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Essays in Behavioral Public Economics

Adrienne Sudbury
University of Tennessee, lxx822@vols.utk.edu

To the Graduate Council:

I am submitting herewith a dissertation written by Adrienne Sudbury entitled "Essays in Behavioral Public Economics." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Economics.

Christian Vossler, Major Professor

We have read this dissertation and recommend its acceptance:

Matthew Harris, Scott Gilpatric, Michael Price

Accepted for the Council:

Dixie L. Thompson

Vice Provost and Dean of the Graduate School

(Original signatures are on file with official student records.)

Essays in Behavioral Public Economics

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Abstract

This dissertation focuses on the behavioral economics of individual decision making and consists of three separate essays. In Chapter 1, I use a laboratory experiment to compare three popular point-of-sale solicitation methods: a fixed donation request (yes or no to a randomly assigned amount); a rounding request (yes or no to an endogenous amount); and an open-ended solicitation. Further, I examine the effects of providing (limited) information on the charity. In Chapter 2, I study key aspects of fundraising campaigns that utilize goals or provision points that must be met in order to provide a good or service. I use a laboratory experiment to compare campaigns characterized by a final goal only, an intermediate goal and a known final goal, and a third setting where the final goal is known only if the intermediate goal is reached. Across these three settings, I vary whether an individual's payoff from reaching a goal is uncertain or certain, which is intended to capture the effects of providing vague or precise information on the good or service to be provided. In Chapter 3, I examine the effects of officer-involved fatalities, including officer-involved shootings, on domestic violence reporting. I conduct this analysis using county and zip code level data to understand how concentrated any effects of police violence may be. Using within-county variation, I test whether the number of domestic violence reports decreases in the week after a fatal officer-involved encounter.

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Introduction

This dissertation focuses on the behavioral economics of individual decision making. In Chapter 1, I use a laboratory experiment to compare three popular point-of-sale solicitation methods: a fixed donation request (yes or no to a randomly assigned amount); a rounding request (yes or no to an endogenous amount); and an open-ended solicitation. Further, I examine the effects of providing (limited) information on the charity. I find that, at amounts less than $1, participants in the rounding treatments were much more likely to donate. Holding fixed the amount of the ask, differences in donation rates between the rounding and fixed request treatments appear to be driven by "loose-change effects," whereby individuals are more likely to donate if they would have less change as a result. Participants in the fixed request treatments exhibited higher mean willingness-to-donate than those in the open-ended. Last, a one sentence information statement about the charity has positive but small effects on donation rates and amounts in the fixed request treatment.

In Chapter 2, I study key aspects of fundraising campaigns that utilize goals or provision points that must be met in order to provide a good or service. I use a laboratory experiment to compare campaigns characterized by a final goal only, an intermediate goal and a known final goal, and a third setting where the final goal is known only if the intermediate goal is reached. Across these three settings, I vary whether an individual's payoff from reaching a goal is uncertain or certain, which is intended to capture the effects of providing vague or precise information on the good or service to be provided. I find that the addition of an intermediate goal decreases the likelihood of reaching the final goal. Moreover, the level of the intermediate goal (holding payoffs for reaching the goal fixed) has no discernible effect on whether the intermediate or final goal is reached. This suggests a possible strategy whereby the campaign designer includes a final goal as a decoy in order to reach the desired "intermediate" goal. Value uncertainty has a negative significant effect on the likelihood of reaching the goal when only one goal is present. Finally, goal uncertainty has a positive significant effect on contributions.

In Chapter 3, I examine the effects of officer-involved fatalities, including officer-involved shootings, on domestic violence reporting. I conduct this analysis using county and zip code level data to understand how concentrated any effects of police violence may be. Using within-county variation, I test whether the number of domestic violence reports decreases in the week after a fatal officer-involved encounter. I find little to no effect of OIFs on domestic violence reporting at the county level. However, using zip code-level 911 call data from Memphis, TN, I find some evidence of decreased 911 calls for domestic violence in neighborhoods where an officer-involved fatality took place. Further, I find that total 911 calls also decrease in affected zip codes and neighborhoods. The magnitude of the effects is dependent on the race of the OIF victim and cause of death.

Chapter 1: Checking Out Checkout Charity: A Study of Point-of-Sale Donation Campaigns

Abstract

In recent years, there has been a proliferation of point-of-sale donation campaigns, and it is natural to ask what factors increase donation rates or total donations in this setting. In this study, we use an experiment to compare three popular solicitation methods: a fixed donation request (yes or no to a randomly assigned amount); a rounding request (yes or no to an endogenous amount); and an open-ended solicitation. Further, we examine the effects of providing (limited) information on the charity. We find that, at amounts less than $1, participants in the rounding treatments were much more likely to donate. Holding fixed the amount of the ask, differences in donation rates between the rounding and fixed request treatments appear to be driven by "loose-change effects", whereby individuals are more likely to donate if they would have less change as a result. Participants in the fixed request treatments exhibited higher mean willingness-to-donate than those in the open-ended. Finally, a one sentence information statement about the charity has positive but small effects on donation rates and amounts in the fixed request treatment.

Introduction

Point-of-sale donation (POS) campaigns have become an increasingly used fundraising tool. Commonly referred to as "checkout charity," these campaigns encourage people to donate at the checkout register. According to report by Cause Marketing Forum (2015), checkout charity generated more than $388 million in donations in 2014 and more than $3.88 billion over the last three decades.[1] There is much variation in the design of these fundraising efforts. Examples range from a collection box at a McDonald's service counter, to a cashier at PetSmart asking a customer if they would like to donate a specific amount (e.g., $1) to help feed hungry pets, to an electronic ask through a payment kiosk at Walmart.

Distinctive situational aspects such as the inability to avoid the ask, rapid decision time, and the amounts requested (or expected) make studying checkout charity campaigns of particular interest. In most charitable giving settings, a potential donor is exposed to two decision stages: (1) participation and (2) donation. In contrast to most settings, checkout charity campaigns rarely allow people the opportunity to avoid participation. Customers are usually caught unaware by the solicitation at checkout and are provided very few options to avoid engaging the solicitor. Furthermore, the actual donation stage is limited to a particularly short period of time (often seconds) in which the consumer must decide. This type of split-second decision-making, known as "impulse-giving", is a potential contributor to the success of checkout charity campaigns.

While there is some survey research suggesting that most consumers are agreeable to checkout charities and prefer some approaches over others (Catalist 2016), there is little research on what methods are most effective at reaching fundraising objectives (e.g., total donations or donation rates), and what behavioral mechanisms underlie donation behavior in the unique POS

[1] This only includes campaigns that raise over $1 million.

donation setting. In this study, we raise donations for a popular charity and vary as experimental treatments the donation solicitation mechanism and whether or not (brief) information on the charity is provided.

We use a controlled lab setting that captures the key characteristics of a checkout charity encounter in the field: a largely unanticipated, quick ask for a small amount of money to go towards a known charity. We compare three solicitation methods commonly used in POS campaigns: fixed donation, rounding, and open-ended. The first two, closed-ended mechanisms just present the potential donor with a yes or no decision. The main distinction is that in rounding mechanism the amount asked for is conditional on the prior actions of the donor. In the field, this rounding request is tied to the customer's bill, and the common ask is to round up the bill to the next whole dollar amount. In the experiment, we ask the participant to round down their earnings from a prior experiment to the next whole dollar amount.

In the fixed request treatment, we elicit donations for a range of amounts: 25¢, 50¢, 75¢, $1, $1.50, $2, and $3. This allows comparisons between the rounding treatments (at amounts of 25¢, 50¢, and 75¢), as well as across the distribution of donations from the open-ended treatments. In doing so, we can make apples-to-apples comparisons by, for example, comparing donations or donation rates across mechanisms while holding prices fixed. Finally, similar to Cryder, Loewenstein, and Scheines (2013), we test whether adding a short information statement about the charity impacts donations.

Our paper adds to the prior literature in many ways. To our knowledge, our study is the first to directly compare the popular solicitation mechanisms used in checkout charity. Most studies concerning charitable contributions in the literature implement a single donation solicitation under constructs such as mailouts, door-to-door campaigns, phone calls, etc.

Furthermore, these campaigns employ unstudied solicitation mechanisms such as “rounding” a bill to the next highest $1. We further study the effects of suggested amounts, information effects, framing effects, and loose-change effects in a new donation setting. The novelty of our study lies in our ability to compare these effects under the constraints of checkout charity in one comprehensive study.

In this paper, we investigate further the drivers of charitable giving. Prior theoretical and experimental research suggests that charitable giving is driven by warm glow, altruism and other social incentives. Andreoni (1990) introduced the concept of impure altruism, speculating that warm glow incentivized giving. Additionally, researchers have demonstrated that social pressure, recognition, and the approval of others all influence an individual’s donation decision (Fathi, Bateson, and Nettle 2014; List 2009; Soetevent 2005). Indeed, these peer effects create social “norms” and can affect donation behavior in both individual and group settings. Furthermore, researchers speculate that individuals also give to avoid saying “no” (Andreoni and Rao 2011; DellaVigna, List, and Malmendier 2012). These studies suggest donation choices may be influences by seen, unseen, or perceived pressures.

Similarly, research on solicitation mechanism design has greatly contributed to the literature on charitable giving. Multiple studies show that suggested donation amounts can increase charitable donations (Edwards and List 2014; Goswami and Urminsky 2016). However, the effects of these suggestions have not been tested in a setting where small, potentially negligible amounts are solicited. Additionally, providing potential donors with information about the charity can increase donations (Cryder, Loewenstein, and Scheines 2013; Goswami and Urminsky 2016). Further, the type of information given matters. In an online eBay checkout charity experiment Horn and Karlan (2018) study whether the type of charity information given during a donation request influence donation decisions, finding that certain information drivers (short mission statement) have a larger effect than others (popularity). However, the effects of a simple information statement relative to a case with no information has yet to be studied in a checkout charity setting.

We should mention other studies have focused on behavior in a setting where individuals are asked to make a quick donation decision. Most closely related to our study is a coin collection experiment by Fielding and Knowles (2015). They tested verbal cues in a laboratory experiment and found that subjects who were verbally prompted were significantly more likely to donate.[2] Of particular interest to our study, they also tested whether people were more willing to donate via coin collection if they were given smaller bills/more loose change.[3] They observed only weak evidence to support their hypothesis of "loose-change effects". However, other studies related to preferences for whole numbers lend their support. Mishra, Mishra, and Nayakankuppam (2006) and Reiley and Samek (2017), demonstrated that consumers exhibit a "bias for the whole" and preferences for round numbers. Our study contributes to a better understanding of these preferences in a checkout charity setting.

Previewing our findings, we find that, at amounts less than $1, conditional donation rates are significantly higher in the rounding treatments relative to the fixed request and open-ended treatments. Differences in donation rates between the two closed-ended mechanisms appear to be driven by loose-change effects, whereby individuals are more likely to donate if they would have less change as a result. Donation rates in the fixed request treatments are either equal to or higher than donation rates for the open-ended treatments at various amounts. This overall leads to a

[2] Here the verbal cues directed the attention of participants to the coin collection box. This created a difference of < 8% of participants donating to > 50%.
[3] The loose change treatment involved more $2 and $1 coins compared with the baseline treatment.

marginally higher mean willingness-to-donate measure. In other words, this suggests that people prefer a fixed donation ask. This could be due to either differences in social norms (e.g., the amount asked could serve as a social norm) or due to the higher cognitive burden people face with the open-ended ask. Additionally, a one sentence information statement about the charity has a positive but small effect on donation rates and amounts in the fixed request treatment.

Model

As discussed earlier, the decision time for checkout charity solicitations is relatively short. However, different solicitation mechanisms could have differential effects on decision cost during this short time period. For example, in the rounding solicitation, a potential donor is faced with a binary donation decision, yes or no. However, in the open-ended solicitation, a potential donor is faced with two decisions: whether to donate, (yes or no); and if so, how much to donate. Therefore, with two decisions, an open-ended solicitation may increase the cognitive burden of donating in the form of increased decision cost relative to a binary choice solicitation.

To illustrate this and other differences in solicitation mechanisms, we adapt a model similar to that of DellaVigna, List, and Malmendier (2012). With this model, we demonstrate the effects of decision cost and information across different solicitations.

Closed-ended Solicitations

We begin with the simplest case, a closed-ended or binary decision solicitation with no information about the charity. This case is descriptive of both a fixed amount request and a rounding request. In its simplest form, a donation decision involves a tradeoff between the utility derived from giving, such as warm-glow and altruism, and the disutility from the resulting decrease in wealth. However, there is also a social cost embedded in the donation function, meaning not donating could have social consequences, generating negative utility. Due to factors such as peer pressure and social norms, giving less than the socially optimal amount (including $0 donations) places an additional burden on the donation decision of a potential donor.

This relationship between the costs and benefits of donating is characterized by the following utility function:

$$(1) \qquad U(g) = u(W - g) + av(g, G_{-i}, h) - s(g)$$

A consumer with initial wealth W will choose to donate an amount $g \geq 0$ to a checkout charity campaign. We assume that the "private" utility function $u(\cdot)$ is concave, and that utility increases with wealth and decreases in the amount donated.

The function $v(\cdot)$ is the utility derived from charitable donations, which depends on the amount the individual donates (g), as well as the giving of others (G_{-i}). We further assume that utility is a function of the donor's knowledge of the charity (h), such as the information they have on what activities the charity engages in. We assume the function is monotonically increasing, and concave in g, with $v(0, G_{-i}, h) = 0$. Thus, an agent only derives utility from giving if they give some amount $g > 0$. Here, the parameter a represents the level of altruism. In the case of pure altruism, the individual cares about the total contributions to the charity, $G_{-i} + g$, meaning the overall utility from giving equals $av(g + G_{-i}, h)$.[4] However, altruism can be impure, meaning an individual cares about the warm glow from giving g. In this case, the

[4] As in DellaVigna, List, and Malmendier (2012), here a also has the ability to capture the belief a donor has about the quality of the charity.

parameter a captures the intensity of warm glow. Note that it is possible for an individual to have both pure and impure altruistic motivations for giving; meaning a captures the relative utility gained from total contributions and individual warm glow.[5]

The function $s(g)$ represents the social cost function generated from social pressure and social norms where $s(g) = S * (g^s - g) \cdot 1_{g<g^s} \geq 0$. The severity of the social cost to an individual is represented by the parameter S. Thus S is a parameter representing the relative social pressure a potential donor feels. Here, social cost is born for a donation g less than the socially optimal amount or social norm g^s or ($g < g^s$). In the closed-ended treatments, we assume that the socially optimal amount to give is the solicited amount. Therefore, by giving in a closed-ended solicitation, an individual's donation will always be equal to the socially optimal amount, or $g = g^s$. Thus, only donations of $g = \$0$ elicit some social cost.

When we introduce information about the charity in the solicitation, we assume that the information would have a positive effect on the utility of donating, i.e. the more a donor knows about where their money is going or how it might be used, the greater the utility of donating. This then alters h, and we hypothesize that this increases $v()$, at least for those who know very little about the charity.[6] This assumption is in line with previous experimental findings related to information in charitable donations. Therefore, an individual is more likely to donate when information about the charity is present.

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[5] Assumptions related to the altruism parameter, a, are directly taken from the model in DellaVigna, List, and Malmendier (2012). Also, in their paper a, can also be less than zero, if individuals give out of spite. In this case, the $av(\cdot)$ would represent the disutility of giving.

[6] As in DellaVigna, List, and Malmendier (2012), here a also has the ability to capture the belief a donor has about the quality of the charity.

Given the up or down choice presented with a closed-ended solicitation, the individual optimally donates the amount requested, g^s, if the utility from doing so is equal to or greater than the utility of not donating:

$$(2) \qquad u(W - g^s) + av(g^s, G_{-i}, h) - s(g^s) \geq u(W) + av(0, G_{-i}, h) - s(0)$$

Open-ended Solicitations

For an open-ended solicitation, individuals have the freedom to choose any donation amount. While this might be beneficial if an individual's underlying willingness to donate is greater than a fixed amount solicitation, the solicitation itself might induce more costs to donation. For example, in this scenario, an individual must expend cognitive effort to determine what amount they would like to give. This effort may increase or decrease depending on their familiarity with their own willingness to donate and the socially optimal amount to give. We incorporate this cognitive cost as a function, $d(\cdot)$, as shown in equation 3.

$$(3) \qquad U(g) = u(W - g) + av(g, G_{-i}, h) - s(g) - d(\sigma)$$

In this case, the donation, g, is decided by the individual. For the social pressure function, $s(g)$, this means that contributing does not necessarily result in zero social pressure costs. Now, giving some amount less than the socially optimal donation, g^s, might be seen as socially undesirable, resulting in some social cost to the individual. Thus, even a positive contribution where $g^* < g^s$ will result in some social pressure cost in this utility function. The farther away g is from the socially optimal amount, g^s, the larger the social cost to the individual.

Additionally, the socially optimal amount to give might not be known to the individual. It is possible that an individual misestimates g^s due to uncertainty and donates less than the actual socially optimal amount. Thus, there is some positive probability that an individual might pay some social cost even if they donate $g^* = E[g_{-i}]$ or $g^* = E[g^s]$. However, an individual can reduce the likelihood of misestimating the socially optimal amount by paying some effort cost, d.

In equation 3, the effort cost function, $d(\sigma)$, represents the cognitive burden of deciding on donation, g^* whereby an individual may expend effort, increasing d, to better estimate the socially optimal donation g^s, lowering the probability of paying $s(g)$.[7] This function increases with the variance, σ, of the socially optimal amount g^s. Meaning, if the variance of the socially optimal amount to give is sufficiently high, more cognitive effort is required to estimate a socially optimal amount to give.

Utility Maximization

To maximize utility in donation decision-making, an individual will first decide if he or she will donate at all. With an open-ended solicitation, an individual knows that by choosing to donate, they will bear additional decision cost, d. Therefore, the individual chooses whether or not to donate by calculating their net utility in both cases.

If he or she does not donate:

$$g = 0 \text{ and } g \neq g^s$$

[7] Thus, $g^s = E(g_{-i})$ with variance σ, or the expected value of others' donations (i.e. the social norm).

$$\text{Thus, } s(0) > 0$$

$$U(0) = u(W) + av(0, G_{-i}, h) - s(0)$$

If he or she does donates:

$$g > 0$$

$$\text{Thus, } s(g^*) = \ S \ * (g^s - g^*) \cdot 1_{g^* < g^s} \geq 0$$

$$U(g^* > 0) = u(W - g^*) + av(g^*, G_{-i}, h) - s(g^*) - d(\sigma)$$

Comparing these two utility functions, if $U(g > 0) > U(0)$, then the net utility of donating is greater than that of *not* donating and the individual will donate. If $U(g > 0) < U(0)$, then the net utility of donating is less than that of abstaining. Thus, the individual will not donate. Note that in the case where a person gives, the difference between closed-ended and open-ended utility functions is the potential social cost and effort cost, $s(g)$ & $d(\sigma)$.

Comparison of Closed versus Open-ended Solicitations

Comparing the two solicitation mechanism's utility maximization, the utility of donating g^* is greater in the closed-ended solicitation (left) than the open-ended solicitation (right) due to the inclusion of potential social cost and decision cost. At any amount g^*, an individual will be more likely to donate under a closed-ended mechanism versus an open-ended mechanism. This assumption holds as long as $d(\sigma) > 0$, or decision cost is non-negative. We use this model to derive testable hypothesis based on our experimental design.

(4) $$u(W - g^*) + av(g^*, G_{-i}, h) > \ u(W - g^*) + av(g^*, G_{-i}, h) - s(g^*) - d(\sigma)$$

Experimental Design

Within the constraints of the experimental laboratory, we designed an experiment to test a variety of methods that parallel those commonly used in real checkout charity campaigns. We examine three solicitation mechanisms, which we label as Fixed Request, Rounding, and Open-ended, under two information conditions.

We decided on the first two solicitation methods (Fixed Request and Rounding) due to their perceived popularity. In a consumer survey report released by Catalist (2016), the "add $1" (a fixed donation request) was the most preferred method of donation at the register at 46 percent. Following close behind was the rounding method with 23% of consumers preferring it. While those surveyed did not express a preference for an open-ended ask, this approach is nevertheless commonly used, especially when the solicitation is through a POS kiosk where it is simple for consumers to freely enter a donation amount.[8] We decided to add an Open-ended donation mechanism for two reasons: (1) to test whether a closed-ended ask elicits higher donation rates than an open-ended ask as our theory suggests, and (2) to account for the possibility that our requested amounts might be too low or too high for this setting.[9] For the Fixed Request, we decided to test a variety of donation amounts, 25¢, 50¢, 75¢, $1, $1.50, $2.00, and $3.00. With these donation amounts we are able to establish reference points for a comparison to the Rounding mechanism at 25¢, 50¢, and 75¢. This allows us to better study potential framing effects and/or loose-change effects between Fixed Request and Rounding treatments.

[8] A related approach is one where customers have the option to select one of several possible donation amounts or instead enter an amount of their choosing.
[9] The Open-ended treatments were added after we ran the first session.

In an effort to expand our investigation of checkout charity methods, we further test an information component. Under our baseline or no information setting, individuals only receive the name of the charity. This reflects common practice for POS donation campaigns, and likely reflects a compromise for businesses who want to raise money while minimizing impacts on customers (e.g., reducing transaction speed). Nevertheless, adding information about a charity has been shown to increase donations. Secondly, among consumers reporting no donations at the register, "not knowing much about the cause asking for money" was the number one cause for declining to donate (Catalist 2016).[10]

Based on the literature, we derived a single descriptive sentence indicating specific uses for monetary donations to provide the additional charity information customers might desire. Specifically related to St. Jude Children's Research Hospital, we gathered information about donation usage from their website and formed the following informational sentence: "Through donations, St. Jude's patients (children) receive care, treatment, and cutting edge research, at no cost to their families". Therefore, whether a subject received a donation prompt with or without information determined the final set of treatments. Table 1.1 shows the finalized mechanisms and treatments.[11]

While reflecting on our subject pool (college students), we decided to choose St. Jude Children's Research Hospital as the recipient charity for two reasons. First, St. Jude's is well known and one of the highest grossing charities. We feared that choosing a lesser-known charity would create problems of nonrecognition and induce exceptionally low donation rates. Second, we also chose St. Jude's because the research hospital has been involved in many checkout

[10] 28% of respondents listed charity brand recognition as the number one reason for giving (2015 Americas Charity Checkout Champions).
[11] All tables and figures for Chapter 1 are located in Appendix A.

charity campaigns over the years. Using a charity that already engages in checkout charity is particularly useful in this setting.[12]

Subsequently, the names of all six treatments are as follows: Fixed Request (*FR*), Fixed Request Info (*FRI*), Rounding (*R*), Rounding Info (*RI*), Open-ended (*OE*), and Open-ended Info (*OEI*). With this design, we can make direct comparisons between solicitation mechanisms to determine which solicitation is the most effective.

Hypotheses

To compare effects, we merge our model with existing theory and common findings in the literature and form six testable null hypotheses:

Hypothesis 1: Fixed-Request mean donations will equal Open-ended mean donations.

Hypothesis 2: Closed-ended mechanism donation rates the same as Open-ended mechanism donation rates.

Hypothesis 3: Potential donors provided with charity information will donate at the same rate and amount than those not provided with information.

Hypothesis 4: Donation rates are constant as the donation amount solicited increases.

Hypothesis 5: Potential donors who can reduce coinage by donating will give at the same rate as those who would have more (or equal) change.

Hypothesis 6: The willingness-to-donate distributions of the Fixed Request and Open-ended treatments are equal.

[12] Participants might see the solicitation as more legitimate if they have experienced something similar previously.

Here, we use the theoretical model to speculate possible behavioral drivers that would lead to rejections of the above hypotheses. For Hypothesis 1 (H1), a rejection might occur if donors' underlying willingness-to-donate is much higher than the amounts solicited in the Fixed Request treatment. Additionally, a rejection might occur if potential donors over or underestimate the socially normative donation amount relative to the donation amounts in the Fixed Request treatments. For Hypothesis 2, based on the theoretical model discussed previously, it is possible that the lack of a value cue in the Open-ended treatments imposes some cognitive cost, reducing the incentive to donate. This is especially true if the variance of the social norm is quite large, causing donors to expend more effort or misestimate the social norm. For example, it is possible that the social norm in the Open-ended treatments is perceived to be 0. This would, in turn, alleviate any social pressure and give rise to lower donation rates. If potential donors are unsure of either the socially acceptable donation amount or whether they should donate at all, they might opt out of donating, rather than exert cognitive effort.

Testing Hypothesis 3 allows us to determine if information affects donation rates and mean donations. Based on outcomes related to the theory model and previous information studies mentioned earlier, we expect that introducing information about a charity increases both donation rates and amounts. Both the brevity of the information provided and the particular charity we use may alter the magnitude of this effect relative to those found in previous studies.

Turning to Hypothesis 4, we expect that donation rates will decline as the specific donation requested increases, given that the marginal utility of wealth is decreasing and the marginal utility of giving is increasing with the donation amount. For Hypothesis 5, we can look for possible "loose change effects," testing whether individuals who can reduce coinage by donating give at higher rates than those who would increase coinage by donating. This hypothesis is best tested with the Fixed Request treatment. We cannot use this test for the Rounding mechanism because every participant randomized into this mechanism will automatically reduce coinage by donating. For the Open-ended mechanism, we can compare the percentages of donors who self-rounded down their earnings (gave themselves less change by donating) to those who did not.

Finally, for Hypothesis 6, we want to examine whether the underlying willingness-to-donate distributions are similar between the Fixed Request and Open-ended treatments. By extending the amounts solicited in the Fixed Request treatments beyond $1, we are able to make distributional comparisons between the two treatments.

Experimental Methods

The data was collected via laboratory sessions at the UT Experimental Economics Laboratory in two stages: 1) Summer and Fall of 2017 and 2) Fall of 2019.[13] All participants were currently enrolled undergraduate students at the University of Tennessee. All decisions were made on personal computers using software programmed in z-Tree (Fischbacher 2007). This experiment was a tag-a-long, joint experiment with four unrelated economics experiments. In the related experiments, subjects earned both a show-up fee as well as earnings based on incentivized decisions. Subjects' earnings were directly tied to their performance in the experiment and are therefore considered earned income. After viewing their earnings at the end

[13] Data collection took place in two stages. Analysis of the initial data revealed exceptionally high donation rates for the Fixed Request and Rounding treatments as well as possible differences in distributions between closed and open-ended treatments. As we were going to collect additional data anyways, this prompted us to add additional amounts in the Fixed Request treatment beyond $1, providing us with better comparisons between the Rounding and Fixed Request treatments at $0.25 and $0.75.

of the first experiment, subjects were then prompted to donate a portion of their earnings to St. Jude Children's Research Hospital.[14] Figure 1.1 shows what subjects would have seen in the Fixed Request mechanism without information.

As shown, subjects were able to select the option of "Yes" or "No thanks." The Fixed Request donation screen with information would appear with the same wording but include the informational sentence after the donation prompt. As for the Rounding mechanism, in a real checkout charity situation, customers would normally be prompted with a statement such as, "would you like to round up your purchase of $19.75 to $20.00 and donate $0.25 to St. Jude Children's Research Hospital today?" However, because our subjects did not make purchases, but rather earned money, we asked them if they would like to round *down* their earnings instead.[15] On the donation screen, subjects were given information about their earnings from that session and the following prompt: "Would you like to round down your earnings to the nearest whole dollar by donating $0.XX to St. Jude Children's Research Hospital?"

As stated earlier, subjects' earnings were a direct reflection of performance in the preceding experiment. Earnings were rounded to the nearest quarter to limit donation asks to increments of 25¢. Thus, collected data involves asks of $0.25, $0.50, and $0.75, which facilitates well-powered comparisons with the other mechanisms.[16]

In an effort to gather feedback on participant donation decisions, we asked a follow-up question, contingent on their response to the donation prompt, on the next screen. Those who donated were asked to disclose their reasons for giving via the following question: "Why did you

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[14] Treatment was randomized at the individual level.
[15] While rounding down may not be equivalent to rounding up in a purchase setting, our findings still suggest that there are important differences in donation rates using this mechanism that might be explored further in a purchase setting. We discuss this further in the conclusion of this paper.
[16] In one of the experiments, earnings prior to the ask were in 5-cent increments. This yielded 10 observations where people were asked amounts other than 25, 50, or 75. These observations are excluded from the data analysis.

chose to donate a portion of your earnings to St. Jude Children's Research Hospital? **(Please check all that apply)**". Subjects were allowed to check one or many of the suggested reasons. Additionally, we provided an open-ended comment box where they could input their own answer. Figure 1.2 displays the options presented to subjects on the screen.

Likewise, subjects who decided *not* to donate were given the prompt: "Why did you choose not to donate a portion of your earnings to St. Jude Children's Research Hospital? **(Please check all that apply)**". Options presented to these subjects are displayed in Figure 1.3.

Finally, subjects completed a brief questionnaire in which demographic characteristics were collected. In the questionnaire, one last follow-up question was presented. We asked subjects "Did you enjoy being asked to donate a portion of your earnings to charity?" with response options "Yes", "No", and "Indifferent".

At the conclusion of the experiment, subjects were shown a letter that would be accompanying their donation to St. Jude Children's Research Hospital. In the letter, we provided information about where the money was coming from, how it was collected, as well as a statement confirming that we would not be using these donations for our own tax purposes. A copy of this letter can be found in the appendix.

At no time were subjects made aware about the donation portion of the experiment in any of the instructions. This allows us to better study reactions to an unexpected, quick-decision situation similar to checkout charity. Finally, subjects were encouraged to email the experimenters with any questions they might have about the validity of their donation.

