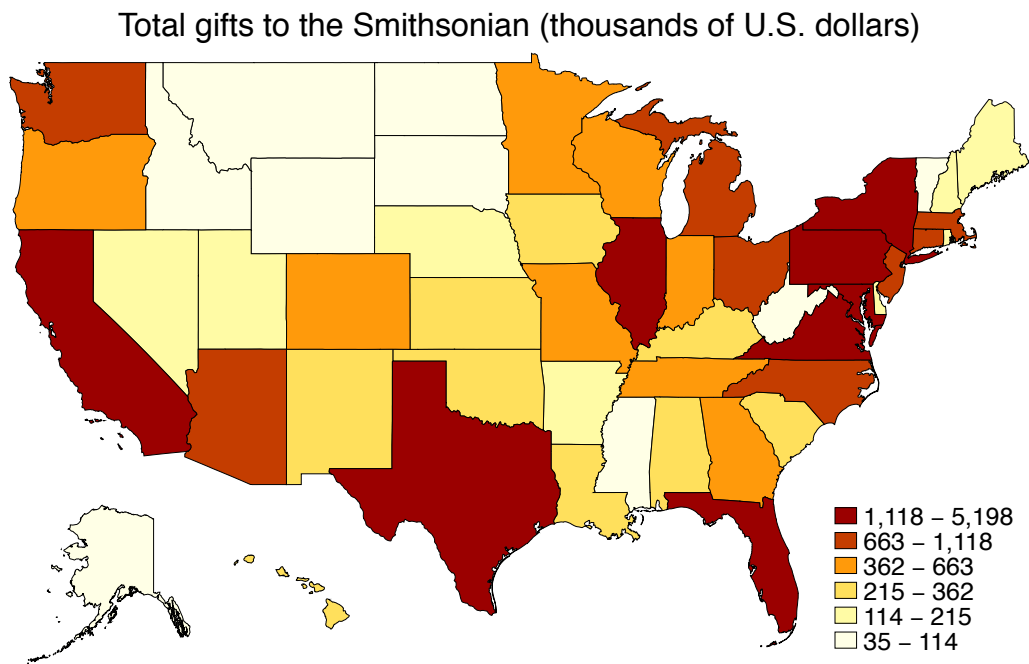


Figure 4. Total amount of monthly gift donations to the Smithsonian from October 1999 to December 2015