Overview

A total of 906 students participated in the study. Table 1.2 shows descriptive statistics. 57% of the subjects were male and 42% female. The average donation rate across all treatments was approximately 49%.[17] In total this experiment collected $377.50 in donations for St. Jude Children's Research Hospital. Subject's earnings ranged from $6.75 to $36.75.[18] Overall, average earnings across all sessions were $22.79 and the mean age 20. Donations across all subjects averaged $0.42, while donations among contributors averaged $0.85.

Figure 1.4 illustrates donation rates by mechanism. For the two closed-ended mechanisms (Fixed Request and Rounding), the numbers simply reflect the percentage of "yes" responses at each dollar amount. These rates represent the lower bound of willingness-to-donate at the specific donation amount. For the Open-ended treatment, presented is a discrete version of a survival function, which reflects the percentage of respondents that donated at least a particular amount, calculated at amounts used in the Fixed Request treatments. This allows us to make an apples-to-apples comparison with closed-ended mechanisms.

As expected, donation rates generally decline as the donation asked increases. Visually, the Rounding treatment had the highest donation rates at donation requests less than $1.[19] In comparison, donation rates for the Fixed Request treatment are somewhat lower than the Rounding for asks at $0.25 and $0.50, but much lower at an ask of $0.75. Donation rates for the Fixed Request treatment continue to decline at amounts greater than $1. For amounts less than $1, donation rates in the Open-ended treatment are lower than that of both closed-ended

[17] This calculation is using the raw data not taking into account the average amount solicited between mechanisms.
[18] The range of earnings was determined by the four unrelated experiments which varied in length (30-90 min). Earnings are correlated with performance and session duration.
[19] Tests confirming the significance of this relationship across treatments can be found in the Appendix.

treatments (except at \$0.75). However, donation rates between the Fixed Request and Open-ended mechanisms are nearly equal for donations amounts greater than \$1.

Table 1.3 provides a breakdown of outcomes by treatment that are *unconditional* on the amount requested (if any). The Rounding treatment without information had the highest donation rate at 82.02%. Performing simple parametric t-tests and pooling observations by solicitation mechanism, we find that the difference in donation rates between the Fixed Request and Rounding treatments (-39.57%, $p < 0.000$) is statistically significant. Even if we limit observations in the Fixed Request treatment to solicitations less than \$1, the difference in donation rates is still statistically significant (-22.66%, $p < 0.000$). Additionally, t-tests between the Open-ended and Rounding treatments reveal a significant difference in donation rates (38.37%, $p < 0.000$).

The Open-ended treatments produced the highest mean donations across all participants and all contributors. However, this was conditional on being allowed to donate any amount. In the two closed-ended treatments, participants were only allowed to give the requested amount. Again, we pool observations by mechanism and use parametric t-tests of means to determine if differences in donation amounts between mechanisms are significantly different. We find differences in donation amounts are statistically significant for comparisons between the Fixed Request and Open-ended treatments (-\$0.42, $p < 0.000$) and the Rounding and Open-ended treatments (-\$0.42, $p < 0.000$).

The summary statistics suggest that information has a positive effect on donation rates in the Fixed Request and Open-ended treatments. However, the effect is in the opposite direction for the Rounding treatment. Parametric t-tests of means reveal only significant difference in donation rates due to information occurs in the Fixed Request mechanism (-8.23%, $p < 0.04$).

Additionally, information seems to have had a positive effect on donation amounts in the Fixed Request treatment. This difference is 9¢ and is statistically significant ($p < 0.03$).

Donation Rates

To gain additional insights, as well as to control for other factors that may also be driving differences across treatments, we estimate ordinary least squares regressions. The dependent variable, *Gave*, is an indicator of whether a participant donated. In model 1, the included explanatory variables are a set of treatment-specific indicator variables. In model 2, we add to the specification experimental earnings, age, gender and whether they had recently donated to charity. the Open-ended treatment without information (T3) as our baseline. These regressions are presented in Table 1.4.

Participants in the Rounding treatment without information donated at a rate 44.1 percentage points higher than those in the Open-ended treatment. Coefficients on both the Rounding indicator variables are statistically different from the coefficients for the Fixed Request indicators. Additionally,

In model 2, we include additional control variables to establish whether the results in model 1 are robust including experimental earnings, age, gender and whether participants had recently donated to charity. Coefficients for all indicator variables remain similar and significant. However, *Earnings* and *Recent* are also significant. For every dollar increase in experimental earnings, a participant's donation rate increased 1.7 percentage points. However, participants who indicated they had recently donated to charity were 10.4 percentage points less likely to donate.

However, these regressions are not accurate comparisons of treatments due to differences in amounts asked. The Rounding treatment is limited to asks of \$0.25, \$0.50, or \$0.75 while the Fixed Request ask range between \$0.25-\$3. Finally, there are no bounds in the Open-ended treatment. Thus, a simple regression of treatment indicators is not an accurate comparison between treatments.

In Table 1.5, we provide a comparison between all three treatments at amounts less than \$1 (i.e. \$0.25, \$0.50, \$0.75). So as to make data from the Open-ended treatments comparable, we randomly assigned an ask of \$0.25, \$0.50, or \$0.75 to each participant and then recorded yes/no responses based on whether the actual amount given is at least as high as the randomly assigned amount.

Model 1 shows a simple regression using only treatment indicators on *Gave*, an indicator of whether or not a participant donated. The baseline treatment is the Open-ended without information. From this model, the Rounding mechanism yields the highest donation rates and the open-ended mechanism the lowest. Differences between either closed-ended mechanism and the open-ended mechanism are rather stark, with a 42.2 percentage point difference for the Rounding mechanism and 19.6 percentage point difference for the Fixed Request. Both of these differences are statistically significant ($p < 0.01$). Similarly, based on coefficients between the two treatments, participants facing the Rounding mechanism donate 22.6 percentage points more often than those in the Fixed Request setting, and this difference is also statistically significant ($p < 0.01$). The coefficient on *Information* is not significant, indicating information had no effect across all treatments.

Model 2 expands upon Model 1 to control for the amount asked; in particular, we include indicators for \$0.50 and \$0.75, making the baseline ask \$0.25. Treatment coefficients are similar in magnitude and significance. In addition, the indicator for an ask of $0.75 is negative and significant. In particular, participants asked to donate $0.75 donated 14.4 percentage points less often than those asked to donate $0.25.

Model 3 incorporates demographic control variables. Coefficients on treatment and price indicators are similar to models 1 and 2. For the demographic controls, coefficients on experimental earnings and whether a participant had recently donated to charity are both significant. An $1 increase in experimental earnings leads to a 1.6 percentage point increase in the likelihood of donating across all treatments. However, if a participant indicated that they had recently donated to charity, they were 7.2 percentage points less likely to donate.[20]

From the results in Table 1.4 we can conclude there are significant differences in donation rates between all three treatments at amounts less than $1. Participants randomized into the Rounding treatment were much more likely to donate than those in the Fixed Request treatment. Similarly, participants randomized into the Open-ended treatment were much less likely to donate than those randomized into the Fixed Request treatment. Thus, we can reject the null of hypothesis 2, conditional on ask being less than $1; participants are more likely to donate under a closed-ended solicitation versus an open-ended solicitation.

Because the amounts solicited are held fixed in the above analysis, the difference in donation rates between the two closed-ended treatments could be due to two possible factors: framing effects and/or loose-change effects.[21] If the difference were driven by framing effects, participants would be more inclined to donate based on the way the question is asked (rounding versus fixed request). However, if participants are more inclined to donate because they have a

[20] Participants were asked about recent donation after the solicitation. Therefore, this relationship is not necessarily causal.

[21] As noted in Table4, comparisons of donation rates between the Fixed Request and Rounding treatments only include observations where the requested amount is either $0.25, $0.50, or $0.75.

preference for whole numbers (or less/no change), this could explain the difference in donation rates between the Rounding and Fixed Request mechanisms.

Loose-change Effects

To determine whether donations were motivated by “loose-change effects,” we create a dummy variable to indicate whether donating the suggested amount would increase or decrease a subject’s amount of change. For example, if a subject earns $16.75 for the session and is prompted to donate $0.50, donating will decrease the subject’s amount of change relative to not donating. Therefore, significant differences in donation rates relative to whether subjects increase or decrease their change through donation is indicative of loose-change effects.

To determine if “loose-change effects” significantly affect donation rates, we examine each mechanism individually. For the Fixed Request mechanism, we divide individuals into two groups, those who would receive less change by donating and those would receive more change by donating. Then, we perform a chi squared distribution test for donation rates. As displayed in Table 1.6, the average donation rate is only 38.78% for those who would receive more change by donating whereas the average donation for those who would receive less change by donating is 81.51%. Notice that donation rate for those in the Fixed Request treatment who would receive less change by donating is very similar to the donation rate of 79.47% in the Rounding mechanism.

Additionally, we look at loose-change effects in the Open-ended treatments by dividing subjects into three groups: those who gave themselves less change by donating, those who gave themselves equal change by donating, and those who gave themselves more change by donating.

78 percent of subjects that donated gave themselves less change, 22 percent gave themselves equal amounts of change, and 0 percent gave themselves more change.

In addition to these tests across mechanisms, we also find anecdotal evidence of loose-change effects from the comment section in the post experimental survey. Participants left comments such as “I didn’t want a quarter” and “I don’t like change anyways,” indicating that loose change effects had an impact on their donation decision. With this evidence, we conclude that loose-change effects, rather than framing effects, are driving the significant difference in donation rates across the two closed-ended mechanisms.

Donation Amounts

We now begin our analysis of treatment effects on donation amounts. Using an similar specification to that used in Table 1.4, we regress treatment indicators on donation amounts. We use ordinary least squares estimation with Donation Amount as our dependent variable and the Open-ended treatment without information (T3) as our baseline.

Model 1 in Table 1.7 shows this regression. All coefficients on indicators for the Rounding and Fixed Request treatments are statistically significant. Participants in the Fixed Request treatment donated \$0.45 to \$0.54 less on average than those in the Open-ended treatment without information. Similarly, participants in the rounding treatments donated \$0.42-\$0.44 less than those in the Open-ended treatment without information. The differences between the two coefficients on the Fixed Request treatment indicators and the Rounding treatment indicators are statistically significant (all $p < 0.003$). There are no significant differences between the Fixed Request treatments with and without information as well as the Rounding treatments with and without information.

Model 2 shows the same indicator variables but also includes controls for experimental earnings, age, gender, and whether participants had recently donated to charity. Results are similar to Model 1 with respect to indicator variable coefficient values and significance. In addition, experimental earnings have a positive significant effect on donation amount, meaning an increase in experimental earnings by $1 translates to increase of $0.02 in donation amount. Further, if a participant indicated that they had recently donated to charity, their average donation was $0.08 lower than those who indicated they had not recently donated.

However, this is not an accurate comparison between treatments as donation amounts were limited to less than $1 in the Rounding treatment and between $0.25 and $3 for the Fixed Request treatment. To get a better idea of differences in donation amounts across bounded and unbounded solicitations, we estimate participants' willingness-to-donate.

Willingness to Donate

The experimental design includes a large range of ask amounts for the Fixed request treatments. As suggested by the prior analysis, while we can compare the donation rates for the Fixed request and Open-ended mechanisms at each amount this is an inefficient way to proceed. We can instead estimate willingness-to-donate (WTD) distributions, which effectively means that we fit curves to the data presented in Table 1.3. By doing so, we can estimate measures of central tendency, e.g., mean WTD. This mean WTD can be interpreted as the dollar amount an average person would have donated under the Fixed Request mechanism, absent the constraints imposed by asking for a particular dollar amount.

To estimate WTD distributions, we use standard approaches from the broader literature that uses binary choice data to undertake welfare analysis (e.g., Cameron and James, 1987). In particular, we employ the interval regression estimator, which accommodates our mix of continuous (Open-ended) and binary censored (Fixed request) data. In the Fixed Request treatment, we obtain either an upper or lower bound on an individual's WTD. For participants who donated, the lower bound is represented by their donation amount with an unknown upper bound (right-censored). For participants who chose not to donate, the amount solicited is the upper bound while the lower bound is $0 (left-censored).[22] In our estimation, we assume that WTD is non-negative, which is logical given our setting. In the Open-ended treatment, we know the exact upper and lower bounds as they are equal to one another and represented by the chosen donation amount (continuous).[23] Using these upper and lower bounds, we jointly estimate WTD distributions for both treatments shown in Table 1.8. In doing so, we allow for differences in both the means and the variances across mechanisms.

Mean WTD for participants randomized into the Fixed Request treatment is $1.02 (Model 1) and mean WTD in the Open-ended treatment is $0.82 (Model 1). As mean donations from the Open-ended mechanism are $0.83 and $0.81 without and with information, respectively, based on the "raw" data (see Table 1.2), this provides some evidence of a reasonable model "fit". Model 2 incorporates additional control variables for whether treatments contained information about the charity and participants earnings, age, gender, and whether they had recently donated to charity. Model 2 shows a significant difference in mean willingness-to-donate between the Fixed Request and Open-ended treatments. Mean WTD for the Fixed Request treatment is similar to Model 1 at $1.04 while mean WTD in the Open-ended treatment

[22] We assume that willingness-to-donate is non-negative. Thus, the lower bound for donations is 0.
[23] We do not include the Rounding treatment in this estimation for two reasons: 1) the donation asks are small and have little range in order to estimate willingness-to-donate and 2) the donation rates across these limited asks are relatively similar.

is $0.79, a difference significant at the 10% level. Further, including information about the charity has a positive significant effect on WTD, increasing WTD by $0.12 at the mean. Participants' experimental earnings had a positive significant effect on WTD, while reported recent donations to charity had a negative significant effect on WTD.

In both models we allowed the variance of the two treatments to be different. As shown in Table 1.8, the variance terms of both treatments are significantly different from one another, with the Open-ended treatment having a much higher variance in donations than the Fixed Request treatment. This could be because of decision errors tied to the complexity of the task. Without a reference point or suggested donations, the variance in donations is much higher in the Open-ended treatment. Therefore, we can reject the null for hypothesis 5, as we find significant differences in both the mean and variance of willingness-to-donate between the Fixed Request and Open-ended treatments.

Information effects

While the results in Table 1.8 show that information had a significant effect on donation amounts across all treatments, we might be interested in whether information has a positive significant effect in every treatment. We use t-tests to generate pairwise comparisons of average donations between treatments with and without information as shown in Table 1.9. It appears that the effect observed in Table 1.8 is primarily driven by the Fixed Request treatment. t-tests are positive and significant for Fixed Request comparison. The addition of information under a Fixed Request solicitation led to a significant increase in donations of $0.09.

We further examine whether information had an effect on donation rates. Thus, also shown in Table 1.9 are pairwise comparisons of donation rates between treatments with and without information. T-tests are positive and significant for both the pooled and Fixed Request comparisons. Similar to donation rates, participants randomized into the Fixed Request treatment with information gave 8.23 percentage points more often on average than those randomized into the treatment without information. There are no significant effects of information in the pooled case or other treatments related to donation rate.

As a robustness check, we regress information and treatment dummies on our two outcomes of interest, *Donation Amount* and whether a person donated (*Gave*). Tables 1.10 and 1.11 display these regressions respectively. Model 1 begins with a simple regression of an information dummy on Donation Amount. The coefficient on information is not statistically significant. Model 2 incorporates treatment indicators and interactions with the information dummy variable. Neither of the interactions with information are statistically significant. However, both indicator variables for Fixed and Rounding treatments are statistically significant. Finally, model 3 includes demographic controls. Like Table 1.7, experimental earnings have a positive effect on donation amount while recently donating to charity had a negative effect on donation amount.

Table 1.11 is similar to Table 1.10, but here the dependent variable is whether or not a person gave to charity. In model 1, the information dummy is positive and significant, indicating that information had a positive and significant effect on the likelihood of donating by 6.9 percentage points across all treatments. In model 2, incorporating treatment indicators and interactions with information causes the coefficient on the information dummy to become insignificant. Finally, in model 3, the coefficient on the information dummy is again, no longer significant.

Thus, the robustness checks for information in Table 1.10 and 1.11 reveal little to no effect on donation rates or amounts. This could be due to a couple of reasons. First, small information effects might be crowded out by other treatment effects. Additionally, we chose a well-known charity, St. Jude Children's Research Hospital. While we chose this charity to ensure high donation rates, we might have confounded the purpose of the informational component by choosing a charity that is vastly popular and well known. In addition, children's charities, like St. Jude's, are the highest grossing charities due to the emotion impact of their cause.[24] As such, we propose that the use of a lesser-known charity, with less emotional appeal would be a better choice to study information effects further.

Motives

After donation, we asked subjects a follow up question where subjects had the option to select one or more reasons behind their donation decision. Of the subjects that donated, the most popular selection was "I like the Charity," with 59% (displayed in Figure 1.5) with "The amount suggested was a reasonable request" in close second with 55%. The most popular reason selected for subjects who chose not to donate was "I recently donated to charity" at 34% with "I just didn't want to" as the second most popular answer at 29% (displayed in Figure 1.6).[25] This could indicate that these subjects have recently given to charity by other means or it could indicate that subjects feel obligated to say so due to social norms. On average, subjects gave an average of 1.84 reasons for their donation decision. However, subjects that donated gave an average of 2.53 reasons, while subjects that declined to donate gave an average of 1.17 reasons.

[24] Catalyst (2016)
[25] Each subject could select more than one option, so the percentages will not add up to 100.

When asked whether they enjoyed being asked to donate, the most popular answer was "Yes" at 43.4% and "Indifferent" in second place at 43.1%. Interestingly, only 13.5% answered "No." Table 1.12 provided a breakdown of participants' donation decision and enjoyment of being asked to donate. An overwhelming majority of participants who donated selected that they either enjoyed or were indifferent to being asked to donate. Only 2.6% of subjects donated but did not like being asked to do so. This indicates that there might be a small but present social pressure to donate, even if subjects do not want to. Meaning, for these participants, the social pressure cost outweighed the utility from donating. For those who chose not to donate, most selected that they were indifferent. Interestingly, 12.6% of participants chose not to donate, but enjoyed being asked to do so.

Discussion

In this study, we are able to compare three popular checkout charity solicitation mechanisms. We find that, at amounts less than $1, participants in the Rounding treatment were much more likely to donate than those in the Fixed Request and Open-ended treatments. Differences in donation rates between the Rounding and Fixed Request treatments are primarily driven by loose-change effects, whereby individuals are more likely to donate if they would have less change as a result. Participants in the Fixed Request treatment exhibited higher mean willingness-to-donate than those in the Open-ended. However, the variance in willingness-to-donate in the Open-ended treatment was much higher. Additionally, a one sentence information statement about the charity has a positive significant effect on donation rates and amounts in the Fixed Request treatment.

When choosing a checkout charity method, the “successfulness” of a particular fundraiser might be different for different organizations. One organization might care about methods that garner the highest donations whereas others might care more about participation rates. Thus, choosing between higher average donations (Fixed Request) versus higher donation rates (Rounding) might depend on a charities goals. Additionally, an individual’s mean willingness-to-donate might not be identical across different charities. Therefore, calibrating mean willingness-to-donate will be integral into choosing the best solicitation mechanism.

However, further exploration of checkout charity mechanisms is needed. There are a few important caveats to note about this study. To start, this experiment took place in a lab setting in which the donation solicitation is slightly different from checkout charity. While we do not expect the main findings of this study to change, it is very possible that the magnitude of effects might differ if implemented in the field. For example, with the Rounding method, we might expect there to be a fundamental difference between “rounding down” and “rounding up.” Additionally, asking people to donate from money earned versus adding on an additional donation that increases spending, represents another difference in setting.

Importantly, real-world situational aspects of the checkout charity setting might interact with mechanisms effects. For example, the type of payment used could affect donation rates. Increasingly, consumers purchase goods with card versus cash. Do loose-change effects exist if consumers pay with card or cash? Does payment type effect a consumer’s underlying willingness-to-donate? Thus, further exploration is needed to determine if there are potential interaction effects between solicitation and purchase structure.

As checkout charity has seen a rapid uptake in use, this paper addresses a paucity in the economics literature by examining the efficacy of such programs, best practices of implementation, and measurement of the behavioral mechanisms at hand. This paper has opened new pathways for experiments in charitable donations in both the laboratory and field. Future work will address framing effects of rounding up versus rounding down, loose-change effects, and information effects.

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Chapter 2: On the Design of Fundraising Campaigns: goal setting and information provision in dynamic fundraisers

Abstract

Using a laboratory experiment, I study key aspects of fundraising campaigns that utilize goals or provision points that must be met in order to provide a good or service. In particular, I compare campaigns characterized by a final goal only, an intermediate goal and a known final goal, and a third setting where the final goal is known only if the intermediate goal is reached. Across these three settings, I vary whether an individual's payoff from reaching a goal is uncertain or certain, which is intended to capture the effects of providing vague or precise information on the good or service to be provided. I find that the addition of an intermediate goal *decreases* the likelihood of reaching the final goal. Moreover, the level of the intermediate goal (holding payoffs for reaching the goal fixed) has no discernible effect on whether the intermediate or final goal is reached. This suggests a possible strategy whereby the campaign designer includes a final goal as a decoy in order to reach the desired "intermediate" goal. Value uncertainty has a negative significant effect on the likelihood of reaching the goal when only one goal is present. Finally, goal uncertainty has a positive significant effect on contributions.

Introduction

With technological advancements leading to an increasing number of online crowdfunding platforms, such as Kickstarter and GoFundMe, there are now relatively low barriers to implementing a fundraising campaign. Consequently, there has been a proliferation in the number of fundraising campaigns as well as the number of people and organizations engaged in fundraising. Importantly, campaign architects face numerous choices related to fundraising design. For example, the number of campaign goals and their levels, the timing of when these goals are revealed, and whether precise information about the good or service to be funded is provided could all impact contribution behavior. This is especially true in crowdsourced fundraising where potential donors have real time information on funds raised and individual donors can make multiple contributions. However, there is little causal evidence on how these choices affect the success of fundraising efforts. To help fill this knowledge gap, I use a laboratory experiment to investigate two key issues faced by the designers of online fundraising campaigns – goal setting and information provision – using a real-time, continuous donation interface that typifies online campaigns.

Many fundraising campaigns make use of goals, in particular, provision points that must be reached in order for a good or service to be provided. However, where should a goal be set? And is it better to use a single goal or multiple goals? Multiple goals are commonly used by nonprofit organizations and online platforms such as Kickstarter but less common on websites like GoFundMe. In addition, "stretch" goals could be introduced during the campaign, perhaps as a strategic move, whereby the campaign designer extends the campaign past the initial goal to a new, higher funding goal (and associated good provision) that was not announced at the beginning of the campaign.

While we might think of provision point mechanisms in the context of providing public goods, many private goods are supported through fundraising campaigns. Examples range from a non-profit organization soliciting donations to feed hungry families to individuals and entrepreneurs crowdfunding to raise capital for a new board game startup through websites such as GoFundMe, Kickstarter, and Indigo. Yet, there may be discretion on the quality of the good, which in turn alters the goal. However, strategies on determining provision points may be context specific. In the case of a nonprofit organization, the provision point is likely to reflect the actual cost of providing a good or service. For instance, a university might decide on two possible provision points to fundraise for a new library: a lower provision point to renovate an existing library and a higher provision point to build a new one. For entrepreneurs raising capital on Kickstarter, the provision point(s) might not only cover the cost of product development but, moreover, a profit margin, the latter amount being discretionary. An entrepreneur may strategically set the goal low in order to capture market share, or to hook consumers with a base model before bringing a more profitable version to market.

As another consideration, the value of the good or service produced upon reaching a campaign goal may be uncertain. This is to be expected for a new market good, but also is likely to characterize many public goods with which donors have little experience or where there is little transparency. Importantly, information provision is at the discretion of the campaign designer. For instance, an aspiring artist on Kickstarter can reduce uncertainty by providing one song for free from a proposed album. Similarly, a university raising funds for a new library can release a video or architectural rendering of the proposed structure. But, is providing better information conducive to fundraising success?

To investigate whether campaign design structures have an impact on the amount raised and the likelihood of provision, I design and implement a controlled laboratory experiment. I focus on a setting where the campaign designer fundraises for a discrete good that may be provided at intermediate levels and may influence *ex ante* uncertainty over values through information provision. Specifically, I compare treatments characterized by the goal structure and whether valuations are certain or uncertain. Within each treatment there are multiple scenarios that vary the goal(s) while holding valuations fixed, which allows us to provide insight on the tradeoffs of strategically altering provision points. In terms of goal structure, I compare settings with one provision point (i.e., a final goal), two provision points (i.e., an intermediate and a final goal) that are *ex ante* known, and two provision points where the final is known only if the intermediate goal is reached. For treatments with goal uncertainty, some of the scenarios introduce uncertainty over *whether* there is a second goal. To parallel field conditions, I implement a real-time contribution game where players are free to make multiple contributions and are continuously updated on the success of the fundraising campaign while campaign is in effect.

This paper expands the existing literature related to the private provision of public goods in the context of multiple thresholds, value uncertainty, and threshold uncertainty. I am the first to combine these relevant aspects of campaign design to best study dynamic fundraising. While previous research has investigated each of these design elements with simultaneous or sequential decision-making, no paper has combined multiple popularized elements of campaign design to study contribution behavior in a dynamic setting. In this way, I am able to provide insight on some fundraising best practices.

I find that the addition of an intermediate goal decreases the likelihood of meeting the final goal. This result holds in treatments with uncertainty over the final goal. However, uncertainty over the final goal does have a positive significant effect on contributions when the value of reaching the goal is certain. Finally, Value uncertainty has a negative significant effect on provision likelihood in the One Goal treatment, but no effect on average contributions.

Although there is a large experimental literature on the private provision of public goods, there are only a handful of studies that examine the case of multiple provision points. Bagnoli and Lipman (1989) formally modelled settings where a public good is provided if and only if contributions meet or exceed a certain threshold/goal. Since then, few papers have investigated contribution behavior in multiple threshold public goods games. Bagnoli et al. (1992) launched the first experimental investigation into multiple provision points with few follow up papers (Chewning et al. 2001; Normann and Rau 2015; Hashim et al. 2017; Liu et al. 2016). In their investigations, both Bagnoli et al. (1992) and Chewning et al. (2001) conclude that introducing multiple thresholds leads to confusion and coordination failure, often reducing overall contributions relative to the single threshold case.

However, differences in refund rules and choice architectures could also account for these results. For example, Chewning et al. (2001) only include a refund rule for failing to reach the first goal, thereby inducing additional risk and uncertainty over the possibility of meeting additional goals. Further, in both studies, participants simultaneously and independently make a single contribution decision.[26] I introduce a real-time dynamic contribution structure whereby donors are able to contribute as much and as often as they want within the given period.

There have been some investigations into real-time decision games (continuous time) across different game designs. Dorsey (1992) was the first to study the impact of real-time

[26] In a single threshold case, participants make 1 decision where there is 1 symmetric equilibrium. Introducing multiple thresholds introduces multiple equilibria, possibly leading to confusion as Bagnoli, Ben-David, and McKee (1992) conclude.

donations finding that continuous time does increase contributions to public goods in a voluntary contribution mechanism (VCM) with a provision point.[27] Additional studies generally conclude that real-time contributions exceed those from a standard simultaneous choice setting ((Goren et al. 2003; Goren et al. 2004; Duffy et al. 2007; Choi et al. 2008). Most closely related, Choi et al. (2008) use a single provision point game to study simultaneous vs. sequential decision settings with multiple periods and find that as the number of contribution rounds increases, provision of the public good increases. Therefore, I expect the introduction of dynamic real-time game play to increase the likelihood of provision.

Some papers have examined threshold uncertainty it in provision point games (Wit and Wilke 1998; Suleiman et al. 2001). The stylized fact from this literature is that threshold uncertainty reduces the rate and intensity of cooperative behavior, decreasing contributions (Boucher and Bramoullé 2010; Chen et al. 1996; Nitzan and Romano 1990). However, theoretical work by McBride (2006) addresses the potentially non-monotonic relationship between threshold uncertainty and coordination. If the value of the good is sufficiently high, uncertainty increases equilibrium contributions. Therefore, is difficult to assume the effect of threshold uncertainty in the case of multiple provision points. One could expect an uncertain second goal to reduce overall contributions, similar to previous literature. Alternatively, the uncertainty could induce higher contributions, to "unlock" or reveal the certain value of the second goal upon reaching the first. Due to these potentially competing effects, I develop a simple model to form predictions on how individuals respond to uncertainty over the second goal.

Additionally, previous research on uncertainty over the value(s) of a public good is limited to linear voluntary contribution mechanism (VCM) games (Gangadharan and Nemes 2009; Levati

[27] Only in the case where subjects were allowed to increase or decrease their contributions.

et al. 2009). By changing either the marginal per-capita return (MPCR) from donating to the public good or the marginal value of money kept, uncertainty has little to no effect on contributions (Levati et al. 2009; Levati and Morone 2013; Gangadharan and Nemes 2009). However, in a linear VCM, the uncertainty is multiplicative (i.e., uncertainty increases with contributions), whereas in the provision point case it is natural to model the uncertainty over the value of the discrete good as additive. Therefore, it is unclear whether the impact of value uncertainty can be inferred from this literature. The effects of value uncertainty might mirror the effects of threshold uncertainty. In this case, it seems likely that uncertainty will have no effect on a risk neutral individual, while decreasing overall contributions for a risk averse individual.

Theoretical Framework

To gain insight on behavior in the experiment, and to formulate testable hypotheses, I use a simple two-player model, where both players choose contributions simultaneously. In doing so, I extend the model of Menezes et al. (2001) to introduce multiple thresholds and value uncertainty using an approach by Bagnoli and Lipman (1989). While this abstracts from complications of the real-time contribution setting with additional players, the main comparative statics results in the simple framework are likely to extend. Moreover, due to the dynamic nature of the real-time contribution game, it is difficult to model the interactive structure of the game without imposing significant simplifying assumptions. Further, any model trying to capture dynamic elements of game play would be unlikely to reveal any solvable best-response functions. The model used by Choi et al. (2008) is most closely related, but follows sequential structures not present in this experiment.

One-Goal Case

Each player is given an endowment of ω and can contribute some amount, x, to fund the public good. The good will be provided if contributions reach some goal, t. If contributions fall short of the goal, all individual contributions are refunded to the individual players. Provision of the good results in a payout of v to each member of the group. The payout is not dependent on the relative contribution of a particular player, or whether the player contributes at all.

The probability that the good is provided, $P(x_1, x_2)$, depends on the contributions of player one, x_1, and contributions of player two, x_2. Because the good is only provided if the goal t is met, the probability of provision is equal to:

(1) $$P(x_1, x_2) = \begin{cases} 0 & if\ x_1 + x_2 < t \\ 1 & if\ x_1 + x_2 \geq t \end{cases}$$

In this model, any contributions in excess of the goal t are refunded to players one and two in proportion to each individual's share of total contributions.[28] Thus player one and two's best response functions are

(2) $$x_1(x_2) = t - x_2$$

and

(3) $$x_2(x_1) = t - x_1,$$

where $v - x_1 > 0$ and $v - x_2 > 0$ for player's one and two respectively. As long as player one believes player two will contribute some amount such that the net benefit of contributing is still positive, player one will contribute. Therefore, there many possible equilibria defined by levels

[28] In the experiment, contributions cannot exceed the threshold, thus $x_1 + x_2$ is always strictly less than or equal to t.

of x_1 and x_2 that are sufficient to reach the threshold. However, there exist many combinations of x_1 and x_2 where the good is not provided, $x_1 + x_2 < t$.

Two-Goal Case

The two-goal setting where there is an intermediate and a final goal, can be characterized as a two single goal games played sequentially. Here, I can assume that a player makes decisions in the first stage in isolation of the second stage. Each goal had a different payout, v_I and v_F. The payout for reaching the final goal is higher than the payout for reaching the intermediate goal. Therefore, in a two-goal case where $v_F = v$ and final goal $t_F = t$, the best-response functions of player one and player two are the same as the one-goal case.[29] Thus, there should be no significant difference in likelihood of provision at the final goal between treatments with one and two goals. This prediction is consistent with Bagnoli and Lipman (1989).

Value Uncertainty

Now assume there is one goal, and the value of the good is uncertain prior to provision. The payout for reaching the goal can now be one of two values, v_1 or v_2, each with a 50% probability. Thus, the expected value of the payoff is equal to $E[v] = \frac{1}{2}(v_1 + v_2)$. Assuming players are risk neutral, and that $E[v] = v$, the best response functions are the same as before.

However, if at least one player is sufficiently risk averse, then their expected utility in the case where the payout is uncertain is strictly less than $\frac{1}{2}(v_1 + v_2) = v$. Therefore, a risk averse player's best response, will always be less than that of a risk neutral player when uncertainty over

[29] Proof in Appendix B.

the value exists. Thus, the goal is less likely to be funded if at least one player in the group is sufficiently risk averse. Because the expected net benefits of contributing decrease as the expected utility of providing the good decreases, players will be less likely to contribute.

Goal Uncertainty

Introducing *ex ante* uncertainty in the second goal substantively changes the decision strategies. In this game design, the exact final goal is not revealed until the intermediate goal is met. Meeting the intermediate goal unlocks information in the game (the exact cost of the second goal), regardless of whether a player then engages in contribution decision-making to meet the final goal. Therefore, we can think of revealing the certain final goal as unlocking an option value. Players derive utility from the option of unlocking the exact cost of meeting the second goal after meeting the first goal.

Using backwards induction, I first find the best response functions for the second threshold, then, based on these, formulate best response functions for the first goal.[30] By incorporating the expected value of meeting the second goal into the first decision stage, the expected utility of meeting the intermediate goal has increased, making it more likely that the intermediate goal is met when the final goal is uncertain. Therefore, goal uncertainty increases the probability the good is funded at the intermediate goal relative to the certain goal case.

Interaction between goal uncertainty and value uncertainty

Incorporating value uncertainty into a model with secondary goal uncertainty yields an ambiguous effect on public good provision and contributions. On one hand, value uncertainty

[30] Please refer to Appendix B for details.

should have a negative effect on contributions if at least one player is risk averse. However, goal uncertainty should have a positive effect on contributions if the expected value of the good at the second goal is sufficiently high. Our model is unable to determine the magnitude of either effect, giving us an ambiguous result for the two-goal case with value uncertainty and secondary goal uncertainty.

Experimental Design

Participants are randomly matched into groups of four, with 4 to 6 groups per session. Participants remain in the same group for the entire experiment, which consists of 15 or 16 scenarios (i.e., separate fundraising campaigns) depending on the treatment. The order in which the scenarios are encountered randomly varies across groups.

A decision period (scenario) proceeds as follows. Each participant is endowed with 8 tokens.[31] They then have the opportunity to contribute any or all of their tokens towards a group “project”. Any tokens not contributed are theirs to keep. Each project has one goal (a final goal) or two goals (an intermediate and a final goal) that must be reached for project provision. If a group contributes enough tokens to reach a goal, all members of the group receive the same payout. In treatments with two goals, the group receives a payout for reaching either goal. However, the payout is larger for reaching the final versus the intermediate goal. Across all scenarios, if only the intermediate goal is reached, the payout to each group member is 5 tokens. If the final goal is reached, each participant receives 10 tokens.[32] While the payouts are fixed, the goals vary across scenarios. This means that the net benefits and the marginal incentives for contributing towards a

[31] All money amounts are denominated in tokens with a conversion rate of 10 tokens to 1 US dollar.
[32] To be clear, these payouts are not additive: the payout for the final goal is not in addition to the payout for the intermediate goal.

goal also varies. In all scenarios, reaching the final goal results in the efficient (i.e. group payoff-maximizing) outcome.

Contributions are only binding if a goal is met. Otherwise, any contributions towards an unattained goal are fully refunded. To be clear, in treatments with two goals, refunds are implemented as follows: (1) if the intermediate goal is not met, all contributions are refunded to contributors, (2) if only the intermediate goal is met, all contributions made after this goal was reached are refunded. Using refunds lowers the risk associated with contributing tokens and increases the likelihood of meeting the final goal. The software is programmed such that a person cannot contribute more than what is needed to meet the (next) goal.

In treatments with uncertainty over the final goal, participants do not know the exact goal at the start of the round. Instead, they are shown two possible values of the goal, and each has an equal chance of being selected. In these treatments, the computer randomly selects a value for the final goal, and only reveals this value to the group when the intermediate goal is reached. In some scenarios, one of the possible values is “no goal”, which reflects a setting where it is unknown whether the campaign organizer will introduce a “stretch” goal.

Similarly, in treatments with value uncertainty, participants know the payout(s) for reaching a goal could be one of two possible amounts, each with a 50% chance. For all scenarios, the payout for only reaching the intermediate goal only is either 3 or 7 tokens, and the payout for reaching the final goal is either 8 or 12 tokens. Thus, the expected value of reaching the intermediate or final goals are 5 and 10 tokens respectively, allowing for comparisons between the parallel treatments with certain values. The actual payout is only revealed at the end of the round.

I implement six treatments using a 3x2 between-subjects design, as depicted in Table 2.1.[33] There are three goal structures, which allows me to make comparisons between settings with one versus two goals, and two-goal structures with and without uncertainty over the final goal. Additionally, I vary whether values are certain or uncertain. Note that in the two-goal treatments with certain values, the values of the good at both goals are known, regardless of whether or not a goal is uncertain.[34] In treatments with value uncertainty, the value of the good remains uncertain until the end of the round, regardless of whether any goals are met during the contribution phase.

Parameters for each scenario and each treatment are detailed in Table 2.2. With these parameters I am able to make many meaningful comparisons both within and between treatments. For example, Scenarios 7, 8, and 9 in the single-goal treatments (T1 and T4) can be compared to Scenarios 4, 6, and 8 in the two goal treatment where both goals are pre-announced (T2 and T5); in all these cases, the final goal equals 12 tokens. Similarly, to compare the effects of goal uncertainty, we can compare Scenario 4 for T2 and T5 treatment with Scenario 2 and Scenario 5 for the two goal treatments where the final goal is uncertain (T3 and T6). The intermediate goals are identical across these scenarios, as are the (expected) level of the final goal. Finally, I introduce an additional case of uncertainty in the last eight scenarios of T3 and T6. Here the uncertainty is over whether there *is* a final goal. In particular, there is a 50% chance that the final goal will be a known value and a 50% chance that there is no final goal. This captures uncertainty over whether there is going to be a "stretch" goal.

[33] All tables and figures for Chapter 2 are located in Appendix B.
[34] While this design choice may abstract from reality, this allows a cleaner comparison between the one and two-threshold cases. Not revealing the value of the good at the second goal would imply ambiguity rather than uncertainty.

Testable hypotheses and power analysis

This experimental design allows me to test hypotheses related to the likelihood of provision and group contributions. These hypotheses stem from predictions derived from the theoretical framework, and from prior experimental work.

Hypothesis 1. Group contributions, and the likelihood of reaching the final goal, decrease as the goal (provision point) increases.

Hypothesis 2. Group contributions, and the likelihood of reaching the final goal, are the same in the one-goal treatments and the two-goal treatments, when all goals are known at the start of the campaign.

Hypothesis 3. If players are risk averse, value uncertainty decreases the likelihood of reaching a goal.

Hypothesis 4. If the net benefit of reaching a goal is sufficiently high, an uncertain final goal increases the likelihood that the intermediate goal is reached, relative to the certain goal case.

For Hypotheses 1, as the goal t increases, the expected net value of reaching the goal decreases. Therefore, the likelihood of funding the public good decreases as t increases (as does contributions, on average). Hypothesis 1 holds in cases with one or two goals where the final goal in a two-goal case is equal to t. Hypothesis 2 stems from the theoretical framework, as well as Bagnoli and Lipman (1989). If $v = v_{\mathrm{F}}$ and $t = t_{\mathrm{F}}$ in treatments with known goals, average contributions as well as the likelihood of provision will not be significantly different between treatments with one and two goals. For Hypothesis 3, introducing uncertainty lowers the expected utility of contributing for risk-averse individuals relative to certain value treatments. Therefore, the likelihood of funding the public good will be lower relative to the certain case in both treatments. Finally, for Hypothesis 4, in treatments where the final goal is uncertain, players gain additional utility from the option value of reaching the intermediate goal, increasing the expected utility of meeting the intermediate goal. Therefore, the likelihood of meeting the intermediate goal increases with final goal uncertainty as along as the net benefit of reaching a goal is sufficiently high.

To determine sample sizes, I conducted a paid pilot experiment with 24 participants using T2. In my power calculations, I assume that the estimated within and between-subject variances from the pilot session are representative of all treatments. Moreover, I assume that tests are based on a linear regression model with standard errors clustered at the group-level. Based on calculations using 80% power and 5% significance level, this led to a target sample size of 15 groups per treatment. This allows one to detect a minimum effect size of 11 percentage points when testing whether two treatments have the same likelihood of reaching a fundraising goal, and a minimum detectable effect size of 0.5 tokens when comparing contributions.

Participants and procedures

Three-hundred and sixty-four undergraduate students participated in the experiment during the Fall of 2019 and Spring of 2020. All sessions were conducted in a designated experimental economics laboratory, and participants were recruited from an existing subject pool. The pool resembles the general population of students with respect to gender, age, etc. In total, there are nineteen sessions with an average of 20 participants per session. Sessions lasted approximately 90 minutes and individual earnings averaged $23.73.

Decisions were entered on networked computers using a program coded with the software z-Tree (Fischbacher 2007). Written instructions were provided to participants, which were read aloud by the same moderator in each session.

The experiment included three separate tasks. First, participants faced a multiple-price-list risk elicitation procedure popularized by Holt and Laury (2002). This was followed by a practice round before moving onto the main experiment in which subjects participate in 15 or 16 decision rounds (scenarios) of the contributions game. The experiment concluded with a post-experiment questionnaire. Representative instructions and computer screenshots are provided in Appendix B.

Results

Table 2.3 summarizes the descriptive statistics for participants. Of the 364 participants, 58.6% are male and the average age is 20. The risk elicitation task suggests that 51.9% of participants can be characterized as risk averse. On average, groups reached the intermediate goal 98.3% of the time while groups reached the final goal 81.5% of the time.

Figure 2.1 shows the final goal success rate by treatment. The largest difference between treatments occurs when comparing the One Goal treatment with certain values (T1) to the Two Goals, Known treatment with certain values (T2), which differ by 19 percentage points. In treatments with certain values (light grey), the inclusion of an intermediate goal significantly lowers the likelihood of reaching the final goal by approximately 18 percentage points.

In treatments with uncertain values, the inclusion of an intermediate goal only significantly lowers the likelihood of reaching the final goal when the final goal is certain.[35] Participants in the Two Goals, Known treatment are less likely to reach the final goal that those in the Two Goals,

[35] The difference in funding rates between the One Goal treatment and the Two Goals, 2nd Unknown is not statistically significant when the value of reaching the goal is uncertain.

2nd Unknown treatment. Thus, the interaction of goal uncertainty and value uncertainty has a positive impact on provision. While this result is counter to Hypothesis 2, it is consistent with previous literature that finds multiple goals decreases the likelihood of provision at a higher level (Bagnoli et al. 1992; Chewning et al. 2001).

Comparing the two-goal treatments with and without goal uncertainty, I find no significant difference in the likelihood of reaching the final goal when the goal is certain versus uncertain. This effect is present in treatments with value certainty and value uncertainty. Final goal success rates between these two-goal structures range between 74-80%.

Value uncertainty only has a significant effect in the One Goal treatment. Here value uncertainty decreases the likelihood of reaching the final goal. However, this effect does not persist in treatments with two goals. When two goals are present, there is no significant difference in the final goal success rate between treatments with certain and uncertain values.

Figure 2.2 shows total group contributions by treatment. Comparing treatments with certain values, contributions are significantly lower in the Two Goals, Known treatment than the One Goal, consistent with hypothesis 2. Additionally, the Two Goals, Known treatment with certain values induces significantly lower group contributions than the Two Goals, 2nd Unknown treatment with certain values. Thus, goal uncertainty seems to have a positive effect on group contributions, consistent with hypothesis 4. There is no significant differences in group contributions between the One Goal and Two Goals, 2nd Unknown treatments with value certainty.

There are no significant differences between treatments when values are certain versus uncertain. Therefore, value uncertainty has no effect on group level contributions within goal structures. In treatments with uncertain values, contributions are significantly lower in treatments with two goals compared with the one-goal treatment.

Group Contributions

I now turn to a formal econometric analysis of group-level contributions. Here, group contributions refers to the intended number of tokens contributed by the group before any refunds are processed. To do this, I analyze the experimental panel data using a linear regression model. To account for correlation across the multiple scenarios undertaken by a particular group, standard errors are clustered at the group level. Table 2.4 displays two regression models. In both, indicators associated with treatments T2 to T6 are included such that their coefficients measure differences from the baseline T1, the One Goal treatment with certain values. Model 1 utilizes only treatment indicators whereas Model 2 includes participant characteristics. Additionally, I control for order effects by including four indicator variables for the four randomized scenario orders.

In treatments with certain values, participants in T2 (two goals) contribute significantly less than participants in T1 (one goal). On average, participants in the T2 treatment contribute 1.174 tokens less than those in the T1 treatment. This result is counter to Hypothesis 2. The difference in contributions between T1 and T3 (Two Goals, Uncertain final goal) is negative but not statistically significant. T2 and T3 are not significantly different ($p < 0.12$). When values are uncertain, using a two-goal structure decreases contributions (T5 versus T4, $p < 0.002$; T6 versus T4, $p < 0.05$). There is no statistical difference between the two-goal treatments with value uncertainty (T5 and T6) ($p < 0.29$). These statistical results are robust to inclusion of additional control variables, as suggested by Model 2.

In model 2, value uncertainty now has a significant negative effect on total group contributions when there is only one goal.[36] On average, participants contribute 0.277 tokens less

[36] Significant at the 10%.

when values are uncertain. Additionally, from this model, there is evidence that risk aversion is negatively correlated with contributions.

Table 2.5 reports regression results testing how contributions vary according to the specific level of the final and/or intermediate goals, and further allow direct tests of the effects of inducing value uncertainty. Separate regressions are estimated for each goal structure. In these regressions I include the magnitude of the intermediate and final goals, an indicator for value uncertainty, and an indicator for whether "No Goal Possible" was an option in the Two Goals, 2nd Unknown treatment.[37]

Increasing the final goal leads to a significant increase in total contributions across all goal structures. This result is interesting as it suggests that, while one is potentially lowering the chance of provision by increasing the goal, this effect is not strong enough to offset the additional contributions obtained from this adjustment. For example, in the one-goal treatments, increasing the final goal by one token leads to an increase of 0.952 tokens contributed. This is almost a one-to-one relationship. However, the effect size is significantly lower in the two-goal treatments. This might be unsurprising consider the results shown in Figure 2.1.

Additionally, the magnitude of the intermediate goal has a statistically significant effect on total contributions in the Two Goals, Known treatment, meaning that a higher intermediate goal increases total group contributions by 0.129 tokens. However, the magnitude of the intermediate goal does not have a significant effect on group contributions in the Two Goals, 2nd Unknown treatments.

[37] In these rounds, participants were given two possible outcomes for the final goal which included the possibility that there would not be a final goal (with a 50% chance). Scenarios shown here only the ones in which "No Goal Possible" was not selected and participants had the option of reaching a final goal.

For all three goal structures, there is no statistically significant effect of value uncertainty on group contributions. However, in the Two Goals, 2^{nd} Unknown treatment, No Goal Possible has a significant negative effect on group contributions. If participant was faced with the uncertainty of even having a final goal, they contributed 1.258 tokens less on average than those in the same treatment that had a certain option of having a final goal.

Final Goal Provision

Tables 6 and 7 parallel the regression analyses in Tables 4 and 5, respectively, but use an indicator of whether the final goal was reached as the outcome variable of interest (i.e., reported are linear probability models). Discussing insights from Table 2.6 first, participants in all treatments were significantly less likely to reach the final goal relative to the baseline treatment, One Goal with certain values (T1). The largest difference between treatments occurs when comparing the One-Goal treatment to the Two Goals, Known treatment (T2) with an 18.7 percentage point decrease in the likelihood of reaching the final goal.

Comparing both Two Goal treatments reveals no significant difference in success rates with between either certain or uncertain value treatments (T2 versus T3, $p < 0.79$; T5 versus T6, $p < 0.46$). In treatments with uncertain values, there is a significant difference between the One Goal (T4) and Two Goals, Known treatment (T5), meaning participants in T5 were less likely to reach the final goal than those in T4 ($p < 0.045$). This is inconsistent with hypothesis 4, but in line with previous literature.

Model 2 shows consistent magnitudes and significance across all treatment indicator variables. Additionally, both the gender of a participant and their relative risk aversion have significant negative effects on the likelihood of meeting the final goal. If a group is made up of all males, they are 17.5 percentage points less likely to reach the final goal than a group that is all female. Similarly, if a group is made up of all risk averse players, then the groups is 19 percentage points less likely to reach the final goal than a group consists of all risk neutral players.

Table 2.7 is much like Table 2.5, but here the dependent variable is whether the final goal was reached or not. For all treatments, as the magnitude of the final goal increases, the likelihood of reaching that goal decreases. This magnitude of this coefficient is much smaller in the One Goal treatment (0.9 percentage points) relative to the two goal treatments (~5.3 percentage points). The magnitude of the intermediate goal has no significant effect on the likelihood of meeting the final goal.

Value uncertainty has a negative significant effect on the likelihood of meeting the final goal in the One Goal treatment, as we saw in Figure 2.1. Value uncertainty has no significant effect in either of the two goal treatments. Finally, "No Goal Possible" has a negative significant effect, lowering the likelihood of reaching the final goal by 12.9 percentage points.

Intermediate Goal Provision

To test Hypothesis 4, that the intermediate goal is more likely to be met in cases with goal uncertainty, I regress an indicator for whether the intermediate goal was met on treatment indicators. These results are displayed in Table 2.8. Again, model 1 includes only treatment dummies whereas model 2 includes participant and order controls. Additionally, the baseline is again, the One Goal treatment with certain values (T1).

I fail to reject that, jointly, all treatment indicators are statistically different from zero ($p = 0.20$). Thus, there is no variation in this outcome across the four treatments with intermediate goals. From the raw data, this result is not unexpected as the intermediate goal was reached in virtually all cases, with little variation across treatments. However, it is worth noting, that in treatments with final goal uncertainty where the option “no final goal” was a possibility, groups met the intermediate goal 100% of the time.

Table 2.9 is similar to Tables 2.5 and 2.7. I regress the magnitudes of both goals, an indicator for value uncertainty, and an indicator for whether the final goal was possible. Neither goal magnitudes have a significant effect on the likelihood that an intermediate goal is met. Additionally, neither value uncertainty nor “No Goal Possible” has an effect on the likelihood of meeting the intermediate goal. Thus, I reject hypothesis 4, finding that goal uncertainty has no effect on the likelihood of meeting the intermediate goal.

Individual Behavior

It is possible that the preceding group-level analyses mask important differences within groups. To investigate heterogeneity in individual-level behavior, I next analyze a measure of within-group variation in contributions. Specifically, I define a variance measure for donor i in group g and scenario s as the squared deviation from the average contributions from the group in this same scenario:

(4) $$Contribution\ Variance_{igs} = \left(x_{igs} - \bar{x}_{gs}\right)^2$$

Regressions with this variance measure as the dependent variable are displayed in Table 2.10. Similar to Tables 2.4 and 2.6, model 1 consists of treatment indicators where T1 is the baseline treatment and model 2 incorporates those same treatment indicators along with demographic characteristics and controls for order effects. In models 1 and 2, we observe no significant effect of any independent variable on individual contribution variance within a group.

Table 2.11, however, shows that increasing the final goal magnitude increases individual contribution variance. The effect on individual contribution variance is largest in the treatment with Two Goals, Known with an increase of 0.260 and almost twice the size of the effect in the One Goal treatment. The magnitude of the intermediate goal has no effect on individual contribution variance. Interestingly, in the Two Goals, Known treatment, value uncertainty leads to a large significant decrease in variance.

Dynamics

Figure 2.3 reports mean group contributions by treatment across all 15 decision rounds in the experiment. In previous experiments, participants often learn over multiple periods, lowering the likelihood of reaching the goal as the experiment progresses (Chewning et al. 2001). However, I do not observe this behavior, as group contributions are relatively stable across all treatments as participants progress through the experiment.

While Figure 2.3 pools observations across the different goal structures, Figure 2.4 provides a breakdown of all six treatments and group contributions over periods. Here again, I do not observe any sign of learning at the aggregate level. Thus, participants' contribution behavior is at best very weakly correlated with behavior from the previous round.[38]

I do, however, observe changes in behavioral dynamics across all 15 periods. Figure 2.5 shows the average time in which groups meet the final goal by period. Beginning in period 1, groups across all treatments reach the goal in around 74 seconds. However, over the next 5 periods,

[38] This finding is consistent with results from the pilot session, where the fraction of the overall variance attributable to differences in group-level behavior over decision periods was just 0.02.

this time increases to an average of 104 seconds. For the rest of the experiment, groups meet the final goal within the last 20 seconds of the period. This waiting behavior or slow-down in contributions might signal some learning on behalf of participants within a group.

This is further demonstrated by the distribution plot shown in Figure 2.6. Figure 2.6 shows the distribution of time in which the final goal is met for all treatments and periods. As shown, more than 50% of the time, the final goal is met within the last 5 seconds of the round, signaling potential strategic behavior. Groups met the final goal in less than 100 seconds in fewer than 20% of all rounds in all treatments.

A closer examination of within period contributions reveals that participants contribute almost 80% of the needed tokens to reach the final goal within the first 60 seconds of the period. Figure 2.7 shows the percent of the final goal funded within the 2-minute contribution window. As shown, participants spend the last minute of the period waiting to see who will contribute the last 10 to 20% of the needed funds.

Finally, Figure 2.8 shows the time in which the intermediate goal is met across all 4 treatments with two goals. In three of the treatments, the intermediate goal is met, on average, at 20-30 seconds into the period. However, in the Two Goals, Known treatment with uncertain values, the intermediate goal is met at approximately 12 seconds, on average. This is twice as fast as the Two Goals, Known treatments with certain values and indicates that participants randomized into this treatment contributed at a quicker rate than those in the other treatments with intermediate goals.

Discussion

In this experiment, I test whether key elements of dynamic fundraising campaigns have an effect on campaign success. Specifically, I test whether the number of goals, goal uncertainty, and value uncertainty have an effect on provision rates using a continuous-time dynamic public goods game. Despite the use of a continuous time decision-making environment, and token refunds for not meeting the goal, inclusion of an intermediate goal reduces the chance a (final) fundraising goal is reached.

While provision rates in this experiment are exceptionally high (~74%) relative to others in this literature, the net benefits to participants from provision in this experiment are comparable to that of other provision point games with single and multiple goals, as well as VCM games (Chewning et al. 2001; Choi et al. 2008). Thus, I attribute observed high levels of provision to the dynamic nature of the game, allowing players continuous information feedback, and the fact that contributions are not binding unless a goal is reached. Choi et al. (2008) compare two variants of a sequential game (2 rounds vs. 5 rounds) to a one-shot game and find that provision rates increase with the number of rounds. Allowing multiple decisions across a contribution window gives group participants a chance to signal other members and coordinate within the group, even without a designated chat box (Choi et al. 2008). Additionally, their reported provision rates in the two and five round sequential games are comparable to the observed high rates of provision in this experiment.

Goal uncertainty has no effect on provision rates but does have a positive effect on contributions when the value of the good is certain. While this is in contrast with previous work on threshold uncertainty, the design in our experiment is slightly different. In previous studies, subjects never learn of the goal until round completion. In our experiment, the goal uncertainty is resolved mid-game as the final goal is known once the intermediate goal is met. Perhaps then it is unsurprising that uncertainty resolved during a campaign has no effect on the likelihood of meeting the goal but could influence within campaign contribution behavior.

Value uncertainty has a negative significant effect on reaching the (final) goal in campaigns without an intermediate goal but has no effect in treatments with an intermediate goal. In addition, I find value uncertainty has no effect on mean contributions. This could be due to the large group-level welfare gains to achieving fundraising goals, or the fact that only half of our participants are considered risk averse (~52%). Thus, further experimental study of goal structure and dynamic fundraising is warranted.

As more businesses, nonprofits, and individuals engage in dynamic fundraising, it is important to investigate best practices and incorporate them into campaign design. These results indicate that fundraisers should engage in either of the following goal-setting strategies: (1) set a single goal, or (2) if using multiple goals, set the most desired funding level as the first goal. This experiment shows that donors tend to default to reaching whatever goal is first. Therefore, the use of "stretch" goals is only optimal if the first goal is the primary goal.

Future work using this real-time and continuous contribution interface might benefit from introducing additional variation in the experiment parameters. When determining the parameters for the experiment, I relied on net benefit ratios characteristic of related provision point experiments. However, I did not anticipate the very high success rates, which far exceed most prior studies. Lowering the potential gains from a successful campaign, by decreasing valuations or increasing provision points, may reveal additional differences across goal structures. Moreover, additional comparisons where the intermediate goal in a two-goal setting is equal to the final goal in a one-goal setting would corroborate our claim that it may be desirable set the intermediate goal at the level of the desired fundraising target.

This paper addresses some important dimensions of dynamic fundraising campaigns including goal setting, goal uncertainty, and value uncertainty. However, there are many variations in online fundraising campaigns that have yet to be explored including competition, ambiguity over the value of the good, and advertising. Future research will investigate current best practices to help facilitate a more complete understanding of dynamic fundraising.

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Chapter 3: The Impact of Police Violence on Domestic Violence Reporting

Abstract

Officer-involved fatalities (OIFs) are on the rise in the United States. According to the Washington Post, almost one thousand people were shot and killed by police in 2018 alone. In this paper, I examine the effects of officer-involved fatalities, including officer-involved shootings, on domestic violence reporting. I conduct this analysis using county and zip code level data to understand how concentrated any effects of police violence may be. Using within-county variation, I test whether the number of domestic violence reports decreases in the week after a fatal officer-involved encounter. I find little to no effect of OIFs on domestic violence reporting at the county level. However, using zip code-level 911 call data from Memphis, TN, I find some evidence of decreased 911 calls for domestic violence in neighborhoods where an officer-involved fatality took place. Further, I find that total 911 calls also decrease in affected zip codes and neighborhoods. The magnitude of the effects is dependent on the race of the OIF victim and cause of death.

Introduction

Officer-involved fatalities (OIFs) have increased in the United States over the last decade. According to the Washington Post, almost one thousand people were shot and killed by police in 2018 alone.[39] Community reactions to these events have been mixed with some fatalities labeled as "justifiable," and others sparking outrage and protest (Wheelock et al. 2019; Kirk and Papachristos 2011). However, little is known about the impact of officer-involved fatalities and their effect on community relations.

Trust between police and community is important for many reasons. It has particular economic relevance because that trust is necessary for effective provision of public safety. Public goods like public safety and the enforcement of law can affect the security of property rights, personal health, human capital formation, etc. (Demsetz 1964; Desmond et al. 2016; Bor et al. 2018). However, while all public goods rely to some degree on community engagement and crime reporting, this is especially the case with public safety/law enforcement.