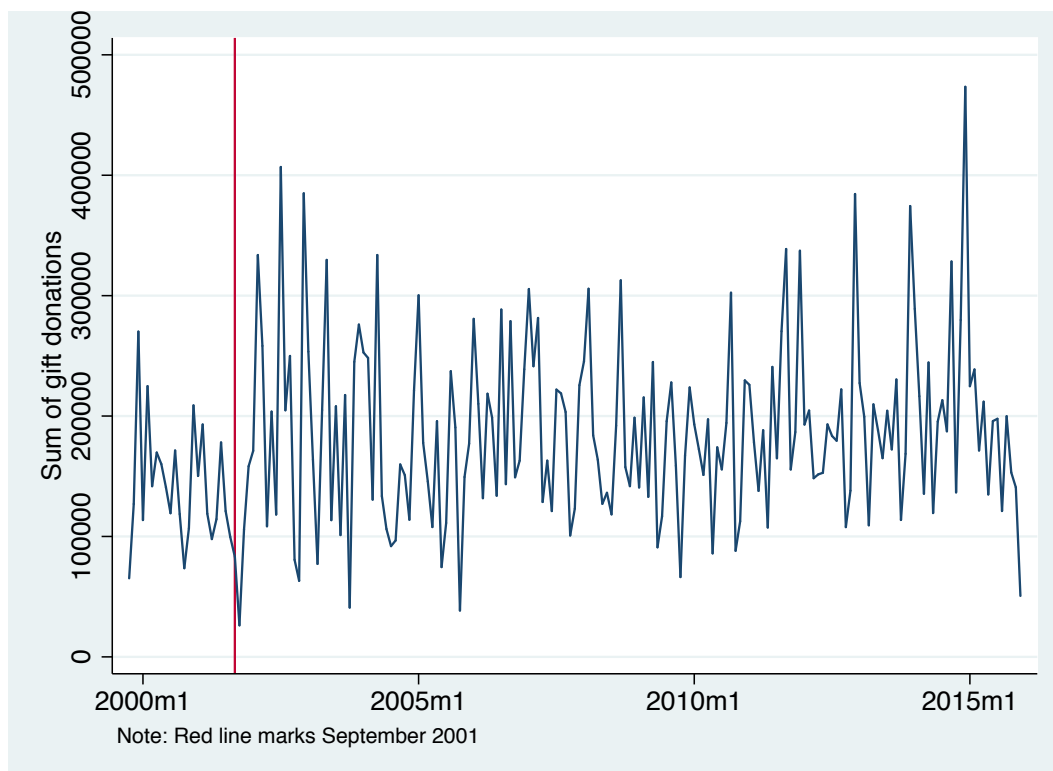


Figure 5. Total amount of monthly gift donations to the Smithsonian during 2001

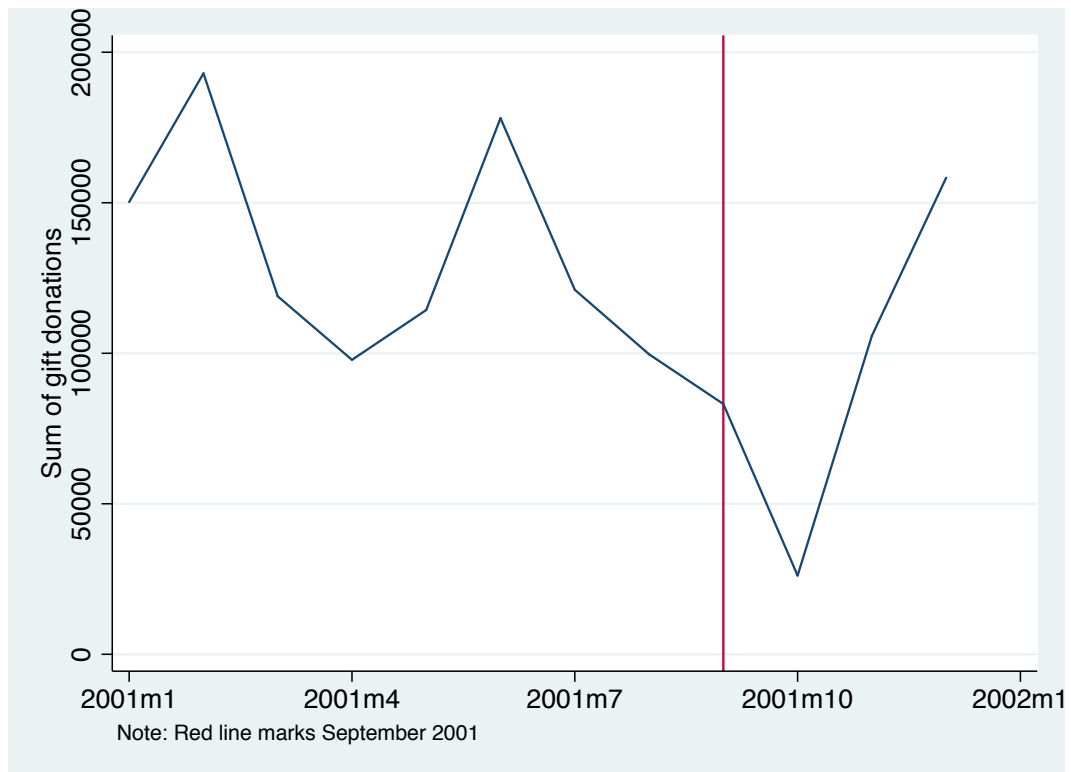


Figure 6. Average state-month gift donation per month to the Smithsonian from October 1999 to December 2015