Prior work using case studies suggests police violence may have an effect on crime reporting. Cloninger (1991) finds that incidents of police shootings are inversely related to the rate of non-homicide violent crime. Similarly, recent work also suggests that police violence erodes trust, and those effects are heterogeneous among different populations (Chenane et al. 2019; Gingerich and Oliveros 2018; Kirk and Papachristos 2011; Brunson and Miller 2005; Bor et al. 2018; Cloninger 1991). In a case study of the beating of Frank Jude, results indicate that residents of black neighborhoods, relative to white neighborhoods, were subsequently less likely to report crime (Desmond et al. 2016). While police violence may affect crime reporting through both

[39]The Washington Post. https://www.washingtonpost.com/graphics/2018/national/police-shootings-2018/

mechanisms, the literature has to date only investigated the effects of police violence on *total* crime reporting.

However, outside of these limited case studies, there is little evidence on whether treatment persists over time or whether these affects are limited to a few a select group of officer-involved fatalities. Further, no paper has studied the impact of repeated exposure to police violence on crime reporting or how widespread the effects are. Therefore, this paper uses a novel dataset to address an important question: what is the impact of police violence on crime reporting over time. I utilize both county-level panel data as well as zip-code level 911 call data in Tennessee to isolate the effects of officer-involved fatalities (OIFs) on crime reporting in both the short and long run (days vs. weeks).

Specifically, I look at the impact of a fatal officer-involved encounter on domestic violence reporting. I chose to investigate domestic violence reporting specifically because this type of crime involves reporting family members. Victims of domestic violence and abuse might be less inclined to report an incident or call for help if there is tangible risk that law enforcement might shoot and kill a family member. Because domestic violence is already fraught with underreporting and barriers to seeking help, I might be concerned if police violence affects a victim's willingness to report domestic violence.

However, any noticeable effect might be mitigated by repetition and preestablished perceptions of the justice system. For example, it is possible that repetition of police violence desensitizes community members to displays of violence. After repeated exposure to police violence, an additional OIF might have little to no effect on crime reporting. This might be especially true in areas where incidents of police brutality and use of force are frequent.

I find little to no effect of OIFs on domestic violence reporting at the county level. However, using zip code-level 911 call data from Memphis, TN, I find some evidence of decreased 911 calls for domestic violence in neighborhoods where an officer-involved fatality took place. Further, I find that total 911 calls also decrease in affected zip codes and neighborhoods. The magnitude of the effects is dependent on the race of the OIF victim and cause of death.

This is the first paper to study the impact of officer-involved fatalities on the reporting of domestic violence. Previous research in this area is limited to one incident and/or one municipality. In this paper, I use 12 years of reporting data across 95 counties, making me better equipped to study the impact of fatal police shootings on crime reporting. One benefit of having so many years of data, is that I am able to analyze the effects of police shootings in a time before OIS's were more heavily reported in the media. Additionally, I am the first to use spatial 911 call data in multiple municipalities over many years. With this and zip code-level data, I am able to hone in on heterogeneous effects across different population groups within cities over time.

Background

It might not be surprising if officer-involved fatalities impact crime reporting. Recent high-profile cases of fatal police encounters with unarmed victims such as Walter Scott, Michael Brown, and Tamir Rice have led to televised protests and nationwide unrest. While the focus on the protests has been on the "racial bias in American policing," there is reason suspect these incidents and others could lead to widespread impacts on trust in the justice system and its enforcers (Blinder, 2017).[40] Subsequent Department of Justice investigations into high violence police

[40] Blinder, A. (2017, December 7). Michael Slager, Officer in Walter Scott Shooting, Gets 20-Year Sentence. Retrieved from https://www.nytimes.com/2017/12/07/us/michael-slager-sentence-walter-scott.html; What Happened in Ferguson? (2014, August 13). Retrieved from https://www.nytimes.com/interactive/2014/08/13/us/ferguson-missouri-town-under-siege-after-police-shooting.html; and Dewan, S., & Oppel, R. A. (2015, January 23). In Tamir

departments have revealed troubling results. Officers often fail to receive adequate training on the use of deadly force policy, leading officers to "engage in a pattern or practice of using force, including deadly force that is unreasonable" (Justice 2017). However, in police departments across the country, complaints warranting investigations are often ignored. This failure to provide accountability and justice can further erode public trust in law enforcement and the police department's ability to effectively prevent crime. Per the Justice Department, "trust and effectiveness in combating violent crime are inextricable intertwined (Justice 2017)."

Equality becomes a concern when minority or low-income citizens are disproportionately exposed to police violence. In the Department of Justice's investigation into the Chicago Police Department, raw statistics revealed that the CPD used force almost "ten times more often against blacks than against whites." This disparity in treatment has led to gaps in perceptions of police competency and fairness between races (Sigelman et al. 1997; Brunson 2007; Bor et al. 2018). Thus, heterogeneous perceptions of police across racial and income groups could impact the effect of police violence on citizen crime reporting.

In the paper most similar to this, Desmond et al. (2016) find some evidence of this by examining the impact of police brutality on 911 calls for service. They find that residents of black neighborhoods, relative to white neighborhoods, were less likely to report crime after Frank Jude's beating was broadcast publicly. However, this study is limited to one incident, the beating of Frank Jude, and one municipality, Milwaukee, WI. Similarly, the only other paper that utilizes 911 call data is Cohen et al. (2019). Using both fatal and non-fatal OIS incidents from Los Angeles County, they find no change in total 911 calls-for-service in the 30 days after an incident. Other research

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Rice Case, Many Errors by Cleveland Police, Then a Fatal One. Retrieved from https://www.nytimes.com/2015/01/23/us/in-tamir-rice-shooting-in-cleveland-many-errors-by-police-then-a-fatal-one.html.

investigating the impacts of police violence utilizes survey data, or in person interviews (Gingerich and Oliveros 2018; Carr et al. 2007; Kirk and Papachristos 2011).

While there are many channels through which police violence may affect crime reporting, I focus on two candidate mechanisms: deterrence and trust. The relationship between a community and law enforcement is inextricably intertwined with law enforcement's ability and effectiveness in combatting crime (Justice 2017). Incidents of police brutality have the potential to affect a community's perception and trust of its institutions, leading to less crime reporting. On the other hand, police brutality could also affect an individual with criminal tendencies and their proclivity to commit crimes. The possibility of an aggressive response by law enforcement may deter some criminal activity. This, in turn, might lead to lower crime reporting as a result of less criminal activity.

However, it is plausible that reporting of certain crimes is more affected by eroded trust than others, while deterrence may dominate for other crimes. For example, deterrence might impact robberies, vandalism, theft and other crimes committed with lower penalties. Alternatively crimes such as domestic violence and familial disputes might be more impacted by eroded trust rather than deterrence (Dunford et al. 1990). To date, no study has looked at the differential effects of police violence across different types of crime. If police violence erodes trust, this could further reduce crime reporting for already underreported crimes like domestic violence.

If we assume that police killings affect crime reporting, different mechanisms are likely to be more salient for different types of crime. For example, an individual's proclivity to call the police might be impacted by the severity of the crime being committed, who is committing the crime (family member or complete stranger), and where the crime is taking place. Focusing on domestic situations, it is possible that perceptions of police aggression might deter reporting in situation where the crime is committed by a relative.

It is unlikely that police violence deters offenders from committing acts of domestic abuse. Previous research on domestic violence demonstrates that offenders are not very responsive to deterrents such as increased penalties and mandatory arrest policies (Dunford et al. 1990). Most states have implemented mandatory arrest policies for domestic situation calls. If officers have probable evidence of domestic violence, they are obligated to arrest the aggressive party. However, this can also lead to a duel arrest of the both the offender and victim in situations where a victim might also demonstrate aggression by fighting back.

These mandatory arrest policies were implemented for two purposes: to demonstrate that domestic violence is a serious crime and perpetrators should face serious consequences through the justice system, and because these policies were assumed to have a deterrent effect on repeated acts of domestic abuse (Justice 2017). However, numerous studies have demonstrated that this last assumption does not hold, especially for victims who are unmarried and whose husbands are unemployed. In these situations, domestic violence acts might actually increase (Aizer 2010). Further, many studies have found links between poverty and unemployment on domestic abuse (Aizer 2011; de Olarte and Llosa 1999). This is also consistent with recent work by Lindo et al. (2018), who find that child maltreatment decreases with indicators for male employment but increases for female employment. Thus, increases in punitive costs are unlikely to affect an abuser’s proclivity to commit crimes. While I cannot separate the differential effects between lower crime rates and lower crime reporting in my datasets, the literature suggests that changes in domestic violence crime are due to changes in reporting rather than changes in crime rates.

However, there is little known about the factors affecting domestic violence reporting outside the household. This paper represents a significant improvement in both literatures on domestic violence reporting and police violence. Combining these two literatures, this paper addresses how outside factors, such as police violence, affects a victim's proclivity to report domestic abuse or call for help.

Data

The data for this study come from three sources: the Tennessee Bureau of Investigation's reporting website (CrimeInsight), Fatalencounters.org, and 911 calls for service obtained via the Freedom of Information Act. I use both the Tennessee crime data on domestic violence reports as well as 911 domestic violence calls to measure the impact of police violence on domestic violence reporting. The CrimeInsight data represents data generated from a report where charges were filed. Importantly, this data is only available at the county level. The 911 call data represents calls for help, which may or may not lead to the generation of crime reports. The 911 call data includes the address from where the call took place, giving me a more precise measurement of geographic effects at the zip code level.

TN Crime Insight Data

I obtained data on reported domestic violence crimes from the state of Tennessee's CrimeInsight website from 2001-2017.[41] This incident-based data is generated from reports filed by officers and listed as daily totals by crime type, geography, etc. Unlike the 911 calls-for-service

[41] State by year time trends are relatively flat with little variation in total incidents reported per year.

data, a report is only generated if an officer is dispatched to the scene, and a report filed. It is quite possible then that this data underrepresents the number of crimes taking place.

TN CrimeInsight categories all domestic violence incidents as “Domestic Situations” where there is a “Domestic Violence Victim.” Any report containing this flag is then listed under domestic violence reports. Within these reports, I focus on the subset of offense types related to Crimes Against Persons.[42] Offense types in this category range from stalking and intimidation to murder and rape. Figure 3.1 shows domestic violence incidents by offense type using 2018 data from TN Crime Insight.[43]

In 2018 alone, there were over 40,000 simple assaults, 9000 aggravated assaults, and 7,000 acts of intimidation reported. Simple assaults, the most common offense type, is classified as a misdemeanor. Aggravated assault, while less frequent, is a felony. However, domestic violence incidents are not always limited to a single offense/charge. For example, an alleged perpetrator can be charged with both aggravated assault and rape for the same incident. As a result, considering all types of offenses could result in the double-counting of domestic violence incidents. Therefore, I limit my analysis to two types of offenses, Simple Assaults and All Assaults (a combination of both simple and aggravated assaults).

I do this for multiple reasons. First, these two offense types are the most frequently reported and make up a vast majority of all domestic violence incidents. Second, assault of any type is usually categorized into either of these categories depending on the severity of the crime. Thus, I am much less likely to double count incidents when I limit my analysis to simple and aggravated assaults.

[42] Other offense types include Crimes Against Property and Crimes Against Society.
[43] All figures and tables for Chapter 3 are located in Appendix C.

More descriptive statistics for Simple and All Assaults can be found in Table 3.1 along with county-level economic data. There is an average of 256 Simple Assaults per county per year in TN. Of these assaults, 81 percent of the victims are white, and 18 percent are black. For All Assaults (simple and aggravated), there is an average of 376 incidents per county per year. 56 percent of these victims are white, 11 percent are black, and 33 percent are not racially identified. Across all counties in Tennessee, the Labor Force Participation rate is 44 percent with 40.6 percent of individuals classified as employed and 3.3 percent as unemployed.

Figure 3.2 shows domestic violence assaults (simple and aggravated) per 1000 residents by county. Here, I can see that domestic violence incidents are not limited to urban areas, but rather dispersed through both urban and rural areas. Shelby county has the most assaults per 1000 residents with 331 incidents. Figure 3.3, showing domestic violence simple assaults per 1000 residents by county paints a similar picture.

Fatal Encounters Data

For my second dataset, I utilize a publicly sourced database of officer-involved fatalities from fatalencounters.org. This data is crowdsourced using news articles to generate a list of officer-involved interactions that resulted in the death of a civilian.[44] Additionally, this dataset is not limited to officer involved shootings, but rather any type of incident between an officer and civilian that resulted in a fatality.

The exact location, date, and cause of death is provided for all 513 reported incidents in TN across a nineteen-year period (2000-2019). Causes of death fall into five major categories: 1) Beaten/Bludgeoned with instrument, 2) Burned/Smoke inhalation, 3) Chemical agent/Pepper

[44] If an OIF occurred and was never reported by the Media, it would not appear in this dataset.

spray, 4) Gunshot, and 5) Vehicle.[45] In my analysis I examine the impact of all causes of death as well as a separate analysis for Gunshot deaths only. Descriptive statistics for the fatal encounters data can be found in Table 3.2.

There is an average of 3.3 officer-involved fatalities (OIFs) per county per year in TN. The victims of these encounters are mostly white (41%) or black (24%). However, in 33 percent of the incidents, the race of the victim is not identified. Over 90 percent of the victims are male with an average age of 28 years old. The most common cause of death is Gunshot at over 76 percent. The next most common cause of death is Vehicle at 17 percent.

Figure 3.4 shows OIFs by county over this 20-year period (2000-2019). OIFs are most concentrated in the urban counties: Shelby, Davidson, Knox, and Hamilton county. Shelby county has the most officer-involved fatalities with 115 incidents between 2000-2019. There are also multiple counties, mostly rural, without a single reported OIF during this period.

911-Calls for Service Data

Finally, my third dataset includes 911-calls for service from the second largest city in Tennessee: Memphis.[46] For each call, I am able to identify the time, address, and reason for the call (domestic violence, traffic accident, etc.). Descriptive statistics for city-level data can be found in Table 3.3. This data is aggregated to the daily level and more location specific, offering a different measurement of domestic violence incidence. Therefore, if the effects of officer-involved fatalities are more localized within a county, I will be able to better observe changes in reporting behavior.

[45] There are more minor categories for cause of death with very few observations including: Asphyxiated/Restrained, Drowned, Fell from a height, Medical emergency, Tasered, and Undetermined.
[46] This data was acquired using the Freedom of Information Act (FOIA) per request for each individual city.

One important caveat to note in both of my domestic violence datasets is that I cannot identify who is reporting the incident. In both the incident reports and 911 calls, I only observe that a call was made, but not by who. It is very possible that a third-party observer could call the police if they witnessed an incident of domestic violence but were not personally the victim. However, according to the Department of Justice in their report on the Practical Implications of Current Domestic Violence Research, "most domestic violence reports are called in by victims, with victim report rates ranging from 59 to 93 percent" (Klein 2009). So, while third-party reporting most likely appears in my datasets, though unidentifiable, it likely constitutes a small portion of observed incidents.

Estimation Strategy

I begin my analysis by looking at county-level reported incidents of domestic violence. This data is more expansive than the 911 call data, covering all 95 counties in TN for 18 years. Here I am interested in whether an OIF effects domestic violence reporting within the same county. Then, I move onto the 911 call data from the four largest cities in Tennessee. While this data is less expansive, it is more detailed in reported geographic location, allowing me to narrow my investigation geographically. For this analysis I aggregate 911 calls to the zip code-level, examining the effects of an officer-involved fatality on domestic violence 911 calls within that same zip code.

I use the same identification strategies for both datasets. Starting with an event study framework, I use the following estimation strategy to determine the effects of an officer involved death/shooting on domestic violence reporting. Using indicator variables for the 7 days prior to an officer-involved civilian death and 7 days after, I am able to determine if there was a significant change in domestic violence incidents. In the estimation equation, y_{id} is the number of domestic violence incidents in county (zip code) i on date d. $FatalOIF_{ij}$ is an indicator of whether county (zip code) i experienced a fatal OIF on date j (estimates effects within a 14-day window around shooting at j=d). I also include county-level (zip code-level) control variables ($\mathbf{x}_{it}$), year fixed effects (θ_t), and county (zip code) fixed effects (c_i).

Event Study Estimation Strategy:

$$y_{id} = \left\{\sum_{j=d-7}^{d+7} FatalOIF_{ij} \cdot \beta_j\right\} + \mathrm{x}_{it}\gamma + c_i + \theta_t + u_{id} \qquad (1)$$

I further expand the event window by aggregating the data to a weekly level of domestic violence incidents and using a 9-week event window (4 weeks before and 4 weeks after an officer-involved fatality). This eliminates some noise from daily level variation in reports but also allows me to test if there are longer-term effects on reporting.

Next, I employ a difference-in-difference estimation strategy, analyzing the effects of an incident on within county (zip code) reporting in the 3-7 days that follow. In equation 2, y_{it} is the number of domestic violence reports in county (zip code) i at time t. I include an indicator variable, $Post_t$ for the four weeks after an incident and a treated county (zip code) indicator, $Treat_i$, for counties (zip codes) where the incident took place. My coefficient of interest is β_3, the interaction of $Treat_i$ and $PostOIF_t$. Similar to the event study, I include county-level (zip code-level) controls, county (zip code) fixed effects, and year/month fixed effects.

Difference-in-Difference Estimation Strategy:

$$y_{it} = \beta_0 + \beta_1 PostOIF_t + \beta_2 Treat_i + \beta_3 PostOIF_t * Treat_i + \beta_4 x_{it} + c_i + \theta_t + u_{it} \qquad (2)$$

Similar to the event study analysis, I also expand the difference-in-difference treatment window to the 4 weeks before and after an OIF, aggregating data to a weekly level. This allows me to test whether effects observed in the 3-7-day windows persist in the weeks after an incident.

Results

TN County-level Event Study

I begin the analysis by looking at the effects of an OIF on domestic violence reporting at the county level. Using variation between counties, I calculate an event window (14 days) in which an OIF occurs within a county using counties that do not have an OIF occurrence within the same timeframe as the control group. Creating daily indicator variables, I use an event study framework and regress the daily number of simple assaults and all assaults on them. Both dependent variables are population-based counts of reported domestic incidents per 1000 residents. I incorporate county controls for unemployment, population, and weather.[47] I also include day of the week, month, and county by year fixed effects.[48]

Figure 3.5 graphs the point estimates for daily reports of domestic violence simple assaults during the 14-day window using the 7th day before an officer-involved incident as the control. Figure 3.6 shows the same estimation using All Assaults as the dependent variable. Both figures show similar point estimates, indicating a response not statically different from zero.[49]

[47] Employment and population data are from the American Community Survey released by Census Bureau. Weather data is compiled by the National Oceanic and Atmospheric Administration (NOAA).
[48] It is important to include day of week fixed effects as domestic violence incidents fluctuate heavily throughout the week with more incidents falling on Saturday and Sunday.
[49] If I limit the event window to 3 days before to 3 days after and OIF, I observe no nonzero responses in domestic violence reports (Appendix, Figures 3.A.1 and 3.A.1)

To eliminate potential noise in the variation of daily level data, I aggregate calls to a weekly level. This reduces some of the daily variation in reports and offers a better picture of domestic violence reporting's responsiveness to police violence. Using the same event study estimation, I regress weekly indicators on reported domestic violence incidents using a nine-week time window (4 weeks before, the week of, and 4 weeks after an officer-involved fatality). Figures 3.7 and 3.8 show the point estimate using weekly measures of reported simple assaults and all assaults per 1000 residents, respectively.

Even with the weekly measurements, I still observe responses not statistically different from zero. From the figures, I observe no significant changes in reports for either Simple Assaults or All Assaults in the weeks after an officer-involved incident. However, I do observe very large confidence intervals the week of an incident, so perhaps the effect is more centralized in the week of the officer-involved fatality.

TN County-level Difference-in-Difference

I next turn to a difference-in-difference estimation strategy. Here I look at the effect of an officer involved fatality on reported incidents of domestic violence within the same county. I limit the treatment time to 10 days, 7 days, and 3 days after an incident takes place. Additionally, I divide the state into three regions: east, middle, and west. Counties in a region outside of the effected county that do not also have an officer involved fatality in the same time period are used as control counties.[50] This allows me to isolate the effect of an incident within a treated county.

[50] One might expect and officer involved fatality could have an effect on incidents in neighboring counties. Thus, by limiting the control counties to those outside the treated counties region, I reduce the likelihood of control counties being semi treated.

Table 3.4 shows the regression results for six different models, three with Simple Assaults per 1000 residents as the dependent variable and three models with All Assaults per 1000 residents as the dependent variable. All regression models include controls for weather (maximum temperature, minimum temperature, average temperature, and precipitation) and county-level socioeconomic status (percent unemployment). [51] I also include day of week, month, and county by year fixed effects in all models.

Post OIF x Treated County is the interaction of the time period after an officer involved fatality and the treated county in which the incident took place. Since I include indicators for whether the victim is white and whether the cause of death was listed as "Gunshot," the coefficient on this interaction can be interpreted as the average change in reported domestic simple assault when the victim of the OIF was nonwhite and the cause of death was not listed as "Gunshot." The coefficient on *Post OIF* indicates the changes in domestic violence incidents across all counties in the days after an OIF. The coefficient on *Treated County* shows changes in domestic violence incidents in the treated county in the 10, 7, or 3 days prior to an OIF taking place. Looking at Simple Assaults first, there are no significant changes in simple assaults in the 10 or 7 days after an OIF occurred in the county where the OIF took place. However, there a small, significant increase in the 3 days after and OIF. Additionally, on the right half of the table, where All Assaults is the dependent variable, there is a significant increase in All Assaults per 1000 residents in the 7 and 3 days after an OIF in the treated county. Since the mean of All Assaults per 1000 residents is 0.0143, an increase of 0.0013 represents a 22 percent increase in domestic violence reports in treated counties after an OIF. This effect is unchanged if the race of the OIF victim was reported as white/Caucasian or cause of death listed as "Gunshot."

[51] Weather data is from the National Oceanic and Atmospheric Administration, https://www.noaa.gov/.

In Table 3.4A, I limit the analysis to gunshot fatalities only. Here, I observe an increase in reports across all counties in the 3-7 days following an OIF. Additionally, the coefficient on *Post OIF x Treated County* is positive and significant in the 10 days after an OIF in the treated county. However, if the victim is white, reports decreased relative to OIF's where the victim was nonwhite. similar patterns across all variables of interest, but slightly lower magnitudes. Additionally, the level of unemployment has a positive significant effect on domestic violence reports in the 3 days after an OIF for All Assaults. This is consistent with previous work showing unemployment and domestic violence are positively correlated (Lindo et al. 2018).

As shown in Figure 3.4, OIF incidents are highly concentrated in Shelby, Davidson, Knox, and Hamilton counties. These counties experience multiple OIFs per year, which might create desensitization toward police violence. Thus, dropping these four counties from the analysis, might allow me to pick up on any effects in less-treated counties. Table 3.5 shows the same regression as Table 3.4, excluding data from the four most populated counties in Tennessee. Here, the coefficient on *Treated County* is negative and significant for the 3-7 days after an OIF when Simple Assaults is the outcome variable of interest and 3 days after and OIF when All Assaults is the outcome variable of interest. This indicates that counties where an OIF occurs experience significantly lower reports of domestic violence relative to untreated counties. Similar to Table 3.4, the coefficient of *Post OIF x Treated County* is positive and significant for both models using a 3-day time window. However, if the cause of death is listed as "Gunshot," I observe a significant decrease in All Assault reports relative to other causes of death listed in the 7-10 days after an OIF in the treated county.

Zip code-level Event Study

Using 911 call data affords me an alternate form of measurement for a typically underreported crime. 911 calls represent calls for help, which might be more frequent than reports filed. Further the 911 call data reports the address from which the call took place, allowing me to study potentially more localized effects of police violence on crime reporting. In this analysis, I use 911 call data from Memphis, TN.

I begin the analysis by studying the effect of an OIF on 911 calls across the city of Memphis. Memphis is an ideal city to begin analyzing zip code level data. First, Memphis has the longest panel set of 911 call data available. Second, Memphis has the highest number of OIFs (41) and largest population in the state.[52] Figure 3.9 shows the population density of each Memphis zip code in persons per square mile. While the downtown area is the most densely populated, the majority of 911 calls come from South and North Memphis rather than downtown. (Figure 3.10). Similarly, 911 calls for domestic situations are mostly from South and North Memphis (Figure 3.11).

Given this disparity in calls, I take a closer look at zip code level socioeconomic data. Figure 3.12 shows the percent of residents living below the poverty line by zip code. Within Memphis, this percentage varies significantly, ranging from 2-64 percent. Similarly, Figure 3.13 shows the unemployment rate by zip code. High rates of unemployment and poverty are usually associated with higher crime (Kelly 2000). So, it is perhaps, unsurprising, that 911 calls are more frequent in zip codes with higher poverty rates and unemployment.

Figure 3.14 shows officer-involved fatalities by zip code. While multiple zip codes never experience an OIF, one zip code has as many as 12 during this 9-year period. Similar to the 911

[52] This number of OIF's is smaller than the 115 listed earlier in Shelby county due to the difference 911 data available (20 years for county-level data, 9-years for 911 data).

calls, OIFs are more frequent in North and South Memphis zip codes, with fewer OIFs in the downtown area. However, visually, the zip codes with the most OIFs are not necessarily the zip codes with the highest poverty rates or 911 calls.

One thing to note is the racial segregation in Memphis geography. Memphis is majority black or African American (64.2%) in sharp contrast with the rest of Tennessee which is majority white (78.5%).[53] Within the city, the racial make-up of zip codes varies significantly. Figure 3.15 shows the percentage of residents who identify as Non-Hispanic/White. The percentage of white residents ranges from 2.4 to 90.2 percent. Comparing Figures 3.14 and 3.15, there is a noticeable similarity between majority non-white zip codes and zip codes with multiple OIFs.

Given this picture of Memphis, it is very likely that OIFs are more common in areas that are majority non-white, with high unemployment rates and more frequent 911 calls for service. Thus, in my analysis I use fixed effect to control for zip code level heterogeneity.

I use the same event study framework from the county-level analysis. Using a 14-day estimation window, I generate indicator variables for the 7 days before and 7 days after an OIF. I then regress daily 911 calls for domestic violence on those indicator variables. Figure 3.16 shows the coefficient estimates for the daily indicator variables. In the days after an officer-involved fatality, there is a slight, statistically significant increase in 911 calls for domestic violence on the 5th and 6th day afterwards. However, this estimation is rather noisy.

As a robustness check, I perform the same regression using the number of total daily 911 calls as the dependent variable. Figure 3.17 shows the coefficients for all daily indicator variables

[53] U.S. Census Bureau, QuickFacts, Memphis, TN (2019) https://www.census.gov/quickfacts/memphiscitytennessee

in the 7 days before and after an incident. Here again, there is a lot of noise in the daily estimation, but there is a positive significant increase in daily 911 calls 6 and 7 days after an OIF.

Due to noisiness in the daily measurements, I next turn to a week-level analysis. Aggregating the data to a weekly level eliminates a lot of variation and noise in the daily data. With the week-level analysis, I extend the treatment window to four weeks before and four weeks after an officer-involved fatality. Figures 3.18 and 3.19 show the coefficient estimates for weekly indicators before and after an incident. In Figure 3.18, there is a significant increase in 911 domestic violence calls one week after an OIF within that zip code. However, the weekly estimates are still quite noisy both before and after the OIF.

Figure 3.19 shows a significant decrease in 911 calls per 1000 residents in the 4th week after a shooting. However, a deeper examination of the data reveals multiple overlapping incidents in the 8-week time window, even within the same zip code. Thus, and event study might not be the best estimation strategy for this pattern of incidence. I next turn to a difference-in-difference estimation strategy.

Zip code-level Difference-in-difference

I begin by using a difference-in-difference estimation strategy similar to that in the county-level analysis. I start with a short time window, where *Post OIF* is equal to 1 in the 3 or 7 days after an officer-involved fatality. Table 3.6 shows the results of five regression models on domestic violence 911 calls in the 7 days after a shooting. Here, the difference-in-difference coefficient of interest is *Post OIF x Treated Zip code*. Because I include interactions based on whether the police victim was white, male, or the cause of death listed as “Gunshot,” the coefficient on *Post OIF x Treated Zip code* can be interpreted as the average effect of an OIF involving a nonwhite female where the cause of death was not “Gunshot” on domestic violence 911 calls within the treated zip code. There is no significant effect of an OIF on domestic violence 911 calls in the same zip code for any of the five models. However, the coefficient on Treat Zip code is negative and significant for models 3. This indicates that zip codes in which OIFs occur experience fewer 911 calls for domestic abuse. This effect drops out when I include zip code and zip code by year fixed effects.

Coefficients on *Percent Unemployed* and *Percent Below Poverty* are positive and significant, indicating zip codes where poverty and unemployment are higher experience more 911 calls for domestic abuse. These effects drop out when including zip code fixed effects.

Narrowing the event window to 3 days before and after an OIF, Table 3.6A shows the results of five regression models on domestic violence 911 calls in the 3 days after a shooting. The coefficient on *Treated Zip code* is negative and significant for all models, indicating zip codes where OIFs occur experience fewer 911 calls for domestic abuse. The coefficient on *Post OIF x Treated Zip code* is positive and significant, meaning 911 calls for domestic violence increased in the 3 days after an OIF. [54] Additionally, models 1-3 indicate that if the victim was male, 911 calls were significantly lower relative to OIFs where the victim was female. This effect does not persist in models 4 and 5 where fixed effects for zip code and zip code by year are included.

As a robustness check, I use the same estimation strategy but with total 911 calls as the dependent variable. Tables 3.7 and 3.7A show these regression results for 7-day and 3-day treatment windows, respectively. Again, my coefficient of interest is *Post OIF x Treated Zip code*. In Table 3.7, there is no significant change in 911 calls within the zip code where and OIF took place in the 7 days after the incident. However, the coefficient on *Race of Police Victim: White* is negative and significant in model 2. Thus, 911 calls in the treated county in the 7 days

[54] The mean of daily 911 domestic violence calls per 1000 residents is 0.341.

after an OIF where the victim was white are 9 percent lower than OIFs where the victim was nonwhite.[55] Additionally, models 1-3 show a large, negative significant coefficient on *Treated Zip code*, similar to Tables 3.6 and 3.6A. Finally, the coefficients on *Percent Below Poverty* are positive and significant, indicating zip codes with higher rates of poverty experience significantly more 911 calls.

Table 3.7A shows the effect of and OIF on 911 calls for service during a 3-day post-fatality window. The coefficient on *Post OIF x Treated Zip code* is negative and significant for models 2-5, indicating zip codes that experience an OIF saw a decrease in 911 calls in the 3 days afterward. The coefficient in model 5, -0.7441 indicates a 16 percent decrease in 911 calls. However, the coefficient on Gender of Police Victim: Male is positive and significant. Thus, treated counties in which the victim of the OIF is male experiences a smaller decrease in 911 calls that those where the victim is female (911 calls decrease only 5 percent). Additionally, the coefficient on Treated Zip code is positive and significant, indicating zip codes that experience an OIF also experience more 911 calls.

Similar to the county-level analysis, I aggregate the data to the week level to reduce the noisiness of daily measure. Using an 8-week and 4-week time window, I use a difference-in-difference estimation strategy to study the effects of an OIF on domestic violence 911 calls in the 2-4 weeks after an incident. For the 8-week window, I generate a treatment variable equal to 1 during the four weeks after an OIS. Similarly, I generate a dummy variable for calls made in the same zip code as an OIS incident. Again, these calls are translated into calls per 1000 residents in a zip code.

[55] The mean of daily 911 calls per 1000 residents is 4.615.

Results for the 8-week window are presented in Table 3.8. The coefficient on *Post OIF x Treated Zip code* is negative and significant for models 1-3, indicating zip codes that experience an OIF saw a decrease in domestic violence 911 calls in the 4 weeks after an incident. However, the coefficient on Post OIF is negative and significant for all models, indicating 911 calls for domestic violence decreased in the 4 weeks after an OIF for all zip codes.

When I shorten the event window to 4 weeks in Table 3.8A, looking at the two weeks before and two weeks after an OIF, I find similar results. The coefficient on *Post OIF x Treated Zip code* is again negative and significant for models 1-3. However, now the coefficient on *Treated Zip code* is positive and significant, indicating zip codes where OIFs occur experience more 911 calls for domestic violence than those that do not experience an OIF. Additionally, the coefficients for *Gender of Police Victim: Male* and *Cause of Death: Gunshot* are positive and significant, meaning OIFs where the victim was male or the cause of death listed as "Gunshot" experienced relatively more 911 calls for domestic violence in the 2 weeks after an incident relative to OIFs where the victim was female and cause of death not listed as "Gunshot."

To determine whether an OIF affected crime reporting across all types of crime, I utilize the same analysis for a week-level estimation but with total 911 calls as the dependent variable. I again use event windows of 8 and 4 weeks. These results are displayed in Tables 3.9 and 3.9A respectively. In Table 3.9, 911 calls decreased in the 4 weeks after an officer-involved fatality by approximately 22 percent (models 3 and 4) across all zip codes.[56] The coefficient on *Post OIF x Treated Zip code* is negative and significant for models 1 and 2. However, this effect drops out when zip code and zip code by year fixed effects are incorporated. In model 5, *Race of Police*

[56] The mean of weekly 911 calls by zip code is 3.808 calls per 1000 residents.

Victim: White is negative and significant, indicating if the OIF victim was white, 911 calls in treated counties decreased more relative to OIFs where the victim was nonwhite.

However, when I shorten the window to two weeks before and after an OIF, I find that OIFs have a negative significant effect on 911 calls in treated zip codes. The coefficient in models 1-4 on *Post OIF x Treated Zip code* indicates treated zip codes with OIFs where the victims is a nonwhite female where cause of death was not listed as “Gunshot,” experienced a 25 percent decrease in 911 calls during the 2 weeks after an OIF. However, in models 3 and 4, if the victim was white, treated counties experienced a smaller decrease in 911 calls of only 6 percent.

Neighborhood-level Difference-in-difference

Tables 3.10 and 3.11 show that the effects of an OIF in one zip code may have widespread effects outside that particular zip code, meaning neighboring zip codes may be impacted as well. To broaden this analysis and potential capture more wide-spread effects, I extend my analysis to include neighborhood effects. Grouping zip codes into “neighborhoods” as defined by the Memphis Chamber, I create neighborhood level indicators for treatment and control groups.[57] Using newly defined treatment and control groups, I estimate the effects of an OIF occurring in a particular neighborhood on domestic violence and total 911 calls. Tables 3.10-3.11 display these estimations examining neighborhood-level effects.

The coefficient on *Post OIF x Treated Neighborhood* is negative and significant in all models, indicating OIFs where the victim is a nonwhite female where the cause of death is not listed as “Gunshot” decreases 911 calls for domestic violence by 14 percent. Additionally, neighborhoods that are treated with OIFs experience significantly higher volumes of 911 calls for

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[57] Neighborhoods include both single and multiple zip codes as defined by the Memphis Chamber https://memphischamber.com/welcome-to-memphis/live-memphis/memphis-neighborhoods/

domestic violence. Finally, the coefficient on *Post OIF* is negative and significant for all models, indicating there is still a city-wide effect across all zip codes of an OIF. Domestic violence 911 calls decrease by 6 percent in the four weeks after an OIF.

Table 3.10A limits the time window to 4 weeks, two before and two after an OIF. Results are similar to that of Table 3.10. Additionally, the coefficient on Gender of Police Victim: Male is positive and significant. Thus, neighborhoods with male victim OIFs saw less of a decrease in domestic violence calls relative to female victims.[58]

Similar to Table 3.9, Table 3.11 shows estimations of the effect of an OIF on total 911 calls with an 8-week time window. The coefficient on *Post OIF x Treated Neighborhood* is negative and significant for all models, demonstrating a decrease in 911 calls in treated neighborhoods after an OIF incident. This effect holds when limiting the time window to 4 weeks in Table 3.11A.

Discussion

This paper is the first to investigate the impact of officer-involved fatalities (OIFs) on domestic violence reporting. Additionally, this is the first analysis of repeated OIFs at the county and zip code level with panel data across as many as 20 years. I find little to no effect of OIFs on domestic violence reporting at the county level. However, using zip code-level 911 call data from Memphis, TN, I find some evidence of decreased 911 for domestic violence across all zip codes in the weeks after an OIF.

When I divide zip codes into neighborhoods, I find that an OIF has a significant negative effect on domestic violence calls within the treated neighborhoods. Additionally, I still observe an overall decrease in domestic violence calls across all neighborhoods in the weeks after an OIF. While the

[58] In some models, like model 4, male victims have a positive significant effect on 911 calls for domestic violence.

effects of an OIF may be more widespread than neighborhood or zip code, 911 calls for domestic violence decrease even further in treated neighborhoods. Additionally, I find that neighborhoods treated by an OIF have higher volumes of 911 calls for domestic violence and total 911 calls.

I also find that total 911 calls decrease significantly in the days/weeks after an OIF in treated zip codes. This effect continues when I extend the analysis to neighborhoods. Thus, the level of overall crime reporting decreases in treated neighborhoods/zip codes after an OIF.

These results demonstrate that officer interactions with citizens could affect an individual's proclivity to report crimes. Further analysis will include additional data from the other 3 largest cities in Tennessee. Including these might offer more insight into potential long run effects.

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Conclusion

In Chapter 1, I use a laboratory experiment to compare three popular point-of-sale solicitation methods: a fixed donation request (yes or no to a randomly assigned amount); a rounding request (yes or no to an endogenous amount); and an open-ended solicitation. I find that, at amounts less than $1, participants in the rounding treatments were much more likely to donate. Holding fixed the amount of the ask, differences in donation rates between the rounding and fixed request treatments appear to be driven by "loose-change effects," whereby individuals are more likely to donate if they would have less change as a result. Participants in the fixed request treatments exhibited higher mean willingness-to-donate than those in the open-ended. Last, a one sentence information statement about the charity has positive but small effects on donation rates and amounts in the fixed request treatment.

In Chapter 2, I study key aspects of fundraising campaigns that utilize goals or provision points that must be met in order to provide a good or service. I find that the addition of an intermediate goal decreases the likelihood of reaching the final goal. Moreover, the level of the intermediate goal (holding payoffs for reaching the goal fixed) has no discernible effect on whether the intermediate or final goal is reached. This suggests a possible strategy whereby the campaign designer includes a final goal as a decoy in order to reach the desired "intermediate" goal. Value uncertainty has a negative significant effect on the likelihood of reaching the goal when only one goal is present. Finally, goal uncertainty has a positive significant effect on contributions.

In Chapter 3, I examine the effects of officer-involved fatalities, including officer-involved shootings, on domestic violence reporting. I find little to no effect of OIFs on domestic violence reporting at the county level. However, using zip code-level 911 call data from Memphis, TN, I find some evidence of decreased 911 calls for domestic violence in neighborhoods where an officer-involved fatality took place. Further, I find that total 911 calls also decrease in affected zip codes and neighborhoods. The magnitude of the effects is dependent on the race of the OIF victim and cause of death.

Appendices

Appendix A.

Chapter 1 Tables and Figures

Table 1.1 Mechanisms by Treatment

	Fixed Request	Rounding	Open-ended
No Information	T1	T2	T3
Information	T4	T5	T6

Table 1.2 Data Description

Variable name	Description	Mean	Std. Dev.
Male	=1 if identified gender is male	0.574	0.495
Age	age, in years	20.478	2.244
Earnings	earnings from experiment session, in $	22.496	5.009
Recently Donated	=1 if participant recently donated to charity	0.658	0.475
Gave	=1 if participant donated	0.491	0.500
Donation Amount	monetary amount donated to charity	0.418	0.748
Fixed Request	=1 if donation ask was a fixed request	0.346	0.476
Fixed Request Info	=1 if donation ask was a fixed request with information	0.310	0.463
Rounding	=1 if donation ask was rounding	0.099	0.299
Rounding Info	=1 if donation ask was rounding with information	0.102	0.302
Open-ended	=1 if donation ask was open-ended	0.074	0.261
Open-ended Info	=1 if donation ask was open-ended with information	0.069	0.254
50 cent	=1 if donation ask was 50¢	0.190	0.392
75 cent	=1 if donation ask was 75¢	0.171	0.377
Information	=1 if donation ask contained information about the charity	0.481	0.500

Table 1.3 Treatment Statistics

Treatment	Fixed Request		Rounding		Open-ended	
Information	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
N	310	278	89	91	66	62
N Gave	115	126	73	72	25	29
Mean Give Rate	37.09%	45.32%	82.02%	79.12%	37.88%	46.77%
Mean Donation	\$0.29	\$0.38	\$0.41	\$0.39	\$0.83	\$0.81
Mean Donation (if Gave)	\$0.79	\$0.84	\$0.50	\$0.50	\$2.49	\$1.68

Table 1.4 Analysis of donation rates

Dependent variable: *Gave*	(1) Model 1	(2) Model 2
Fixed Request	-0.008	0.010
	(0.066)	(0.067)
Fixed Request with Info	0.074	0.092
	(0.067)	(0.068)
Rounding	0.441***	0.417***
	(0.073)	(0.074)
Rounding with Info	0.412***	0.396***
	(0.074)	(0.075)
Open-ended with Info	0.089	0.075
	(0.087)	(0.087)
Earnings		0.017***
		(0.003)
Age		0.010
		(0.006)
Gender		0.015
		(0.032)
Recently Donated		-0.104***
		(0.022)
Constant	0.379***	-0.092
	(0.060)	(0.168)
Observations	896	891
R-squared	0.105	0.136
F-statistic	26.96	21.42

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for both models use robust standard errors.

Table 1.5 Mechanism Comparison at Amounts under $1

Depended Variable: *Gave*	Model 1	Model 2	Model 3
Fixed Request	0.196***	0.198***	0.220***
	(0.053)	(0.054)	(0.053)
Rounding	0.422***	0.429***	0.418***
	(0.052)	(0.053)	(0.054)
Information	0.022	0.019	0.017
	(0.039)	(0.039)	(0.039)
50cent		0.010	0.007
		(0.047)	(0.047)
75cent		-0.144***	-0.131***
		(0.048)	(0.048)
Earnings			0.016***
			(0.004)
Age			-0.000
			(0.008)
Gender			0.007
			(0.040)
Recently Donated			-0.072***
			(0.026)
Constant	0.372***	0.415***	0.115
	(0.047)	(0.054)	(0.196)
N	574	574	572
R^2	0.096	0.114	0.134
F-Statistic	23.69	16.66	13.71

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for all models use robust standard errors.

Table 1.6 Loose-change Effects by Mechanism

Potential change leftover post donation relative to current change	Donation Rate	
	Fixed Request	Rounding
Less	81.51%	79.47%
More	38.78%	--
Fixed Request Less = Fixed Request More	p = 0.000	
Fixed Request Less = Rounding	p = 0.555	

Note: The p-value for each test is from a chi2 test of donation rates.

Table 1.7 Analysis of Donation Amount by Treatment

Dependent variable: *Donation Amount*	(1) Model 1	(2) Model 2
Fixed Request	-0.536***	-0.515**
	(0.204)	(0.203)
Fixed Request with Info	-0.449**	-0.428**
	(0.205)	(0.204)
Rounding	-0.419**	-0.437**
	(0.204)	(0.203)
Rounding with Info	-0.437**	-0.446**
	(0.204)	(0.204)
Open-ended with Info	-0.015	-0.031
	(0.263)	(0.261)
Earnings		0.020***
		(0.005)
Age		0.008
		(0.008)
Gender		-0.023
		(0.050)
Recently Donated		-0.078**
		(0.030)
Constant	0.830***	0.347
	(0.202)	(0.297)
Observations	896	891
R-squared	0.052	0.067
F-statistic	4.457	4.507

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for both models use robust standard errors.

Table 1.8 Estimation of Willingness-to-Donate

Dependent variable: *Donation Amount*	Model 1	Model2
Fixed Request	0.198	0.243*
	(0.141)	(0.141)
Information	0.101	0.116*
	(0.068)	(0.068)
Earnings		0.034***
		(0.007)
Age		0.022
		(0.015)
Gender		0.019
		(0.068)
Recently Donated		-0.074*
		(0.044)
Intercept	0.821***	0.792***
	(0.132)	(0.131)
Standard deviation function (σ):		
Fixed Request	0.497	0.606*
	(0.330)	(0.322)
Constant	2.273***	2.208***
	(0.300)	(0.288)
Observations	716	712
Log pseudolikelihood	-693.435	-679.975

Note: Observations for the Rounding treatment are not included in this regression. All sociodemographic variables are demeaned so that the intercept can be interpreted as the estimated mean WTP for the Fixed Request treatment in all models. *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for both models use robust standard errors.

Table 1.9 Tests of information effects

	Pooled	*Fixed Request*	*Rounding*	*Open-ended*
H_0: Info donations = No Info donations	$0.06	$0.09*	$0.00	$0.02
H_0: Info donation rate = No Info donation rate	7.46%**	8.23%**	0.19%	8.90%

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively

Table 1.10 Information Effects on Donation Amount

Dependent variable: *Donation Amount*	(1) Model 1	(2) Model 2	(3) Model 3
Information	0.054	-0.015	-0.031
	(0.050)	(0.263)	(0.261)
Fixed Request		-0.536***	-0.515**
		(0.204)	(0.203)
Fixed Request*Information		0.102	0.118
		(0.267)	(0.265)
Rounding		-0.419**	-0.437**
		(0.204)	(0.203)
Rounding*Information		-0.002	0.022
		(0.266)	(0.264)
Earnings			0.020***
			(0.005)
Age			0.008
			(0.008)
Gender			-0.023
			(0.050)
Recently Donated			-0.078**
			(0.030)
Constant	0.392***	0.830***	0.347
	(0.036)	(0.202)	(0.297)
Observations	896	896	891
R-squared	0.001	0.052	0.067
F-statistic	1.15	4.46	4.51

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for all models use robust standard errors.

Table 1.11 Information Effects on Donation Rate

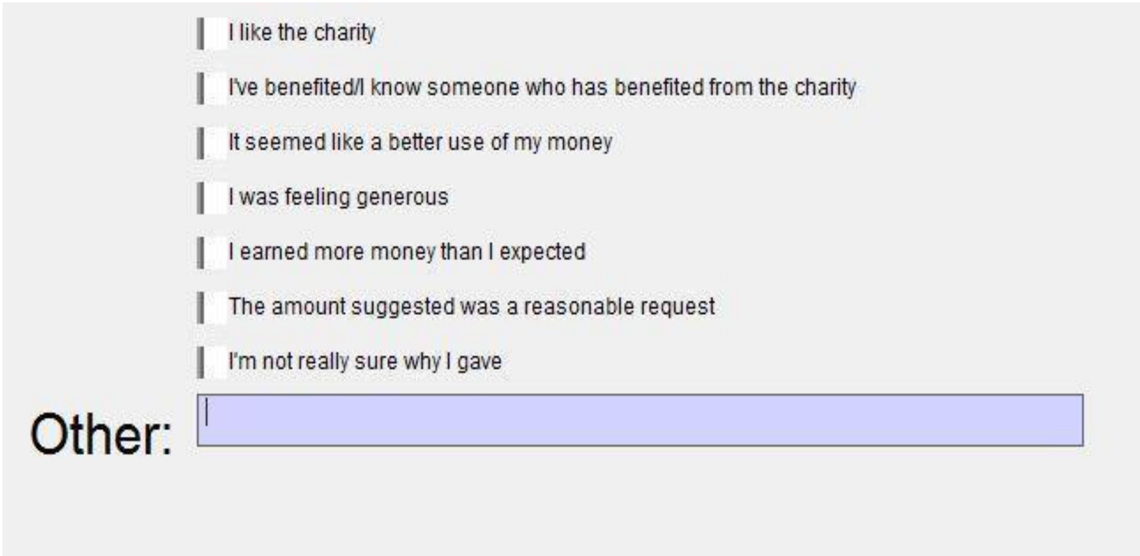
Dependent variable: *Gave*	(1) Model 1	(2) Model 2	(3) Model 3
Information	0.069**	0.089	0.075
	(0.033)	(0.087)	(0.087)
Fixed Request		-0.008	0.010
		(0.066)	(0.067)
Fixed Request*Information		-0.007	0.008
		(0.096)	(0.096)
Rounding		0.441***	0.417***
		(0.073)	(0.074)
Rounding*Information		-0.118	-0.095
		(0.105)	(0.105)
Earnings			0.017***
			(0.003)
Age			0.010
			(0.006)
Gender			0.015
			(0.032)
Recently Donated			-0.104***
			(0.022)
Constant	0.458***	0.379***	-0.092
	(0.023)	(0.060)	(0.168)
Observations	896	896	891
R-squared	0.005	0.105	0.136
F-statistic	1.15	4.46	4.51

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for all models use robust standard errors.

Table 1.12 Enjoyment

	No	Indifferent	Yes	Total
Donation	2.6%	15.8%	30.8%	49.2%
No Donation	10.9%	27.3%	12.6%	50.8%
Total	13.5%	43.1%	43.4%	100%

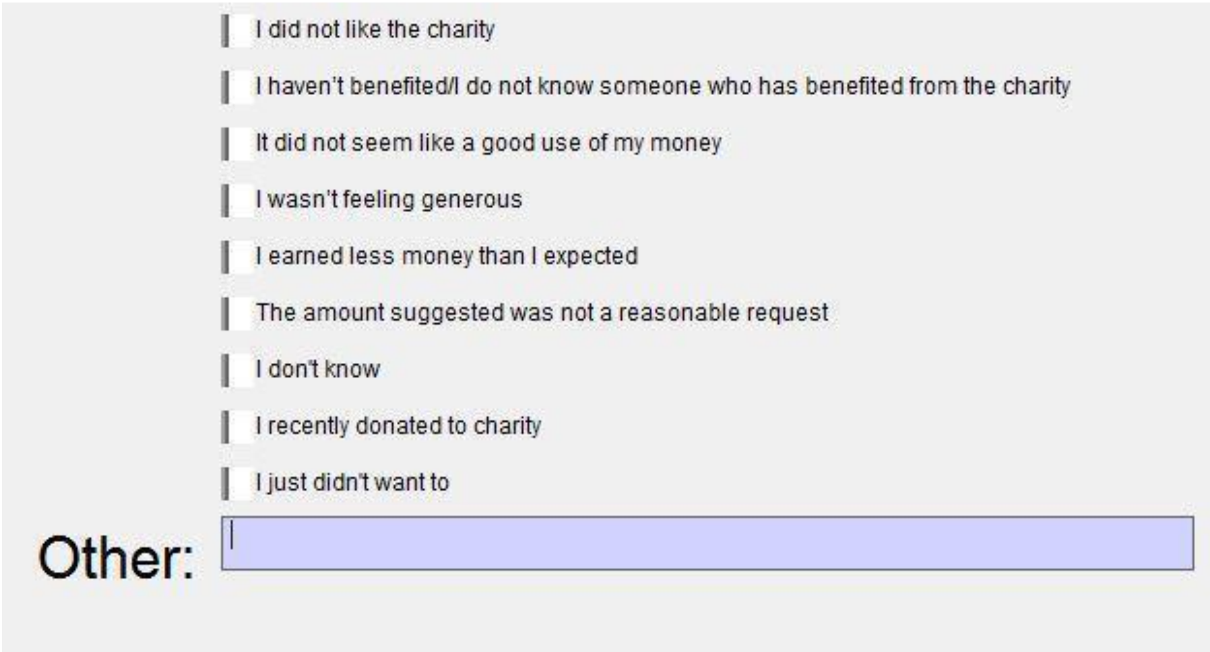
Figure 1.1 Screen presented to subjects in the Fixed Request mechanism with no information



- I like the charity
- I've benefited/I know someone who has benefited from the charity
- It seemed like a better use of my money
- I was feeling generous
- I earned more money than I expected
- The amount suggested was a reasonable request
- I'm not really sure why I gave

Other:

Figure 1.2 Reasons for Donating



- I did not like the charity
- I haven't benefited/I do not know someone who has benefited from the charity
- It did not seem like a good use of my money
- I wasn't feeling generous
- I earned less money than I expected
- The amount suggested was not a reasonable request
- I don't know
- I recently donated to charity
- I just didn't want to

Other:

Figure 1.3 Reasons for Not Donating

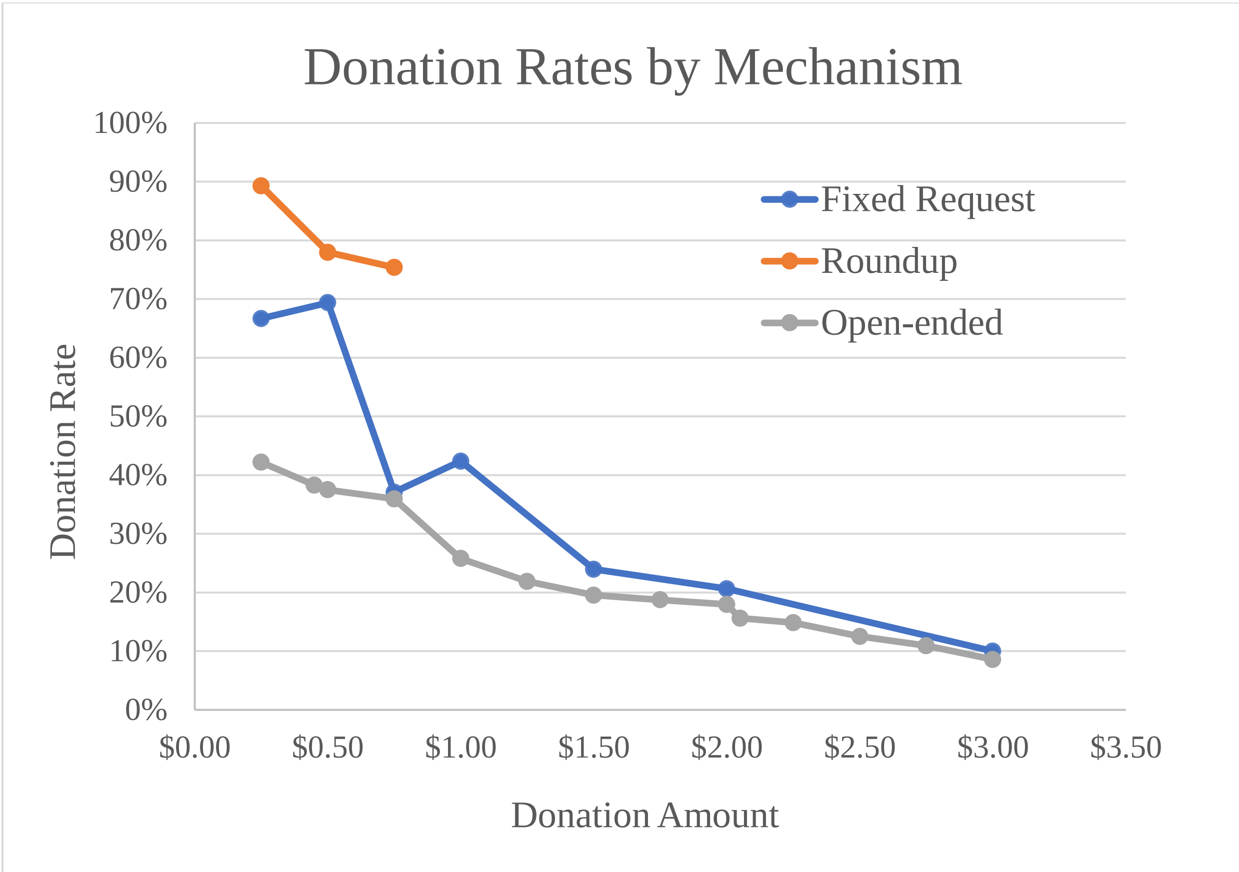


Figure 1.4 Donation Rates by Mechanism

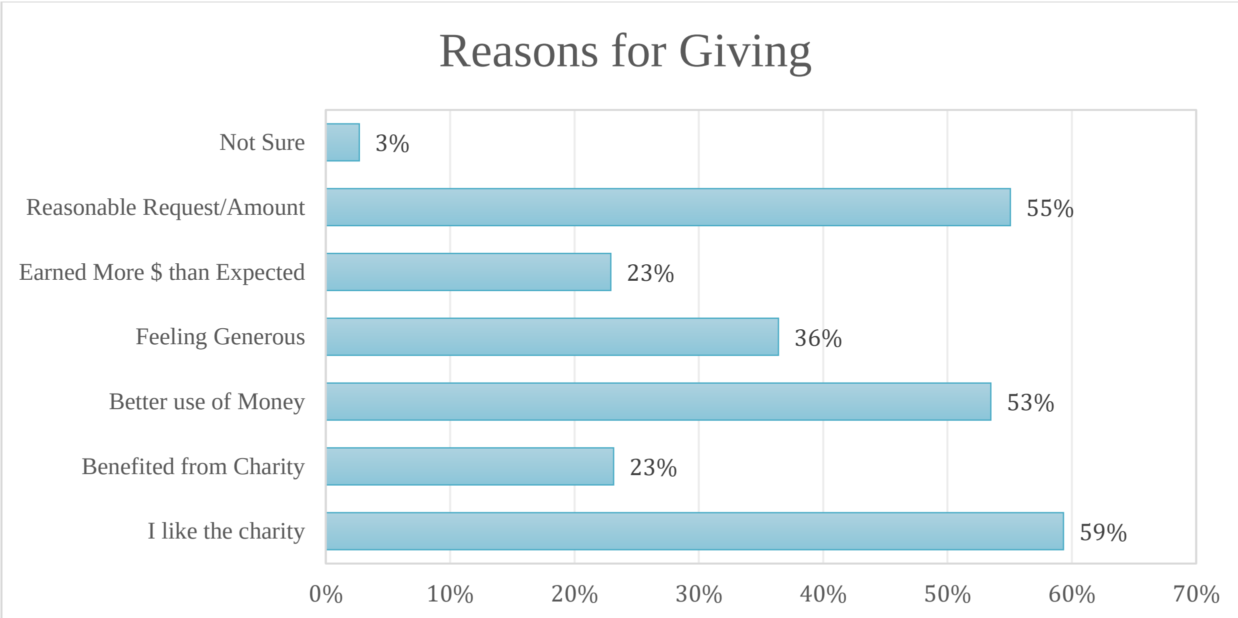


Figure 1.5 Reasons for Giving

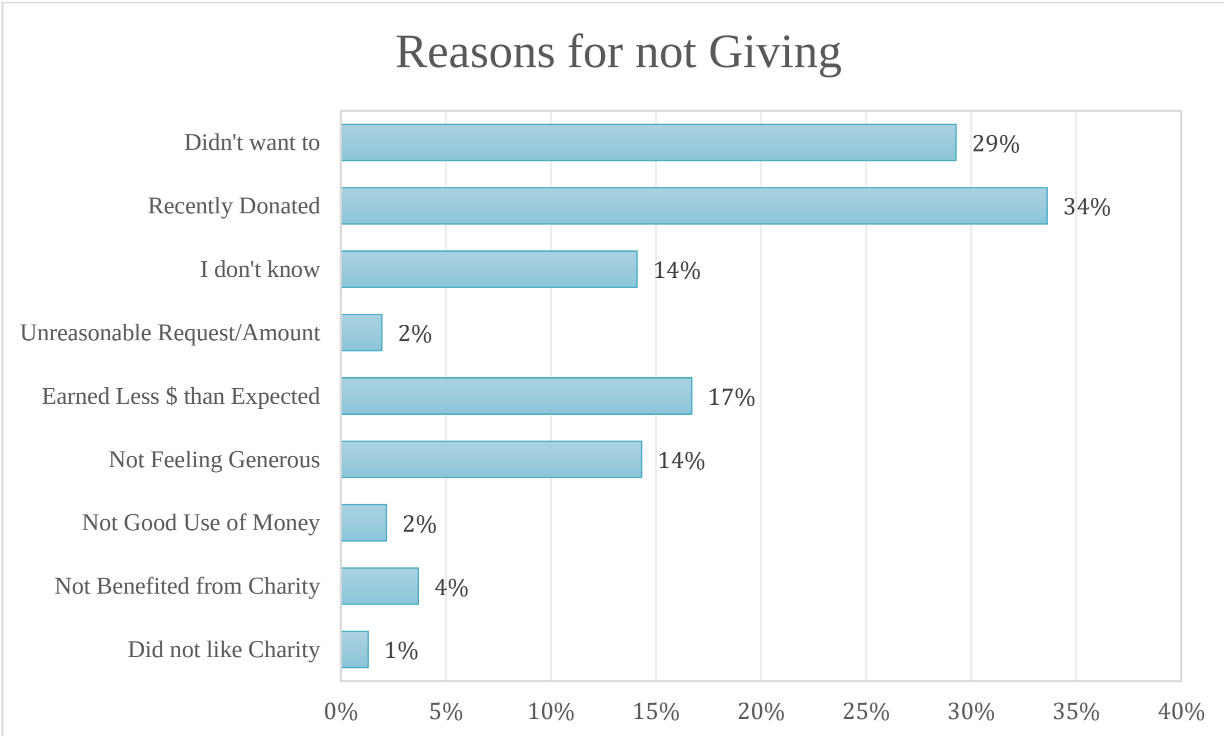


Figure 1.6 Reasons for Not Giving

Appendix B.