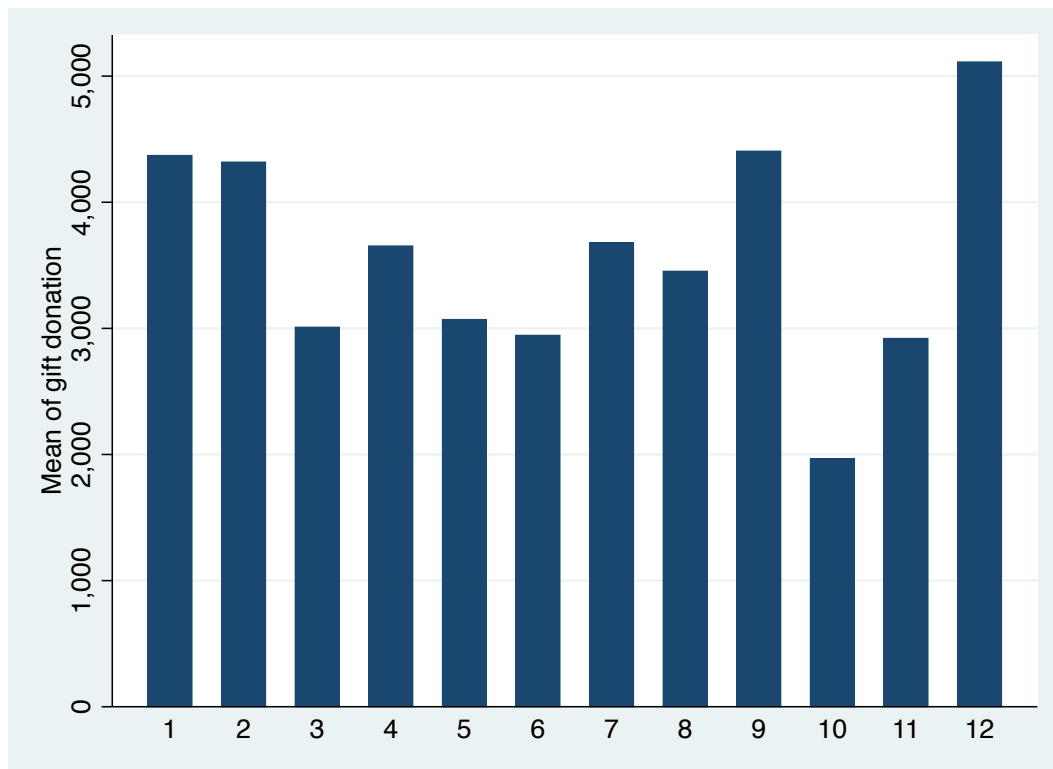
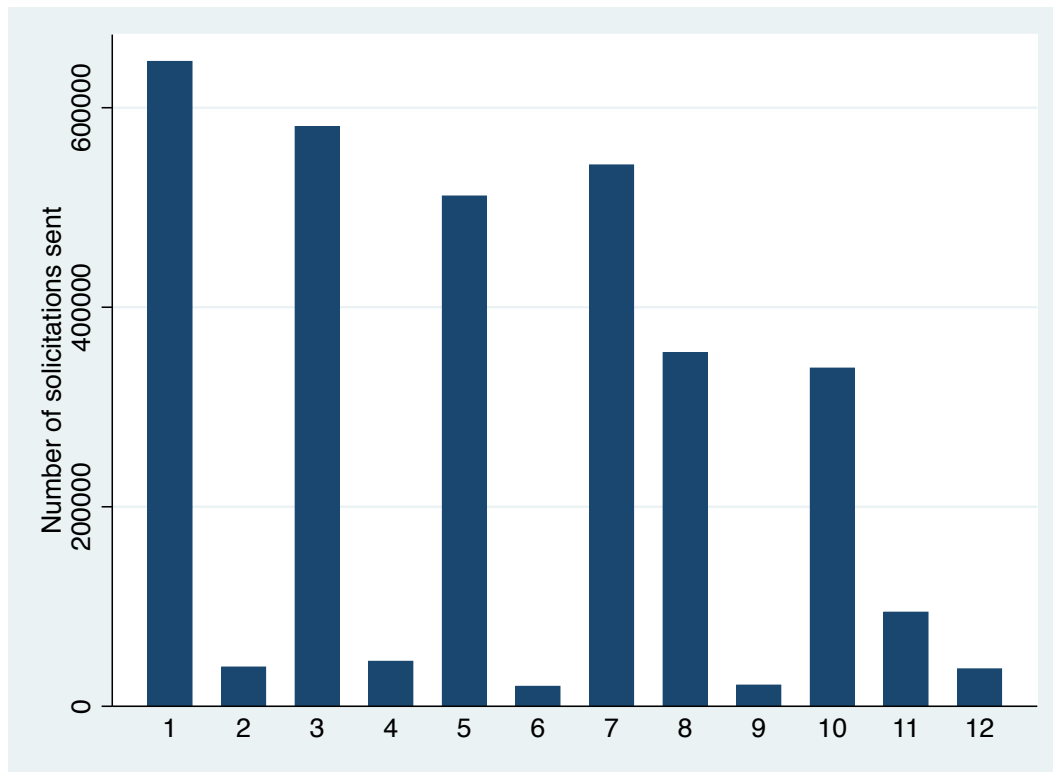


Figure 7. Total number of solicitations sent by the Smithsonian per month during 2001



B. Robustness checks

Table 5. Monthly effects of the 9/11 on donations to the Smithsonian

Results. Dependent variable: Ln(donations)	
Independent Variables	(1)
June 2001 × Directly affected	0.058 (0.092)
July 2001 × Directly affected	0.290 (0.198)
August 2001 × Directly affected	-0.165 (0.131)
September 2001 × Directly affected	-0.094 (0.238)
October 2001 × Directly affected	0.162 (0.215)
November 2001 × Directly affected	0.302 (0.203)
December 2001 × Directly affected	0.317** (0.111)
Sample	2001
Year-month fixed effects	Yes
State fixed effects	Yes
Observations	612
R-squared	0.83
Adjusted R-squared	0.81

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors clustered at state level in parentheses. The sample is restricted to include state-month observations from 2001 only.