Chapter 2. Tables and Figures

Table 2.1 Experimental Design

	One Goal	Two Goals	Two Goals (Uncertain final goal)
Certain values	T1	T2	T3
Uncertain values	T4	T5	T6

Table 2.2 Goals by Treatment and Scenario

	One Goal	Two Goals, Known		Two Goals, 2nd Unknown	
	Final	Intermediate	Final	Intermediate	Final
Scenario 1	8	2	8	4	8 or 16
Scenario 2	8	4	8	4	8 or 16
Scenario 3	8	4	10	6	8 or 16
Scenario 4	10	4	12	6	8 or 16
Scenario 5	10	4	14	4	10 or 14
Scenario 6	10	4	16	4	10 or 14
Scenario 7	12	6	8	6	10 or 14
Scenario 8	12	6	10	6	10 or 14
Scenario 9	12	6	12	4	12 or no goal
Scenario 10	14	6	16	4	12 or no goal
Scenario 11	14	6	14	6	12 or no goal
Scenario 12	14	8	10	6	12 or no goal
Scenario 13	16	8	12	4	16 or no goal
Scenario 14	16	8	14	4	16 or no goal
Scenario 15	16	8	16	6	16 or no goal
Scenario 16				6	16 or no goal

Note: in cases where the final goal is uncertain, the two outcomes have a 50% chance of being drawn. In a case when "no goal" is chosen, the fundraising campaign stops after the intermediate goal is reached.

Table 2.3 Data Description

Variable name	Description	Mean	Std. Dev.
Male	=1 if identified gender is male	0.592	0.492
Age	age, in years	20.136	1.681
Earnings	Earnings from experiment session, in $	23.728	2.093
Funded1	=1 if intermediate goal is reached	0.652	0.466
Funded2	=1 if final goal is reached	0.737	0.202
GPA	cumulative GPA; midpoint of selected range	3.335	0.460
Employed	=1 if participant has a part-time or full-time job	0.351	0.478
Risk	number of Lottery A (safe) choices selected in risk MPL	5.380	1.653
Risk Averse	=1 if number of Lottery A (safe) choices >5 in risk MPL	0.519	0.500
One Goal	=1 if treatment is one goal with known values	0.174	0.380
One Goal, UV	=1 if treatment is one goal with unknown values	0.163	0.370
Two Goals, Known	=1 if treatment is two goals with known values	0.163	0.370
Two Goals, 2nd Unknown	=1 if treatment is two goals with unknown values	0.163	0.370
Two Goals, Known UV	=1 if treatment is two goals, 2nd unknown with known values	0.163	0.370
Two Goals, 2nd Unknown UV	=1 if treatment is two goals, 2nd unknown with unknown values	0.174	0.380
No Goal 2	=1 if No Goal was selected treatment =T3 or T6	0.090	0.141
No Goal Possible	=1 if scenario used No Goal as an option and treatment =T3 or T6	0.159	0.223
Value Uncertainty	=1 if the payout is uncertain	0.500	0.501
Final Goal	Number of tokens needed for provision at the final level	11.590	0.789
Intermediate Goal	Number of tokens needed for provision at the first level	3.458	2.479
Total Group Contributions	Number of tokens contributed by the group	10.742	1.286
Individual Contributions	Number of tokens contributed by the individual	2.690	0.914
Order1	=1 if order of randomized order of rounds was order 1	0.224	0.401
Order2	=1 if order of randomized order of rounds was order 2	0.245	0.413
Order3	=1 if order of randomized order of rounds was order 3	0.255	0.418
Order4	=1 if order of randomized order of rounds was order 4	0.214	0.395

Note: All variables are presented at the individual level with the exception of total group contributions.

Table 2.4 Analysis of fundraising success: group contributions

Dependent Variable: *Total Group Contributions*	(1) Model 1	(2) Model 2
Two Goals, Known (T2)	-1.174*** (0.274)	-1.316*** (0.305)
Two Goals, 2nd Unknown (T3)	-0.275 (0.499)	-0.246 (0.508)
One Goal, Value Uncertainty (T4)	-0.107 (0.070)	-0.273* (0.160)
Two Goals, Known, Value Uncertainty (T5)	-1.147*** (0.330)	-1.261*** (0.347)
Two Goals, 2nd Unknown, Value Uncertainty (T6)	-0.679** (0.292)	-0.776** (0.295)
Age		0.139 (0.138)
Gender		-0.456 (0.402)
GPA		0.274 (0.241)
Risk Averse		-0.918** (0.403)
Controls for order effects?	No	Yes
N	1163	1163
R^2	0.022	0.025
F-statistic	7.07***	3.70***

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively. Observations where the "no goal" option was drawn are not directly comparable and are excluded from these regressions.

Table 2.5 Analysis of fundraising success: group contributions, by goal structure

Dependent variable: *Total Group Contributions*	**One Goal**	**Two Goals, Known**	**Two Goals, 2nd Unknown**
Final Goal	0.968*** (0.015)	0.567*** (0.066)	0.656*** (0.057)
Value Uncertainty	-0.107 (0.071)	-0.032 (0.432)	0.450 (0.544)
Intermediate Goal		0.129** (0.060)	-0.023 (0.162)
No Goal Possible			-1.258*** (0.451)
N	465	450	355
R^2	0.930	0.303	0.291
F-statistic	2114.75***	47.25***	39.42***

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively. Observations where the "no goal" option was drawn are not directly comparable, and are excluded from these regressions.

Table 2.6 Analysis of fundraising success: final goal

Dependent Variable: *Final Goal Funded*	(1) Model 1	(2) Model 2
Two Goals, Known (T2)	-0.187*** (0.050)	-0.215*** (0.048)
Two Goals, 2nd Unknown (T3)	-0.167** (0.065)	-0.188** (0.057)
One Goal, Value Uncertainty (T4)	-0.058* (0.033)	-0.098** (0.038)
Two Goals, Known, Value Uncertainty (T5)	-0.165*** (0.048)	-0.216*** (0.048)
Two Goals, 2nd Unknown, Value Uncertainty (T6)	-0.120*** (0.044)	-0.157*** (0.049)
Age		-0.010 (0.018)
Gender		-0.180*** (0.058)
GPA		0.016 (0.034)
Risk Averse		-0.186*** (0.060)
Controls for order effects?	No	Yes
N	1163	1163
R^2	0.032	0.064
F-statistic	5.97***	3.53***

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively. Observations where NoGoal2=1 are excluded from the regressions.

Table 2.7 Analysis of fundraising success: final goal, by goal structure

Dependent variable: *Final Goal Funded*	**One Goal**	**Two Goals, Known**	**Two Goals, 2nd Unknown**
Final Goal	-0.009* (0.005)	-0.053*** (0.009)	-0.054*** (0.008)
Value Uncertainty	-0.058* (0.034)	0.022 (0.065)	0.064 (0.073)
Intermediate Goal		0.007 (0.012)	-0.010 (0.017)
No Goal Possible			-0.129** (0.061)
N	465	450	355
R^2	0.012	0.117	0.159
F-statistic	4.15**	14.83***	21.77***

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively. Observations where NoGoal2=1 are excluded from the regression in the last column.

Table 2.8 Analysis of fundraising success: intermediate goal

Dependent Variable: Intermediate Goal Funded	(1) Model 1	(2) Model 2
Two Goals, 2nd Unknown (T3)	0.014 (0.017)	0.013 (0.016)
Two Goals, Known, Value Uncertainty (T5)	0.000 (0.020)	-0.007 (0.017)
Two Goals, 2nd Unknown, Value Uncertainty (T6)	0.019 (0.016)	0.016 (0.015)
Age		-0.003 (0.005)
Gender		-0.017 (0.021)
GPA		0.021* (0.012)
Risk Averse		-0.019 (0.017)
Controls for order effects?	No	Yes
N	946	946
R^2	0.001	0.007
F Statistic	0.99	1.25

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively.

Table 2.9 Analysis of fundraising success: intermediate goal, by goal structure

Dependent variable: *Intermediate Goal Funded*	**Two Goals, Known**	**Two Goals, 2nd Unknown**
Final Goal	-0.010 (0.006)	-0.004 (0.004)
Intermediate Goal	-0.001 (0.002)	-0.001 (0.001)
Value Uncertainty	-0.000 (0.020)	-0.008 (0.008)
No Goal Possible		0.000 (.)
N	450	248
R^2	0.006	-0.003
F-statistic	2.04	0.35

Notes: cluster-robust standard errors in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively. Standard errors are clustered at the group level.

Table 2.10 Variance in individual contributions, by treatment

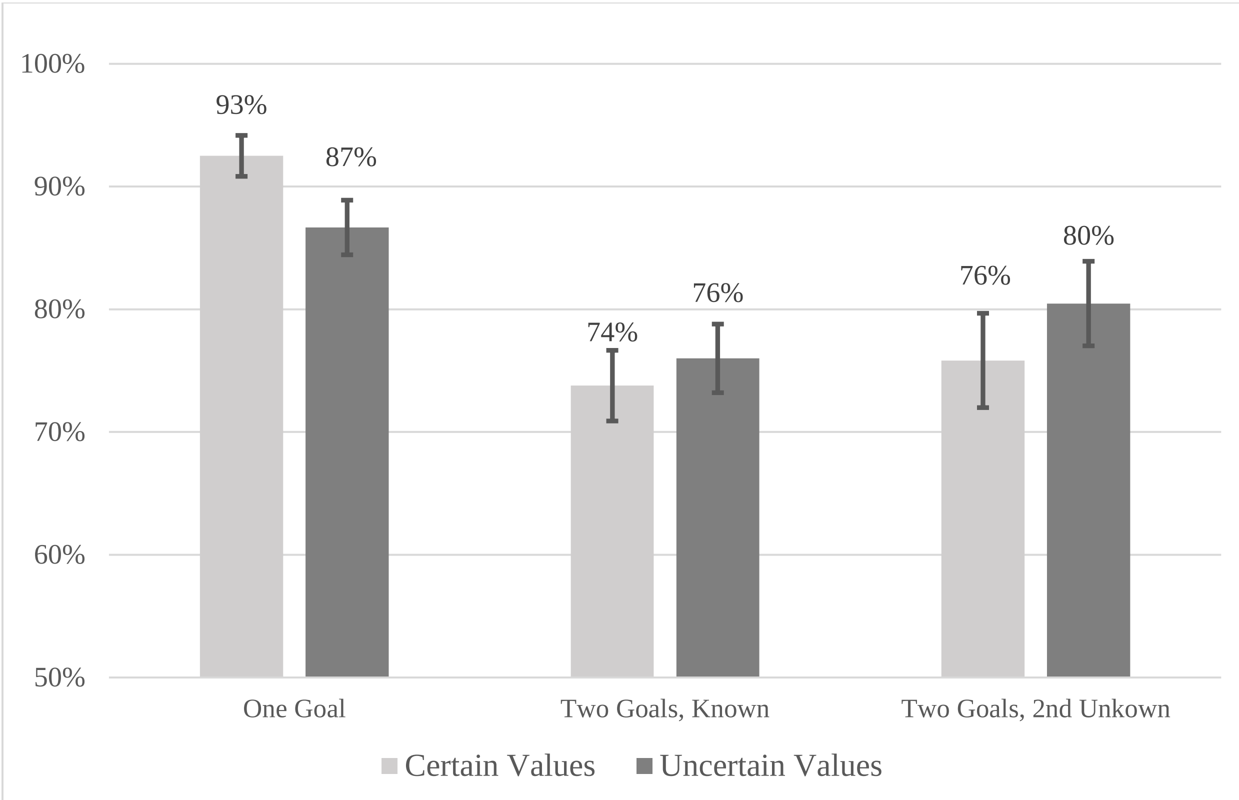
Dependent Variable: *Contribution Variance*	(1) Model 1	(2) Model 2
Two Goals, Known (T2)	0.878	0.814
	(0.572)	(0.610)
Two Goals, 2nd Unknown (T3)	0.230	0.177
	(0.514)	(0.562)
One Goal, Value Uncertainty (T4)	-0.086	-0.065
	(0.591)	(0.568)
Two Goals, Known, Value Uncertainty (T5)	0.009	0.106
	(0.495)	(0.536)
Two Goals, 2nd Unknown, Value Uncertainty (T6)	0.695	0.538
	(0.592)	(0.662)
Age		0.089
		(0.153)
Gender		0.027
		(0.574)
GPA		-0.354
		(0.392)
Risk Averse		-0.579
		(0.533)
Controls for order effects?	No	Yes
N	1214	1214
R^2	0.027	0.036
F Statistic	1.32	1.28

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively.

Table 2.11 Variance in individual contributions, by goal structure

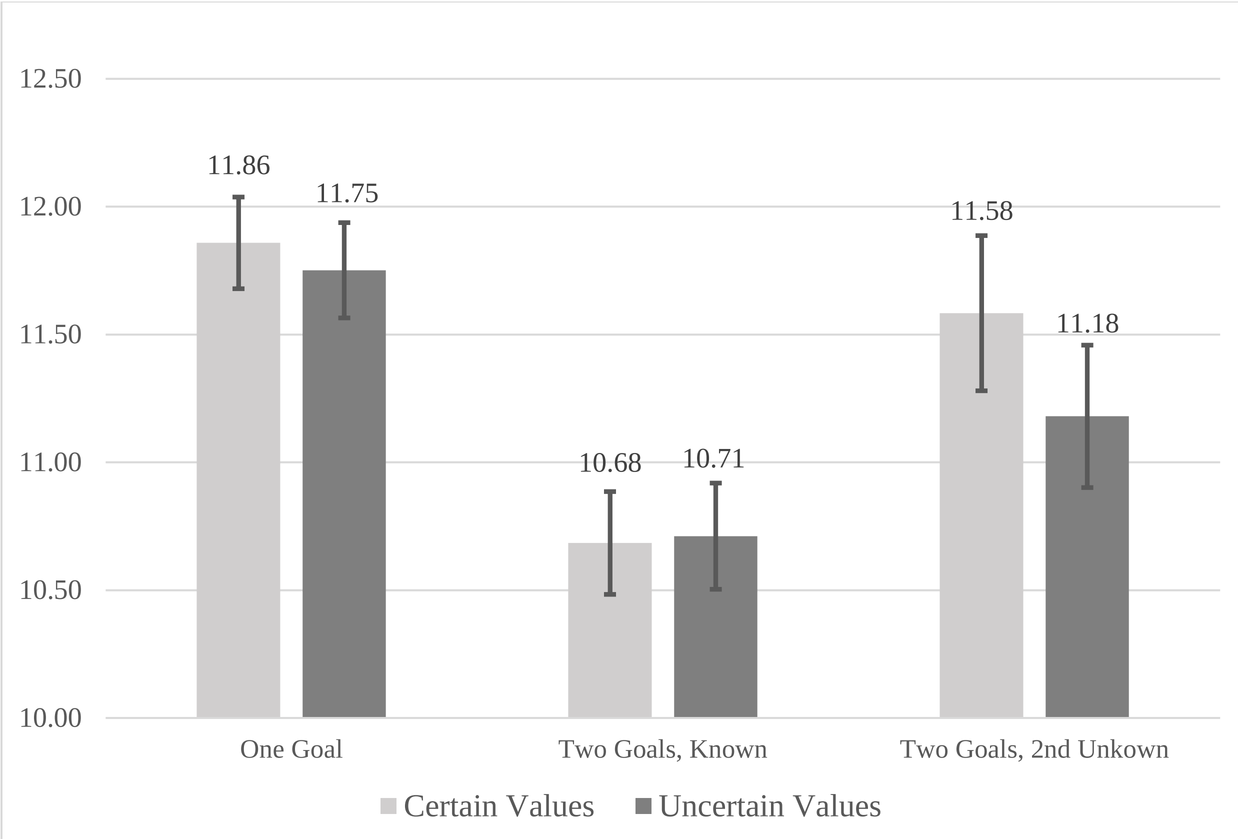
Dependent variable: *Contribution Variance*	**One Goal**	**Two Goals, Known**	**Two Goals, 2nd Unknown**
Final Goal	0.131*** (0.033)	0.260*** (0.023)	0.202*** (0.043)
Value Uncertainty	-0.086 (0.597)	-0.867** (0.417)	0.502 (0.461)
Intermediate Goal		0.007 (0.042)	-0.100 (0.089)
No Goal Possible			0.343 (0.325)
N	1860	1800	1196
R^2	0.011	0.055	0.042
F-statistic	8.29***	48.06***	12.87***

Notes: standard errors clustered by group in parentheses. *, **, and *** indicate estimate is statistically significant at the 10%, 5%, and 1% significance levels, respectively. Periods where NoGoal2=1 are dropped from this regression. Standard errors are clustered at the group level.



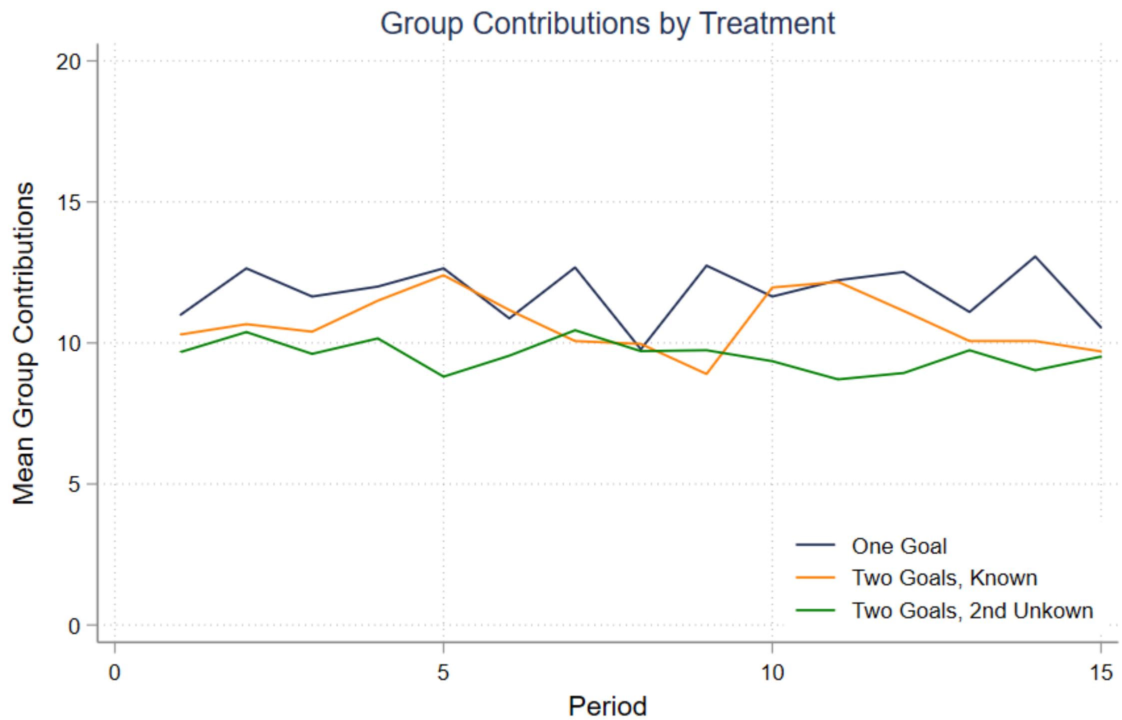
Note: Observations where the “no goal” option was drawn are not directly comparable and are excluded from this figure.

Figure 2.1. Final goal success rate, by treatment



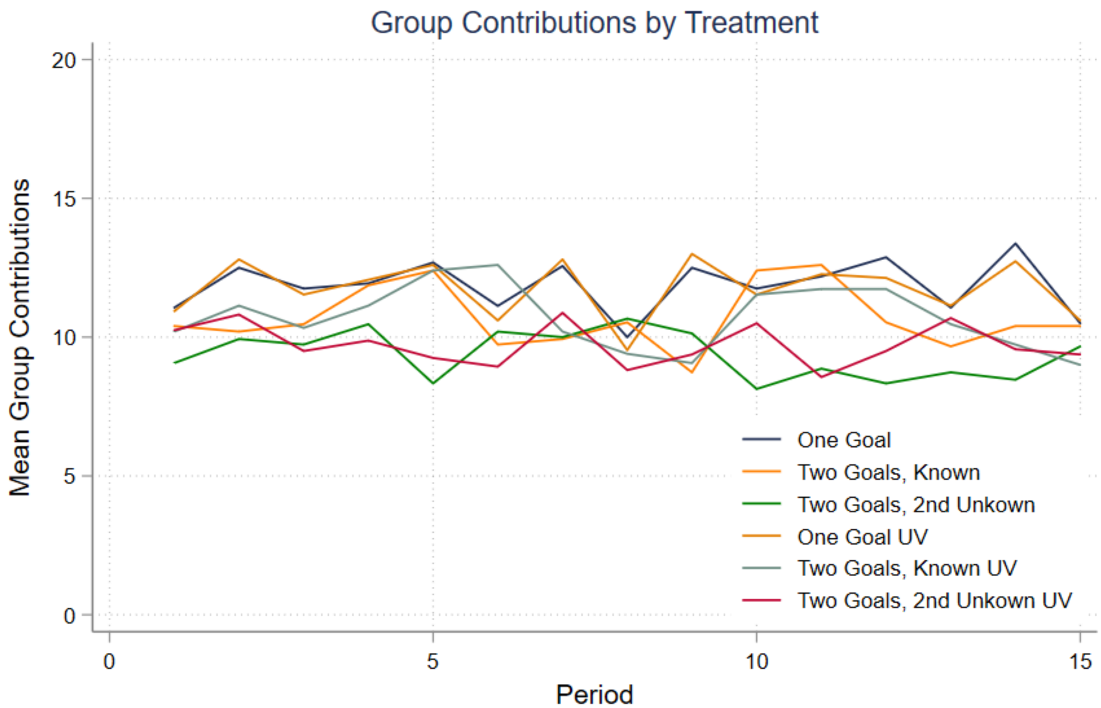
Note: Observations where the “no goal” option was drawn are not directly comparable and are excluded from this figure.

Figure 2.2 Total group contributions, by treatment



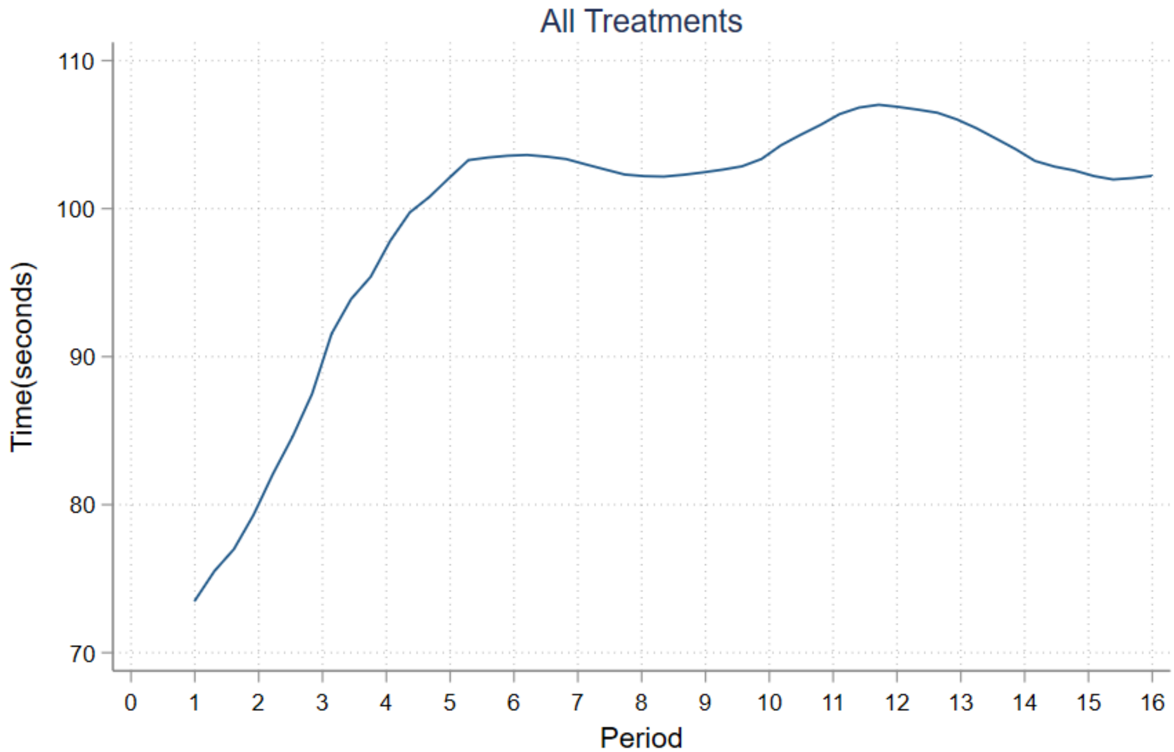
Note: Observations where the "No Goal Possible" option was drawn are not directly comparable and are excluded from this figure.

Figure 2.3 Mean Contributions across decision periods, by treatment



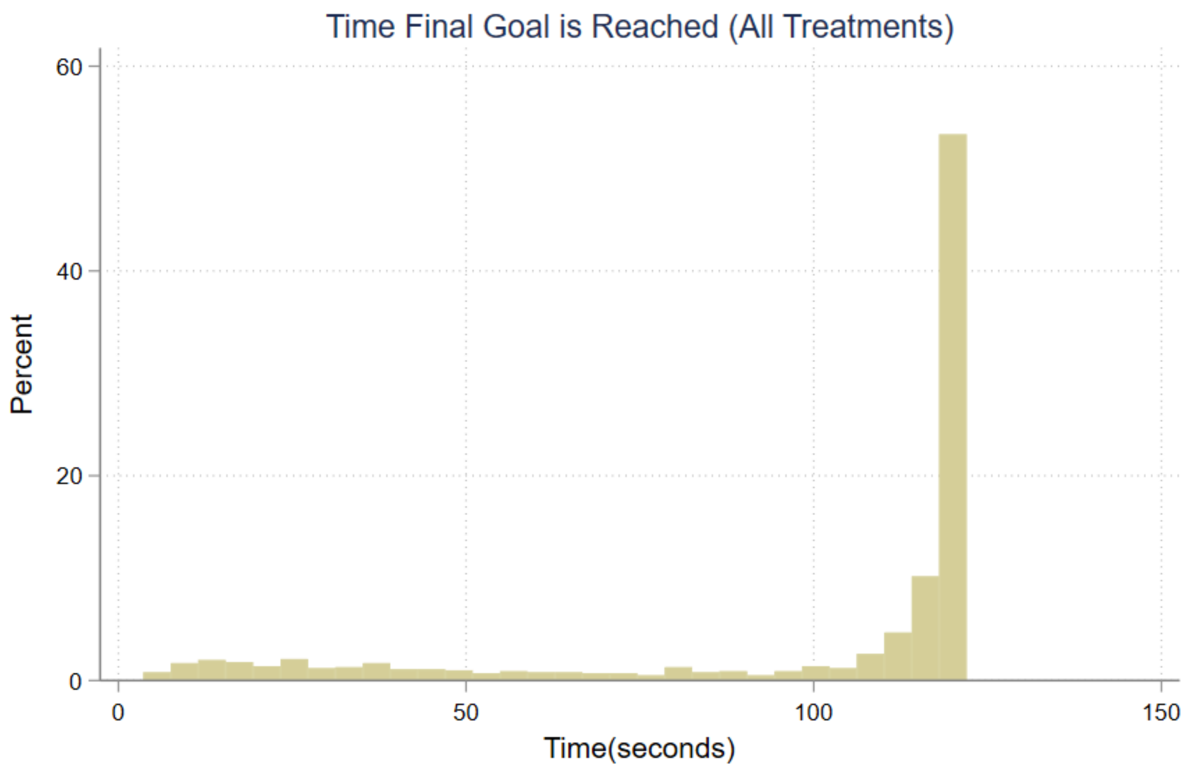
Note: Observations where the "No Goal Possible" option was drawn are not directly comparable and are excluded from this figure.

Figure 2.4 Mean Contributions across decision periods, by treatment



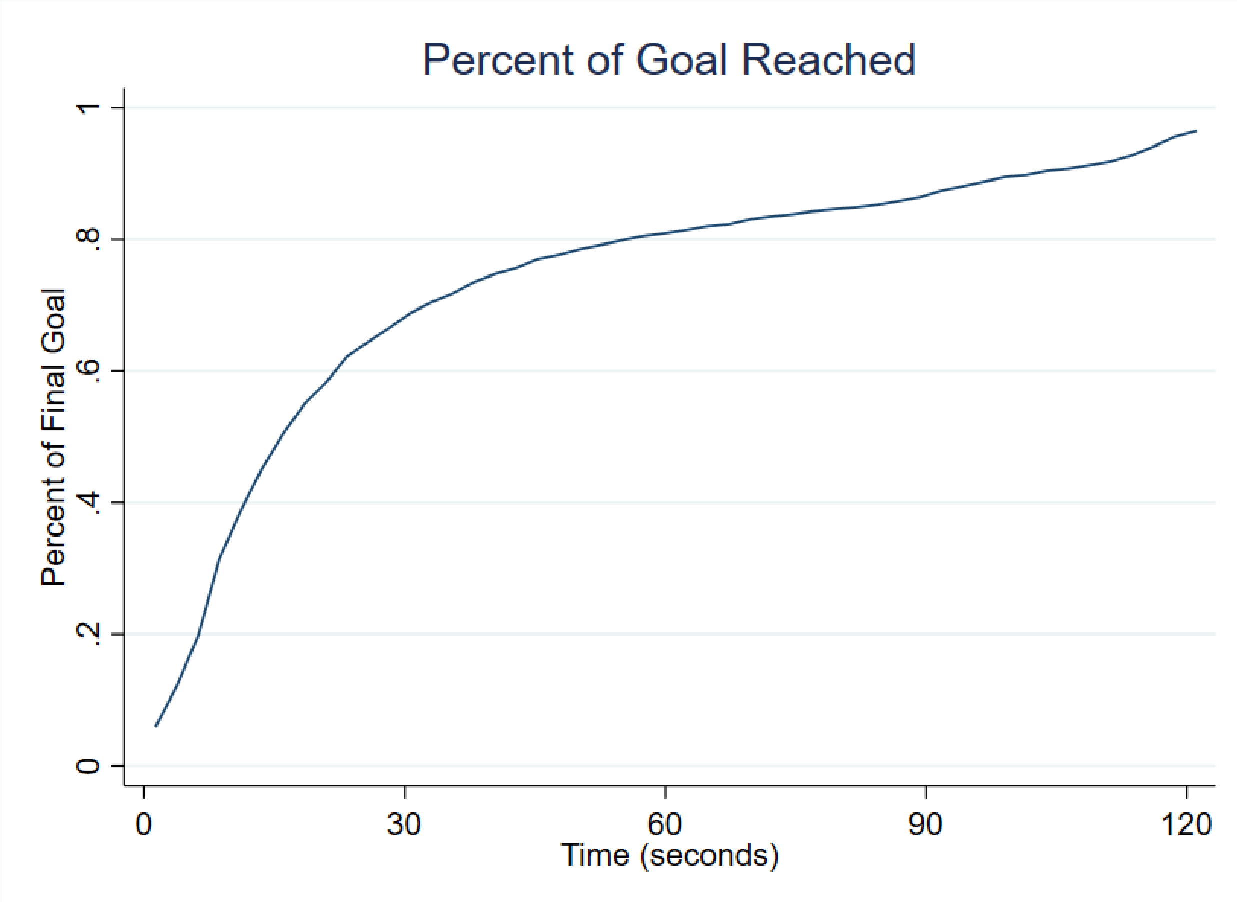
Note: Observations where the “No Goal Possible” option was drawn are not directly comparable and are excluded from this figure.

Figure 2.5 Time final goal reached by decision period



Note: Observations where the "No Goal Possible" option was drawn are not directly comparable and are excluded from this figure.

Figure 2.6 Distribution of Time Final Goal is Reached



Note: Observations where the "No Goal Possible" option was drawn are not directly comparable and are excluded from this figure.

Figure 2.7 Percent of Final Goal Reached within the 2-minute contribution window

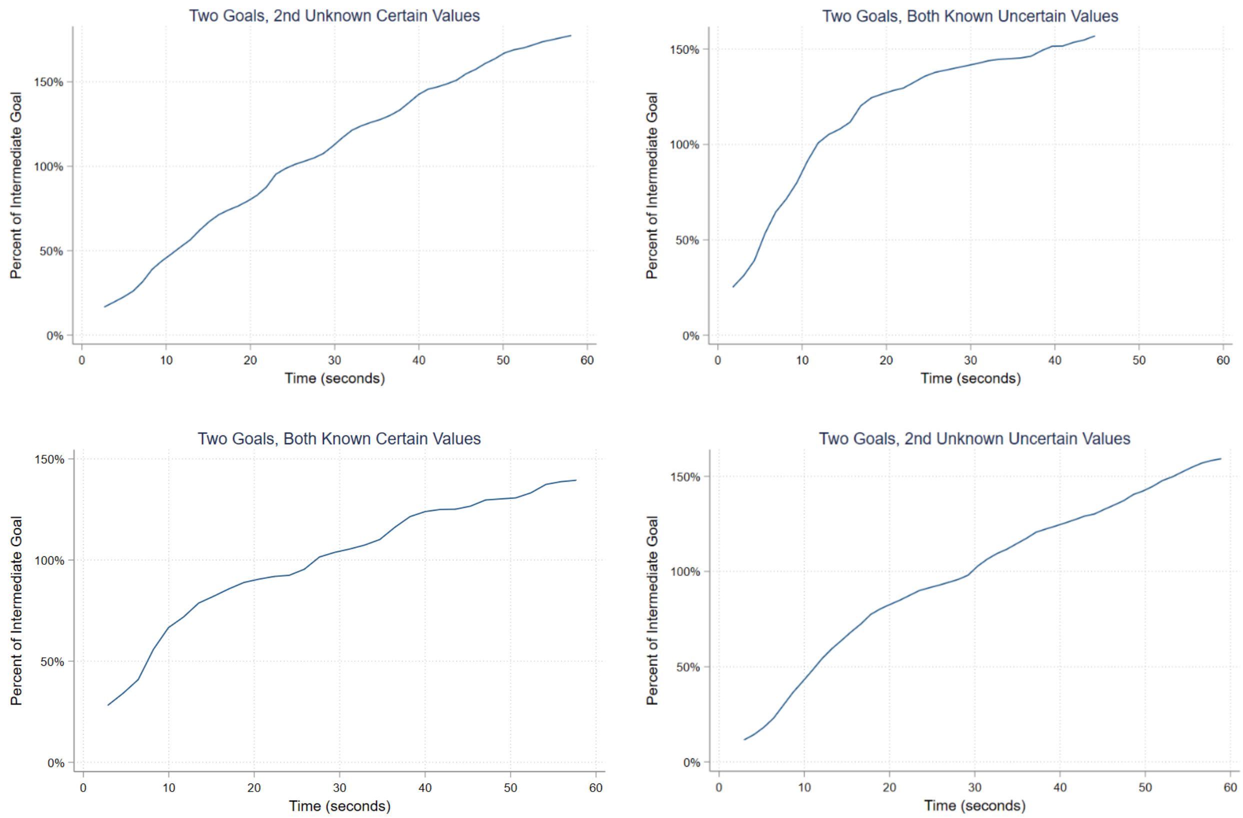


Figure 2.8 Time Intermediate Goal Reached

Chapter 2 Theory

Two Goal Case

The two-goal setting can be characterized as a two-stage game played sequentially. Here, I can assume that a player makes decisions in the first stage in isolation of the second stage. This results in two separate probability functions for reaching goals one and two:

$$P_I(x_{1I}, x_{2I}) = \begin{cases} 0 \;\; if \; x_{1I} + x_{2I} < t_I \\ 1 \;\; if \; x_{1I} + x_{2I} \geq t_I \end{cases}$$

$$P_F(x_{1F}, x_{2F}) = \begin{cases} 0 \;\; if \; x_{2F} + x_{2F} < t_F \\ 1 \;\; if \; x_{2F} + x_{2F} \geq t_F \end{cases}$$

Here, t_F is the second goal, with an individual payout of v_F. Thus, I can formulate response functions for both the first stage and second stage:

First stage

1. $\{(x_{1I}, x_{2I}): x_{1I} + x_{2I} = t_I;\; x_{1I} \leq v_I;\; x_{2I} \leq v_I\}$;
2. (0,0);
3. $\{(x_{1I}, x_{2I}):\; x_{1I} < t_I - v_I;\; x_{2I} < t_I - v_I\}$.

Second stage

1. $\{(x_{1F}, x_{2F}): x_{1F} + x_{2F} = t_F;\; x_{1F} \leq v_F;\; x_{2F} \leq v_F\}$;
2. (0,0);
3. $\{(x_{1F}, x_{2F}):\; x_{1F} < t_F - v_F;\; x_{2F} < t_F - v_F\}$.

For any case where the value of a good in a single-goal game equals $v = v_F$, the two-goal problem reduces to a one-goal game where $t = t_F$.

Two-goal case with value uncertainty

Similar to the single goal case, there exist two possible values of the good at both thresholds: v_{1I} and v_{2I} for the first goal t_I, and v_{1F} and v_{2F} for the second goal t_F. Hence $\frac{1}{2}(v_{1I}+v_{2I})=v_I$ and $\frac{1}{2}(v_{2F}+v_{2F})=v_F$ in comparison with the two-goal case with known values. Therefore, player one solves the first stage, making a contribution decision x_{1I} before moving to the second stage, making decision x_{1F}. The following are player one's best response functions.

First stage

1. $x_{1I}^*(v)=\begin{cases}0, & if\ E[v_I]<\frac{1}{2}t_I,\\ \frac{1}{2}t_I, & if\ \frac{1}{2}t_I\le E[v_I].\end{cases}$

2. $x_{1F}^*(v)=\begin{cases}0, & if\ E[v_I]<t_I-x_{2I},\\ t-x_{2I}, & if\ t_I-x_{2I}\le E[v_I]\le\omega.\end{cases}$

Second stage

1. $x_{1F}^*(v)=\begin{cases}0, & if\ E[v_F]<\frac{1}{2}t_F,\\ \frac{1}{2}t_F, & if\ \frac{1}{2}t_F\le E[v_F].\end{cases}$

2. $x_{1F}^*(v)=\begin{cases}0, & if\ E[v_F]<t_F-x_{2F},\\ t_F-x_{2F}, & if\ t_F-x_{2F}\le E[v_F]\le\omega.\end{cases}$

Similar to the case with known values, if $t_F=t$ and $\frac{1}{2}(v_{1F}+v_{2F})=v_F$, then the two-stage game is equivalent to the one-stage game with uncertain values.

Chapter 2. Experiment Instructions

(Note: Instructions are unaltered, with the exception of changing the task labels to reflect those used in the manuscript)

You are about to participate in an experiment in economic decision making. Please follow the instructions carefully. At any time, please feel free to raise your hand if you have a question. At the end of today's session, you will be paid your earnings privately and in cash.

You have been randomly assigned an ID number for this experiment. You will never be asked to reveal your identity to anyone. Your name will never be associated with any of your decisions. In order to keep your decisions private, please do not reveal your choices or otherwise communicate with any other participant. Importantly, please refrain from verbally reacting to events that occur during the experiment.

Today's session consists of three parts: Experiment 1, Experiment 2 and a short questionnaire. In Experiment 1, you will make a series of lottery decisions. In Experiment 2, you will be randomly sorted into groups and have the opportunity to contribute money to fund a project. If a project goal is reached, the project is funded and each player receives a payout.

Chapter 2 Experiment Instructions

<u>Experiment 1</u>

Please click “Continue” and refer to your computer screen while we read the instructions.

We would like you to make a decision for each of 10 scenarios. Each scenario involves a choice between playing a lottery that pays $4 or $0 according to specified chances (Option A) or receiving $2 for sure (Option B).

You will notice that the only differences across scenarios are the chances of receiving the high or low prize for the lottery. At the end of the today’s session, ONE of the 10 scenarios will be selected at random and you will be paid according to your decision for this selected scenario ONLY. Each scenario has an equal chance of being selected.

Please consider your choice for each scenario carefully. Since you do not know which scenario will be played out, it is in your best interest to treat each scenario as if it will be the one used to determine your earnings.

Before making decisions, are there any questions?

Once you are ready to submit your decisions, please click the “Submit” button.

Experiment 2

Overview

In this experiment, all money amounts are denominated in tokens, and will be exchanged at a rate of 10 tokens to 1 US dollar at the end of the experiment.

There will be many decision rounds. You will not know the number of rounds until the experiment has ended. Each decision round is separate from the other rounds, in the sense that the decisions you make in one round will not affect the outcome or earnings of any other round.

In this experiment, participants will be randomly placed into four-person groups. You will remain in the same group for the entire experiment.

The decision setting

In each round, you are given an endowment of 8 tokens. You have the opportunity to contribute some or all your tokens towards funding a project. Any tokens not contributed are yours to keep.

If enough tokens are contributed from the group, everyone in the group receives a "payout". The payout is the same for every group member, and does not depend on how many tokens a particular person contributed.

If you contribute tokens towards a goal, but that goal is not reached, these tokens will be refunded to you. These tokens will be yours to keep.

Project goals

The project will have up to two funding goals: an intermediate goal (Goal 1), and a final goal (Goal 2). Reaching either goal results in a payout to all members of the group. The payout to the group is higher when the final goal, Goal 2, is reached.

At the start of the round, Goal 2 is uncertain, and will only be revealed if Goal 1 is reached. If Goal 1 is reached, the computer will randomly select Goal 2 from two possible options. Each option will have an equal chance of being selected.

Know that, in some decision rounds, one of the two possible options for Goal 2 is "No Goal 2". If this option is randomly selected, Goal 2 does not exist. No more contributions are possible.

Project payouts

The payout for reaching a funding goal is uncertain. At the end of the decision round, if a goal is reached, the computer will randomly select the payout from two possible amounts. Each amount will have an equal chance of being selected. Along with these instructions we have provided you with an example of what the **decision screen** on your computer will look like. Please refer to this as we read through the instructions.

In this example,

- Goal 1 is 4 tokens. If 4 tokens are contributed from the group, this goal is reached, and each group member receives a payout of either 3 or 7 (each with a 50% chance).
- Goal 2 is either 10 or 14 tokens (each with a 50% chance) and is revealed only if Goal 1 is reached.
- The payout associated with Goal 2 is either 8 or 12 (each with a 50% chance). If this funding goal is reached, each group member receives a payout of either of 8 or 12 tokens (each with a 50% chance).

How to contribute tokens

To contribute tokens, you enter the number of tokens you would like to contribute and click the SUBMIT button. Once you do so, you will see progress made towards the funding goal on the right side of the screen.

After your first contribution, you have the opportunity to contribute additional tokens. To do so, you follow the same procedure: enter the amount you want to contribute and click the SUBMIT button. You do not have the opportunity to alter your original contribution or otherwise take back tokens you previously contributed.

When necessary, the computer will limit the amount you can contribute to make sure you do not contribute more than what is needed to reach the next goal, and to make sure you do not contribute more than your endowment.

Timer

There is a timer on the upper right corner of the screen. You will have 2 minutes to make your decisions. During those 2 minutes, you can contribute tokens to the project fund. After 2 minutes, the round will end regardless of whether any goals have been reached.

Calculating your earnings

In each round, there are three possible outcomes: (1) no goal is reached, (2) only Goal 1 is reached, or (3) Goal 2 is reached. We will discuss your earnings in each case.

No goal reached. If there are not enough contributions to reach Goal 1, there are no payouts to the group. Any contributions you made towards Goal 1 will be refunded to you. Your earnings are then equal to the 8 tokens you started with.

Your earnings = Endowment (8 tokens)

ONLY Goal 1 reached. Each group member receives the Goal 1 payout. All contributions you made towards Goal 1 will be subtracted to calculate your earnings. If you contributed any tokens after Goal 1 was reached, these are refunded to you.

Your earnings = Endowment + Goal 1 payout – tokens YOU contributed

Goal 2 reached. Every group member receives the Goal 2 payout. All contributions you made will be subtracted to calculate your earnings.

Your earnings = Endowment + Goal 2 payout – tokens YOU contributed

At the end of each decision round you will be shown a **results screen** that summarizes the outcomes from the round, along with a calculation of your earnings.

Proceeding through the experiment

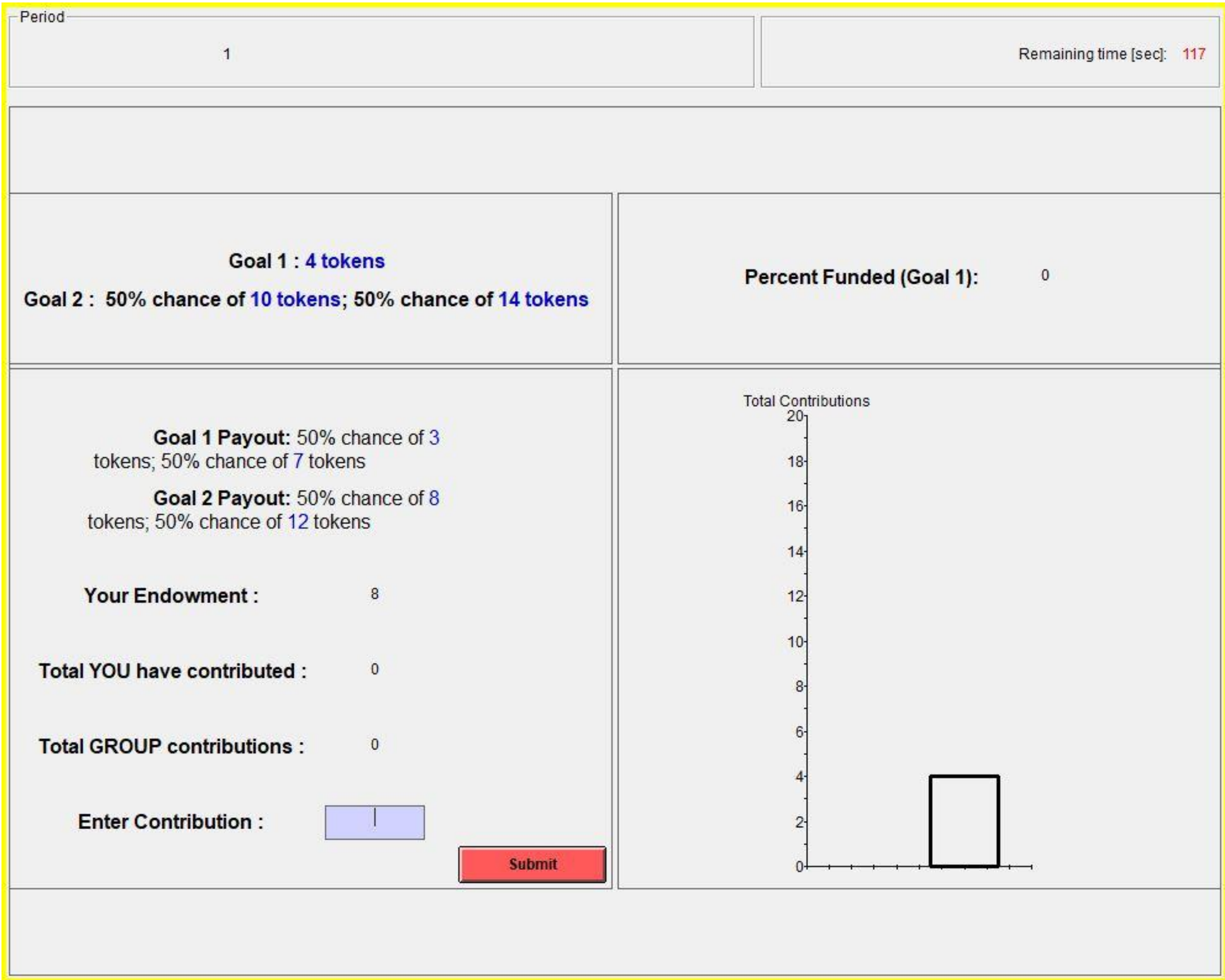
At the start of each decision round, you will be informed of the project goals and payouts in effect. Know that the project goals and payouts may differ from one round to the next, so pay close attention to this information.

We realize that we have just provided you with plenty of information to think about. Before we proceed to the paid decision rounds, we will go through a training round to better familiarize you with the procedures.

Aside from decisions in this training round, you will be paid based on the outcome of each decision round. This means that it is very important to consider each decision prior to making it.

Before we proceed to the training round, are there any questions?

Example decision screen.



Appendix C.

Chapter 3 Tables and Figures

Table 3.1 Reported domestic violence incidents and county-level statistics

	Observations	Mean	Std. Dev
Simple Assault Reports – county by year	589,847	256.235	892.49
Simple Assault Reports – county by day	589,847	1.399	4.380
Percent white - victim	589,847	0.811	0.326
Percent black - victim	589,847	0.176	0.3157
Percent race unknown - victim	589,847	0.013	0.095
All Assault Reports – county by year	589,847	375.581	1294.66
All Assault Reports – county by day	589,847	2.049	6.279
Percent white - victim	589,847	0.556	0.404
Percent black - victim	589,847	0.113	0.244
Percent race unknown - victim	589,847	0.331	0.377
County-level Socioeconomic Controls			
Percent in Labor Force - county	589,847	0.439	0.044
Percent Employed - county	589,847	0.406	0.044
Percent Unemployed - county	589,847	0.033	0.012
Weather Controls			
Temperature Minimum - Index	589,847	45.085	67.972
Temperature Maximum - Index	589,847	112.679	97.603
Temperature Average - Index	589,847	7.859	27.243
Precipitation Average - Index	589,847	36.660	92.797

Table 3.2 Reported Officer Involved Fatalities - TN

	Observations	Mean	Std. Dev
Incidents by County by Year	536	3.325	2.938
Race of Victim			
European-American/white	536	0.407	0.492
African American/black	536	0.239	0.427
Hispanic/Latino	536	0.021	0.142
Middle Eastern	536	0.002	0.043
Asian/Pacific islander	536	0.004	0.061
Race unspecified	536	0.328	0.470
Gender - Male	536	0.903	0.296
Victim Age	536	27.77	13.40
Cause of Death			
Gunshot	536	0.761	0.427
Vehicle	536	0.174	0.379
Beaten/Bludgeoned	536	0.011	0.105
Burned/Smoke Inhalation	536	0.002	0.043
Chemical Agent/Pepper Spray	536	0.006	0.075

Source: Fatalencounters.org, Tennessee Data, 2001-2019.

Table 3.3 Descriptive statistics of 911 Calls for Service and Zip code Controls

	Observations	Mean	Std. Dev
911 Calls for Service	6,240,117	110.327	52.659
911 Calls for Domestic Violence	6,240,117	8.289	5.592
Zip code Controls			
Percent Unemployed	6,220,275	6.462	2.784
Percent Female	6,220,275	53.702	3.684
Mean Household Income	6,220,188	54913.02	24095.6
Percent Below Poverty Line	6,220,188	29.045	11.367
Weather Controls			
Temperature Minimum - Index	6,160,882	43.652	35.586
Temperature Maximum - Index	6,160,882	89.717	37.182
Temperature Average - Index	6,160,882	18.537	18.760
Precipitation Average - Index	6,160,882	39.358	42.060

Table 3.4 Impact of Officer Involved Fatalities on Domestic Violence Reports per 1,000 Residents

Dependent Variable:	Simple Assaults			All Assaults		
	10 Days	7 Days	3 Days	10 Days	7 Days	3 Days
Post OIF	0.0003	0.0004	0.0004	0.0001	0.0002	0.0003
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Treated County	-0.0003	-0.0011	-0.0020	-0.0012	-0.0013	-0.0025
	(0.0012)	(0.0011)	(0.0013)	(0.0014)	(0.0013)	(0.0020)
Post OIF x Treated County	0.0012	0.0023	0.0033*	0.0025	0.0032**	0.0039**
	(0.0016)	(0.0015)	(0.0017)	(0.0018)	(0.0016)	(0.0019)
Post OIF x Treated County x Race of Police Victim: White	-0.0003	-0.0002	-0.0003	0.0000	-0.0005	-0.0014
	(0.0008)	(0.0008)	(0.0012)	(0.0010)	(0.0011)	(0.0017)
Post OIF x Treated County x Cause of Death: Gunshot	-0.0006	-0.0020	-0.0030	-0.0020	-0.0029	-0.0030
	(0.0017)	(0.0016)	(0.0020)	(0.0019)	(0.0017)	(0.0022)
Percent Unemployed	0.0001	0.0001	0.0001	0.0002	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Constant	0.0133***	0.0132***	0.0134***	0.0200***	0.0201***	0.0203***
	(0.0009)	(0.0009)	(0.0009)	(0.0013)	(0.0013)	(0.0013)
Observations	470,326	493,689	536,362	470,326	493,689	536,362
R-squared	0.0797	0.0801	0.0810	0.1095	0.1100	0.1110
Weather Controls	YES	YES	YES	YES	YES	YES
Fixed Effects						
Month	YES	YES	YES	YES	YES	YES
County x Year	YES	YES	YES	YES	YES	YES
Day of Week	YES	YES	YES	YES	YES	YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Estimates for both models use robust standard errors.

Table 3.4A Impact of Officer Involved Fatalities on Domestic Violence Reports Filed: Cause of Death - Gunshot

Dependent Variable:	Simple Assaults			All Assaults		
	10 Days	7 Days	3 Days	10 Days	7 Days	3 Days
Post OIF	0.0001	0.0002	0.0005**	0.0003	0.0004*	0.0008**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Treated County	-0.0004	0.0002	-0.0004	-0.0007	0.0002	-0.0006
	(0.0008)	(0.0007)	(0.0011)	(0.0012)	(0.0011)	(0.0012)
Post OIF x Treated County	0.0013*	0.0011	0.0011	0.0015	0.0013	0.0016
	(0.0007)	(0.0007)	(0.0010)	(0.0010)	(0.0010)	(0.0012)
Post OIF x Treated County x Race of Police Victim: White	-0.0015*	-0.0014	-0.0015	-0.0016	-0.0018	-0.0025
	(0.0009)	(0.0010)	(0.0014)	(0.0012)	(0.0014)	(0.0019)
Percent Unemployed	0.0001	0.0002	0.0003	0.0002	0.0002	0.0004*
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Constant	0.0129***	0.0127***	0.0117***	0.0195***	0.0192***	0.0177***
	(0.0009)	(0.0009)	(0.0011)	(0.0014)	(0.0014)	(0.0015)
Observations	209,434	170,531	96,198	209,434	170,531	96,198
R-squared	0.0801	0.0795	0.0813	0.1087	0.1085	0.1106
Weather Controls	YES	YES	YES	YES	YES	YES
Fixed Effects						
Month	YES	YES	YES	YES	YES	YES
County x Year	YES	YES	YES	YES	YES	YES
Day of Week	YES	YES	YES	YES	YES	YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the county level.

Table 3.5 Impact of Officer Involved Fatalities on Domestic Violence Reports Filed – Less Treated Counties

Dependent Variable:	Simple Assaults			All Assaults		
	10 Days	7 Days	3 Days	10 Days	7 Days	3 Days
Post OIF	0.0003	0.0004	0.0004	0.0001	0.0002	0.0003
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Treated County	-0.0016	-0.0032**	-0.0053**	-0.0032	-0.0036	-0.0069**
	(0.0020)	(0.0015)	(0.0020)	(0.0025)	(0.0024)	(0.0030)
Post OIF x Treated County	0.0015	0.0039*	0.0053	0.0034	0.0049	0.0070*
	(0.0027)	(0.0023)	(0.0033)	(0.0030)	(0.0030)	(0.0035)
Post OIF x Treated County x Race of Police Victim: White	0.0003	-0.0004	-0.0005	0.0010	-0.0006	-0.0032
	(0.0018)	(0.0018)	(0.0028)	(0.0026)	(0.0028)	(0.0037)
Post OIF x Treated County x Cause of Death: Gunshot	-0.0013	-0.0033	-0.0046	-0.0040*	-0.0045*	-0.0044
	(0.0023)	(0.0022)	(0.0034)	(0.0023)	(0.0025)	(0.0038)
Percent Unemployed	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Constant	0.0128***	0.0127***	0.0129***	0.0195***	0.0196***	0.0197***
	(0.0009)	(0.0009)	(0.0009)	(0.0014)	(0.0013)	(0.0013)
Observations	450,701	473,079	513,927	450,701	473,079	513,927
R-squared	0.0669	0.0673	0.0681	0.0931	0.0937	0.0945
Weather Controls	YES	YES	YES	YES	YES	YES
Fixed Effects						
Month	YES	YES	YES	YES	YES	YES
County x Year	YES	YES	YES	YES	YES	YES
Day of Week	YES	YES	YES	YES	YES	YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the county level. Observations for Knox, Davidson, Hamilton, and Shelby counties are dropped from this regression.