Table 6. Monthly effects of the 9/11 on donations to the Smithsonian

Results. Dependent variable: Ln(donations)	
Independent Variables	(1)
June 2001 \times ln(victims)	-0.008 (0.027)
July 2001 \times ln(victims)	0.101 (0.087)
August 2001 \times ln(victims)	-0.059 (0.036)
September 2001 \times ln(victims)	0.008 (0.042)
October 2001 \times ln(victims)	0.051 (0.074)
November 2001 \times ln(victims)	0.092 (0.072)
December 2001 \times ln(victims)	0.054* (0.024)
Sample	2001
Year-month fixed effects	Yes
State fixed effects	Yes
Observations	612
R-squared	0.83
Adjusted R-squared	0.81

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors clustered at state level in parentheses. The sample is restricted to include state-month observations from 2001 only.

Table 7. The effect of 9/11 on donations to the Smithsonian with different measures

Results. Dependent variable: Ln(donations)

Independent Variables	(1)	(2)	(3)	(4)	(5)
Directly affected	1.833**				
	(0.298)				
2002 × Directly affected	0.033				
	(0.041)				
September-December 2001 × Directly affected	0.044				
	(0.092)				
Close to NYC		0.511			
		(0.378)			
September-December 2001 × Close to NYC		0.087			
		(0.117)			
2002 × Close to NYC		-0.019			
		(0.053)			
Bordering NY			0.744		
			(0.491)		
September-December 2001 × Bordering NY			0.112		
			(0.086)		
2002 × Bordering NY			-0.051		
			(0.043)		
September-December 2001 × Victims				0.000	
				(0.000)	
2002 × Victims				0.000	
				(0.000)	
September-December 2001 × ln(victims)					0.023
					(0.023)
2002 × ln(victims)					-0.002
					(0.013)
Sample	<2003	<2003	<2003	<2003	<2003
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	No	No	Yes	Yes
Observations	1,989	1,989	1,989	1,989	1,989
R-squared	0.34	0.18	0.19	0.85	0.86
Adjusted R-squared	0.33	0.16	0.17	0.85	0.85

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors clustered at state level in parentheses. The sample is restricted to only include state-month observations between October 1999 and December 2002.

Table 8. Baseline results of the effect of 9/11 on donations to the Smithsonian, with conventional standard errors

Results. Dependent variable: Ln(donations)

Independent Variables	(1)	(2)	(3)
After 9/11	0.208** (0.045)	0.050 (0.066)	
Directly affected	1.833** (0.113)	1.833** (0.115)	1.833** (0.113)
After 9/11 × Directly affected	0.010 (0.121)	0.036 (0.179)	
September-December 2001			-0.623** (0.109)
September-December 2001 × Directly affected			0.044 (0.293)
2002			0.274** (0.071)
2002 × Directly affected			0.033 (0.193)
Constant	6.913** (0.042)	6.913** (0.042)	6.913** (0.042)
Sample	Full	<2003	<2003
Year-month fixed effects	No	No	No
State fixed effects	No	No	No
Observations	9,945	1,989	1,989
R-squared	0.19	0.18	0.21
Adjusted R-squared	0.19	0.18	0.21

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors in parentheses. The full sample contains all state-month observations from October 1999 to December 2015. In column 2 and 3 the sample is restricted to only observations before 2003.

Table 9. Baseline results of the effect of 9/11 on donations to the Smithsonian, with conventional standard errors

Results. Dependent variable: Ln(donations)

Independent Variables	(1)	(2)	(3)
After 9/11 × ln(victims)	-0.002 (0.014)	0.004 (0.023)	
September-December 2001 × ln(victims)			0.023 (0.027)
2002 × ln(victims)			-0.002 (0.017)
Sample	Full	<2003	<2003
Year-month fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Observations	9,945	1,989	1,989
R-squared	0.73	0.70	0.86
Adjusted R-squared	0.73	0.69	0.85

* $p < 0.05$; ** $p < 0.01$

Notes: Standard errors in parentheses. The full sample contains all state-month observations from October 1999 to December 2015. In column 2 and 3 the sample is restricted to only observations before 2003.

C. Number of 9/11 victims per state

Table 10. Number of fatalities per state following the 9/11 attacks

State	Victims
New York	1747
New Jersey	694
Massachusetts	92
Virginia	81
Connecticut	65
Maryland	50
California	49
Pennsylvania	30
District of Columbia	11
New Hampshire	10
Illinois	9
Texas	6
Florida	5
Rhode Island	5
Maine	4
Georgia	4
Alabama	4
Louisiana	3
North Carolina	3
Ohio	3
Kansas	2
Delaware	2
Missouri	2
Hawaii	2
Michigan	2
Oklahoma	2
Colorado	2
Tennessee	1
Nebraska	1
Nevada	1
Mississippi	1
Kentucky	1
New Mexico	1
Arizona	1
Oregon	1
Iowa	1
Utah	1
Idaho	1
Wisconsin	1
Source: StateMaster /Website Memorial (2002)	