Table 3.6 7-day Treatment Window Diff-in-Diff – Domestic Violence Calls

Dependent Variable: *Domestic Violence 911 Calls*	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Post OIF	-0.0092	-0.0076	-0.0001	-0.0007	-0.0007
	(0.0128)	(0.0127)	(0.0132)	(0.0130)	(0.0130)
Treated Zip code	-0.0596	-0.0656	-0.0719*	-0.0300	-0.0392
	(0.0503)	(0.0460)	(0.0415)	(0.0356)	(0.0441)
Post OIF x Treated Zip code	0.0542	0.0565	0.0562	0.0598	0.0615
	(0.0410)	(0.0395)	(0.0402)	(0.0414)	(0.0428)
Post OIF x Treated Zip code x Race of Police Victim: White	-0.0168	-0.0166	-0.0159	-0.0087	-0.0078
	(0.0390)	(0.0404)	(0.0393)	(0.0384)	(0.0378)
Post OIF x Treated Zip code x Gender of Police Victim: Male	-0.0460	-0.0482	-0.0485	-0.0179	-0.0203
	(0.0568)	(0.0603)	(0.0589)	(0.0558)	(0.0560)
Post OIF x Treated Zip code x Cause of Death: Gunshot	-0.0310	-0.0293	-0.0283	-0.0652	-0.0629
	(0.0575)	(0.0628)	(0.0583)	(0.0518)	(0.0515)
Zip code-level Controls					
Percent Unemployed	0.0248***	0.0248***	0.0248***	-	-
	(0.0049)	(0.0049)	(0.0049)		
Percent Female	0.0041**	0.0042**	0.0042**	-	-
	(0.0020)	(0.0020)	(0.0020)		
Mean Household Income	-0.0000	-0.0000	-0.0000	-	-
	(0.0000)	(0.0000)	(0.0000)		
Percent Below Poverty	0.0075***	0.0074***	0.0074***	-	-
	(0.0016)	(0.0016)	(0.0016)		
Constant	-0.1890	-0.1670	-0.2230*	0.3387***	0.3387***
	(0.1306)	(0.1304)	(0.1256)	(0.0175)	(0.0175)
Observations	1,012,631	1,012,631	1,012,631	1,012,631	1,012,631
R-squared	0.5425	0.5490	0.5523	0.5895	0.5950
Day of Week FE	YES	YES	YES	YES	YES
Month FE		YES	YES	YES	YES
Year FE			YES	YES	
Zip code				YES	
Zip code by Year FE					YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.6A 3-day Treatment Window Diff-in-Diff – Domestic Violence Calls

Dependent Variable: *Domestic Violence 911 Calls*	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Post OIF	0.0139	0.0153	0.0060	0.0056	0.0054
	(0.0145)	(0.0124)	(0.0114)	(0.0115)	(0.0114)
Treated Zip code	-0.2650***	-0.2648***	-0.2645***	-0.1950*	-0.2325*
	(0.0903)	(0.0821)	(0.0884)	(0.1004)	(0.1146)
Post OIF x Treated Zip code	0.2629**	0.2630**	0.2562**	0.2252*	0.2548*
	(0.1115)	(0.1057)	(0.1116)	(0.1224)	(0.1318)
Post OIF x Treated Zip code x Race of Police Victim: White	-0.0151	-0.0136	-0.0134	-0.0066	-0.0062
	(0.0304)	(0.0319)	(0.0325)	(0.0338)	(0.0336)
Post OIF x Treated Zip code x Gender of Police Victim: Male	-0.2229*	-0.2240**	-0.2173*	-0.1823	-0.2100
	(0.1161)	(0.1098)	(0.1153)	(0.1225)	(0.1325)
Post OIF x Treated Zip code x Cause of Death: Gunshot	-0.0276	-0.0255	-0.0253	-0.0303	-0.0322
	(0.0547)	(0.0559)	(0.0546)	(0.0523)	(0.0527)
Zip code-level Controls					
Percent Unemployed	0.0265***	0.0265***	0.0265***	-	-
	(0.0048)	(0.0048)	(0.0048)		
Percent Female	0.0034*	0.0034*	0.0034*	-	-
	(0.0019)	(0.0019)	(0.0019)		
Mean Household Income	-0.0000	-0.0000	-0.0000	-	-
	(0.0000)	(0.0000)	(0.0000)		
Percent Below Poverty	0.0071***	0.0071***	0.0071***	-	-
	(0.0015)	(0.0015)	(0.0015)		
Constant	-0.1766	-0.1464	-0.1634	0.3600***	0.3605***
	(0.1293)	(0.1299)	(0.1273)	(0.0268)	(0.0266)
	508,568	508,568	508,568	508,564	508,564
Observations	0.5424	0.5479	0.5502	0.5853	0.5927
R-squared	YES	YES	YES	YES	YES
Day of Week FE		YES	YES	YES	YES
Month FE			YES	YES	
Zip code				YES	
Zip code by Year FE					YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.7 7-Day Treatment Window Diff-in-Diff – 911 Calls

Dependent Variable: *911 Calls*	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Post OIF	0.0376	0.0577	-0.0685	-0.0771	-0.0801
	(0.0757)	(0.0850)	(0.0871)	(0.0880)	(0.0888)
Treated Zip code	-1.4666***	-1.5237***	-1.5309***	-0.0872	-0.0940
	(0.3894)	(0.3646)	(0.3730)	(0.1118)	(0.1303)
Post OIF x Treated Zip code	-0.2429	-0.1892	-0.1846	-0.1181	-0.1052
	(0.1747)	(0.2167)	(0.2266)	(0.1687)	(0.1659)
Post OIF x Treated Zip code x Race of Police Victim: White	-0.4042	-0.4380*	-0.4201	-0.2385	-0.2403
	(0.2834)	(0.2344)	(0.2559)	(0.2152)	(0.2060)
Post OIF x Treated Zip code x Gender of Police Victim: Male	0.1392	0.0554	0.0561	0.0891	0.1653
	(0.2390)	(0.2727)	(0.2582)	(0.2068)	(0.2272)
Post OIF x Treated Zip code x Cause of Death: Gunshot	0.2712	0.3005	0.2820	0.1047	0.0089
	(0.2021)	(0.2081)	(0.1974)	(0.1906)	(0.2219)
Zip code-level Controls					
Percent Unemployed	0.0953	0.0951	0.0947	-	-
	(0.0966)	(0.0966)	(0.0966)		
Percent Female	-0.0930	-0.0929	-0.0929	-	-
	(0.0828)	(0.0827)	(0.0827)		
Mean Household Income	0.0000	0.0000	0.0000	-	-
	(0.0000)	(0.0000)	(0.0000)		
Percent Below Poverty	0.1229***	0.1227***	0.1228***	-	-
	(0.0322)	(0.0321)	(0.0321)		
Constant	3.6084	3.6714	4.5886	4.3618***	4.3679***
	(4.2941)	(4.2952)	(4.3136)	(0.1720)	(0.1720)
Observations	1,012,631	1,012,631	1,012,631	1,012,631	1,012,631
R-squared	0.5414	0.5523	0.5599	0.8232	0.8324
Day of Week FE		YES	YES	YES	YES
Month FE		YES	YES	YES	YES
Year FE			YES	YES	
Zip code FE				YES	
Zip code by Year FE					YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.7A 3-Day Treatment Window Diff-in-Diff – 911 Calls

Dependent Variable: *911 Calls*	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Post OIF	0.1865**	0.1114	-0.0024	0.0051	0.0018
	(0.0881)	(0.0880)	(0.0776)	(0.0785)	(0.0792)
Treated Zip code	-0.6163	-0.6162	-0.6234	0.5547***	0.5776***
	(0.7125)	(0.5454)	(0.5731)	(0.0962)	(0.1094)
Post OIF x Treated Zip code	-1.0207	-1.0556*	-1.0779*	-0.7183***	-0.7441***
	(0.7248)	(0.6036)	(0.6325)	(0.1990)	(0.1595)
Post OIF x Treated Zip code x Race of Police Victim: White	-0.6243**	-0.6174**	-0.6256**	-0.3877	-0.3732
	(0.2684)	(0.2788)	(0.3030)	(0.2684)	(0.2693)
Post OIF x Treated Zip code x Gender of Police Victim: Male	0.7623	0.7941	0.8738	0.4728**	0.5285**
	(0.7040)	(0.5872)	(0.6240)	(0.2174)	(0.1949)
Post OIF x Treated Zip code x Cause of Death: Gunshot	0.3991**	0.4016**	0.3428*	0.3464*	0.3159
	(0.1764)	(0.1781)	(0.2014)	(0.1929)	(0.1928)
Zip code-level Controls					
Percent Unemployed	0.0976	0.0977	0.0973	-	-
	(0.0984)	(0.0983)	(0.0982)		
Percent Female	-0.0919	-0.0920	-0.0919	-	-
	(0.0839)	(0.0838)	(0.0837)		
Mean Household Income	0.0000	0.0000	0.0000	-	-
	(0.0000)	(0.0000)	(0.0000)		
Percent Below Poverty	0.1233***	0.1231***	0.1232***	-	-
	(0.0321)	(0.0321)	(0.0321)		
Constant	3.1654	3.3180	4.6575	4.4739***	4.4745***
	(4.3429)	(4.3429)	(4.3727)	(0.1411)	(0.1417)
Observations	508,568	508,568	508,568	508,564	508,564
R-squared	0.5452	0.5552	0.5654	0.8305	0.8408
Day of Week FE		YES	YES	YES	YES
Month FE		YES	YES	YES	YES
Year FE			YES	YES	
Zip code FE				YES	
Zip code by Year FE					YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.8 4 Weeks after OIF Diff-in-Diff – Domestic Violence 911 Calls

Dependent Variable: *Domestic Violence 911 Calls*	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	-0.1769***	-0.1721***	-0.1584**	-0.1569**
	(0.0550)	(0.0603)	(0.0582)	(0.0584)
Treated Zip code	0.0737	0.0753	0.2746**	0.0018
	(0.1654)	(0.1702)	(0.1081)	(0.0850)
Post OIF x Treated Zip code	-0.6220**	-0.6252*	-0.6219*	-0.5751
	(0.3021)	(0.3096)	(0.3368)	(0.3504)
Post OIF x Treated Zip code x Race of Police Victim: White	0.0961	0.0909	0.2074	0.4161
	(0.3095)	(0.3019)	(0.2409)	(0.2947)
Post OIF x Treated Zip code x Gender of Police Victim: Male	0.1512	0.1459	0.3350	0.2939
	(0.2971)	(0.2977)	(0.3079)	(0.3206)
Post OIF x Treated Zip code x Cause of Death: Gunshot	0.5116	0.5167	0.2901	0.2576
	(0.3261)	(0.3207)	(0.3026)	(0.3146)
Zip code-level Controls				
Percent Unemployed	0.1028**	0.1030**	-	-
	(0.0478)	(0.0479)		
Percent Female	0.0285**	0.0283**	-	-
	(0.0121)	(0.0121)		
Mean Household Income	-0.0000	-0.0000	-	-
	(0.0000)	(0.0000)		
Percent Below Poverty	0.0705***	0.0704***	-	-
	(0.0109)	(0.0110)		
Constant	-1.8052**	-2.0215***	1.6010***	1.5947***
	(0.6922)	(0.6810)	(0.1310)	(0.1276)
Observations	1,944	1,944	1,944	1,944
R-squared	0.8663	0.8688	0.9173	0.9239
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Zip code			YES	
Zip code by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.8A 2 Weeks after OIF Diff-in-Diff – Domestic Violence 911 Calls

Dependent Variable: *Domestic Violence 911 Calls*	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	-0.0268	-0.0263	-0.0255	-0.0253
	(0.0185)	(0.0188)	(0.0179)	(0.0183)
Treated Zip code	0.1059**	0.1057**	0.0610*	-0.0196
	(0.0486)	(0.0483)	(0.0317)	(0.0318)
Post OIF x Treated Zip code	-0.2920***	-0.2917***	-0.2161*	-0.1997
	(0.0877)	(0.0845)	(0.1248)	(0.1372)
Post OIF x Treated Zip code x Race of Police Victim: White	0.1168	0.1171	0.1925	0.2183
	(0.2203)	(0.2214)	(0.1963)	(0.1943)
Post OIF x Treated Zip code x Gender of Police Victim: Male	0.2051**	0.2041**	0.1821*	0.1790
	(0.0781)	(0.0758)	(0.0958)	(0.1060)
Post OIF x Treated Zip code x Cause of Death: Gunshot	0.2126**	0.2115**	0.1185	0.1045
	(0.0860)	(0.0801)	(0.0715)	(0.0749)
Zip code-level Controls				
Percent Unemployed	0.0171*	0.0174*	-	-
	(0.0095)	(0.0096)		
Percent Female	0.0084**	0.0084**	-	-
	(0.0038)	(0.0038)		
Mean Household Income	0.0000*	0.0000*	-	-
	(0.0000)	(0.0000)		
Percent Below Poverty	0.0187***	0.0187***	-	-
	(0.0026)	(0.0026)		
Constant	-0.5725***	-0.6534***	0.3775***	0.3775***
	(0.2060)	(0.2015)	(0.0295)	(0.0290)
Observations	1,597	1,597	1,597	1,597
R-squared	0.8291	0.8310	0.8770	0.8868
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Zip code			YES	
Zip code by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.9 4 Weeks after OIF Diff-in-Diff – 911 Calls

Dependent Variable: *911 Calls*	(1) Model 4	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	-1.0228**	-1.1541***	-0.8742***	-0.8770***
	(0.4478)	(0.4103)	(0.3146)	(0.3174)
Treated Zip code	-3.4038	-3.4303	0.5306	0.7431
	(3.1427)	(3.2556)	(0.7252)	(0.8193)
Post OIF x Treated Zip code	-5.3144**	-5.2823**	-2.0724	-2.1430
	(2.3670)	(2.2818)	(2.5091)	(2.4310)
Post OIF x Treated Zip code x Race of Police Victim: White	-9.2797	-9.2883	-2.2726	-2.3725*
	(5.9242)	(6.4756)	(1.6288)	(1.2653)
Post OIF x Treated Zip code x Gender of Police Victim: Male	4.2057*	4.1680*	1.8031	2.2189
	(2.1443)	(2.1584)	(1.8098)	(1.7637)
Post OIF x Treated Zip code x Cause of Death: Gunshot	6.5662**	6.5463**	1.6478	1.6134
	(3.1676)	(3.1182)	(2.0349)	(1.8656)
Zip code-level Controls				
Percent Unemployed	-0.6719	-0.6664	-	-
	(0.8186)	(0.8193)		
Percent Female	-0.5253	-0.5261	-	-
	(0.6348)	(0.6350)		
Mean Household Income	-0.0000	-0.0000	-	-
	(0.0000)	(0.0000)		
Percent Below Poverty	1.0189***	1.0205***	-	-
	(0.1914)	(0.1918)		
Constant	30.0032	31.1348	22.4803***	22.5234***
	(33.5460)	(33.5512)	(0.7154)	(0.7206)
Observations	1,944	1,944	1,944	1,944
R-squared	0.7614	0.7635	0.9657	0.9697
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Zip code			YES	
Zip code by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.9A 2 Weeks after OIF Diff-in-Diff – 911 Calls

Dependent Variable: *911 Calls*	(1) Model 4	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	0.1578	0.1540	0.1589	0.1513
	(0.1446)	(0.1480)	(0.1199)	(0.1209)
Treated Zip code	-0.3736	-0.3761	0.3125**	0.3017**
	(0.6135)	(0.5949)	(0.1305)	(0.1331)
Post OIF x Treated Zip code	-1.5050***	-1.4940***	-1.0080**	-0.9652**
	(0.3893)	(0.3438)	(0.3814)	(0.3955)
Post OIF x Treated Zip code x Race of Police Victim: White	-2.8655***	-2.8785***	-0.4103	-0.5332
	(0.9050)	(0.9135)	(0.3947)	(0.4266)
Post OIF x Treated Zip code x Gender of Police Victim: Male	1.3171***	1.3235***	0.6877*	0.7511*
	(0.3710)	(0.3706)	(0.3696)	(0.3788)
Post OIF x Treated Zip code x Cause of Death: Gunshot	1.3463**	1.3207**	0.5553	0.4836
	(0.5045)	(0.5032)	(0.3754)	(0.3655)
Zip code-level Controls				
Percent Unemployed	-0.1199	-0.1188	-	-
	(0.1351)	(0.1350)		
Percent Female	-0.1026	-0.1027	-	-
	(0.1191)	(0.1194)		
Mean Household Income	0.0000	0.0000	-	-
	(0.0000)	(0.0000)		
Percent Below Poverty	0.1850***	0.1851***	-	-
	(0.0332)	(0.0333)		
Constant	4.8600	5.0934	3.7104***	3.7309***
	(6.1778)	(6.1063)	(0.2004)	(0.2026)
Observations	1,597	1,597	1,597	1,597
R-squared	0.7288	0.7318	0.9360	0.9407
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Zip code			YES	
Zip code by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the zip code level.

Table 3.10 4 Weeks after OIF Diff-in-Diff – Domestic Violence 911 Calls by Neighborhood

Dependent Variable: *Domestic Violence 911 Calls*	(1) Model 4	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	-0.1439**	-0.1269**	-0.1499**	-0.1516**
	(0.0505)	(0.0517)	(0.0536)	(0.0538)
Treated Neighborhood	0.5457***	0.5329***	0.3403***	0.1735***
	(0.1272)	(0.1335)	(0.0551)	(0.0500)
Post OIF x Treated Neighborhood	-0.4600***	-0.4801***	-0.3315***	-0.3353***
	(0.0887)	(0.0746)	(0.0954)	(0.0864)
Post OIF x Treated Neighborhood x Race of Police Victim: White	-0.2382	-0.2523	-0.1198	-0.1345
	(0.3341)	(0.3204)	(0.2944)	(0.2797)
Post OIF x Treated Neighborhood x Gender of Police Victim: Male	0.2995*	0.2888**	0.1930	0.2099
	(0.1365)	(0.1307)	(0.1345)	(0.1247)
Post OIF x Treated Neighborhood x Cause of Death: Gunshot	0.3082**	0.3309**	0.0928	0.1275
	(0.1192)	(0.1116)	(0.0711)	(0.0811)
Zip code-level Controls				
Percent Unemployed	0.0928	0.0930	0.0236	0.0236
	(0.0685)	(0.0689)	(0.1201)	(0.1206)
Percent Female	0.0301*	0.0300*	0.0770	0.0770
	(0.0144)	(0.0144)	(0.0472)	(0.0474)
Mean Household Income	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Percent Below Poverty	0.0678***	0.0678***	0.0557*	0.0557*
	(0.0186)	(0.0187)	(0.0254)	(0.0255)
Constant	-1.7030	-1.9510*	-3.5462	-3.5240
	(1.0386)	(1.0185)	(2.6750)	(2.6996)
Observations	1,536	1,536	1,536	1,536
R-squared	0.8526	0.8561	0.8867	0.8923
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Neighborhood			YES	
Neighborhood by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the neighborhood level.

Table 3.10A 2 Weeks after OIF Diff-in-Diff – Domestic Violence 911 Calls by Neighborhood

Dependent Variable: *Domestic Violence 911 Calls*	(1) Model 4	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	-0.0188	-0.0205	-0.0233*	-0.0237*
	(0.0120)	(0.0120)	(0.0125)	(0.0123)
Treated Neighborhood	0.1255***	0.1224***	0.0887***	0.0577***
	(0.0366)	(0.0360)	(0.0213)	(0.0186)
Post OIF x Treated Neighborhood	-0.1123***	-0.1133***	-0.0919***	-0.0910**
	(0.0247)	(0.0245)	(0.0246)	(0.0298)
Post OIF x Treated Neighborhood x Race of Police Victim: White	-0.0213	-0.0213	0.0355	0.0348
	(0.0898)	(0.0860)	(0.0696)	(0.0648)
Post OIF x Treated Neighborhood x Gender of Police Victim: Male	0.1081***	0.1054***	0.0933***	0.1012***
	(0.0333)	(0.0333)	(0.0259)	(0.0316)
Post OIF x Treated Neighborhood x Cause of Death: Gunshot	0.0217	0.0291	-0.0093	-0.0024
	(0.0414)	(0.0391)	(0.0258)	(0.0301)
Zip code-level Controls				
Percent Unemployed	0.0197	0.0197	0.0058	0.0058
	(0.0114)	(0.0114)	(0.0070)	(0.0070)
Percent Female	0.0078**	0.0077**	0.0155***	0.0155***
	(0.0033)	(0.0033)	(0.0039)	(0.0039)
Mean Household Income	0.0000**	0.0000**	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Percent Below Poverty	0.0188***	0.0188***	0.0178***	0.0178***
	(0.0028)	(0.0028)	(0.0019)	(0.0019)
Constant	-0.5862***	-0.6855***	-0.9116***	-0.9095***
	(0.1767)	(0.1680)	(0.2057)	(0.2062)
Observations	1,378	1,378	1,378	1,378
R-squared	0.8084	0.8107	0.8437	0.8483
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Neighborhood			YES	
Neighborhood by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the neighborhood level.

Table 3.11 4 Weeks after OIF Diff-in-Diff – 911 Calls by Neighborhood

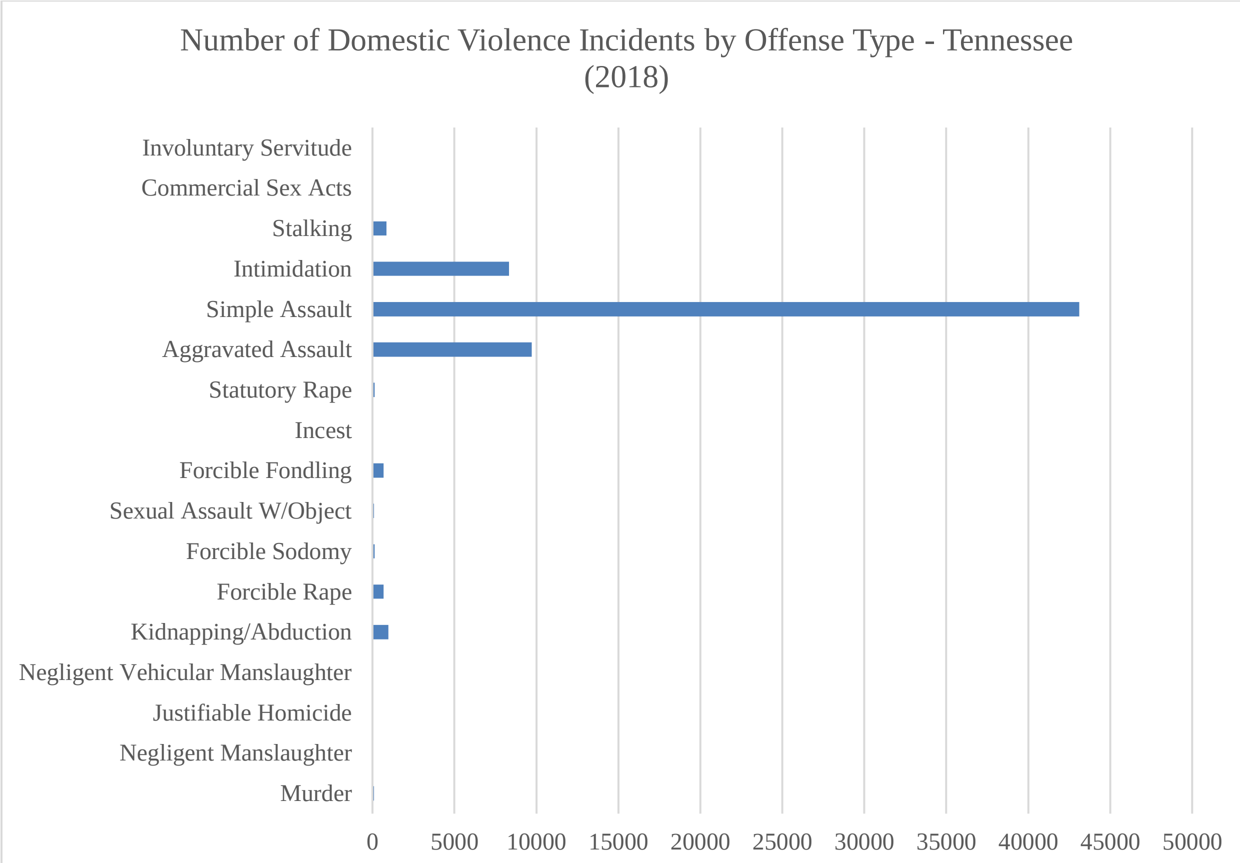
Dependent Variable: *911 Calls*	(1) Model 4	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	-0.2381	-0.4150	-0.8784**	-0.8747**
	(0.4076)	(0.4091)	(0.3098)	(0.3148)
Treated Neighborhood	4.0601*	4.0274	0.5469	0.9485**
	(2.2243)	(2.5134)	(0.3916)	(0.3711)
Post OIF x Treated Neighborhood	-4.7594***	-4.8156***	-1.7484*	-1.7205*
	(1.2226)	(1.3053)	(0.8671)	(0.9286)
Post OIF x Treated Neighborhood x Race of Police Victim: White	-4.2061	-4.4180	-2.4105	-1.2240
	(3.8740)	(4.7499)	(1.5708)	(1.5773)
Post OIF x Treated Neighborhood x Gender of Police Victim: Male	4.5407***	4.5197***	2.1156	2.0935
	(1.3503)	(1.0902)	(1.2949)	(1.3468)
Post OIF x Treated Neighborhood x Cause of Death: Gunshot	4.8558**	4.9873**	1.2000	1.0631
	(1.8329)	(1.7517)	(0.7837)	(0.8139)
Zip code-level Controls				
Percent Unemployed	-0.0802	-0.0789	-0.6493	-0.6493
	(1.1235)	(1.1269)	(1.1623)	(1.1670)
Percent Female	-0.6283	-0.6283	0.1445	0.1445
	(0.5737)	(0.5745)	(0.7419)	(0.7449)
Mean Household Income	0.0001	0.0001	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Percent Below Poverty	0.9343***	0.9341***	0.6076**	0.6076*
	(0.2908)	(0.2921)	(0.2759)	(0.2770)
Constant	27.8475	29.9205	12.7824	12.7447
	(30.4092)	(30.5678)	(40.1394)	(40.2800)
Observations	1,536	1,536	1,536	1,536
R-squared	0.6896	0.6943	0.9052	0.9097
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Neighborhood			YES	
Neighborhood by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the neighborhood level.

Table 3.11A 2 Weeks after OIF Diff-in-Diff – 911 Calls by Neighborhood

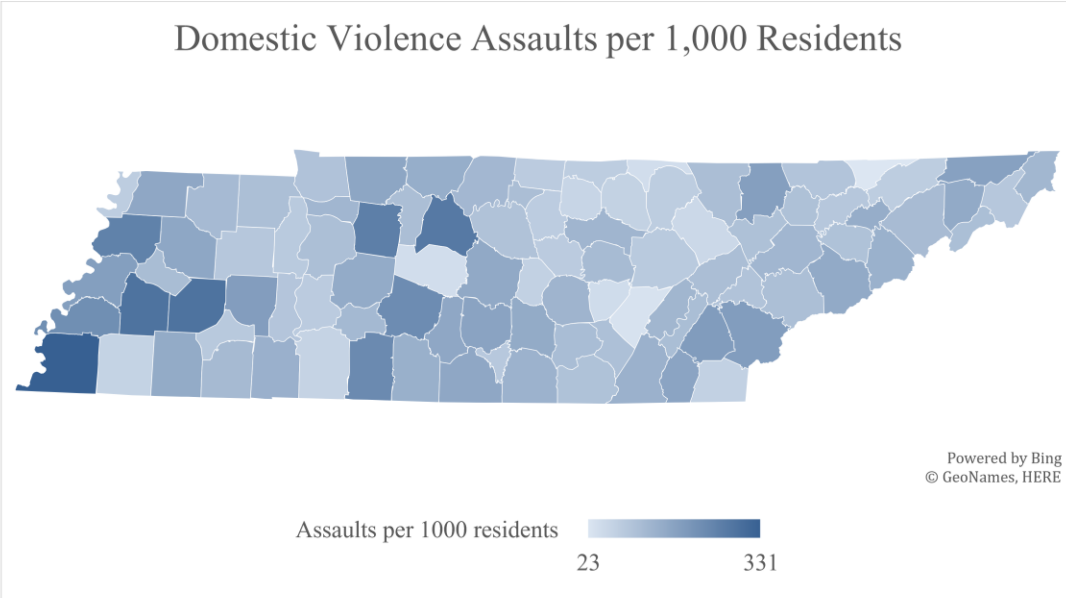
Dependent Variable: *911 Calls*	(1) Model 4	(2) Model 2	(3) Model 3	(4) Model 4
Post OIF	0.2733	0.2637*	0.2380*	0.2361*
	(0.1551)	(0.1266)	(0.1301)	(0.1311)
Treated Neighborhood	0.5321	0.5261	0.4195***	0.4762***
	(0.3455)	(0.3449)	(0.0911)	(0.0946)
Post OIF x Treated Neighborhood	-1.0354***	-0.9866***	-0.8001***	-0.7847***
	(0.2824)	(0.2579)	(0.1349)	(0.1399)
Post OIF x Treated Neighborhood x Race of Police Victim: White	-1.7375*	-1.8573**	-0.4630*	-0.3652
	(0.8500)	(0.8068)	(0.2362)	(0.2315)
Post OIF x Treated Neighborhood x Gender of Police Victim: Male	1.0581***	1.1305***	0.7497***	0.7480***
	(0.2570)	(0.2466)	(0.2385)	(0.2361)
Post OIF x Treated Neighborhood x Cause of Death: Gunshot	0.8013*	0.7207*	0.2894	0.2379
	(0.3825)	(0.3763)	(0.1977)	(0.2020)
Zip code-level Controls				
Percent Unemployed	-0.0281	-0.0280	-0.2125*	-0.2125*
	(0.1510)	(0.1513)	(0.1148)	(0.1154)
Percent Female	-0.1205	-0.1205	-0.1335**	-0.1335**
	(0.1021)	(0.1022)	(0.0473)	(0.0475)
Mean Household Income	0.0000	0.0000	-0.0000*	-0.0000*
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Percent Below Poverty	0.1794***	0.1793***	0.1619***	0.1619***
	(0.0473)	(0.0474)	(0.0252)	(0.0253)
Constant	4.6718	5.1975	9.6371***	9.6278***
	(5.2676)	(5.2629)	(2.2508)	(2.2621)
Observations	1,378	1,378	1,378	1,378
R-squared	0.6854	0.6899	0.8858	0.8893
Month FE	YES	YES	YES	YES
Year FE		YES	YES	
Neighborhood			YES	
Neighborhood by Year FE				YES

Note: *, **, and *** indicate significance at the 10%, 5%, and 1% significance levels, respectively. Standard Errors are clustered at the neighborhood level.



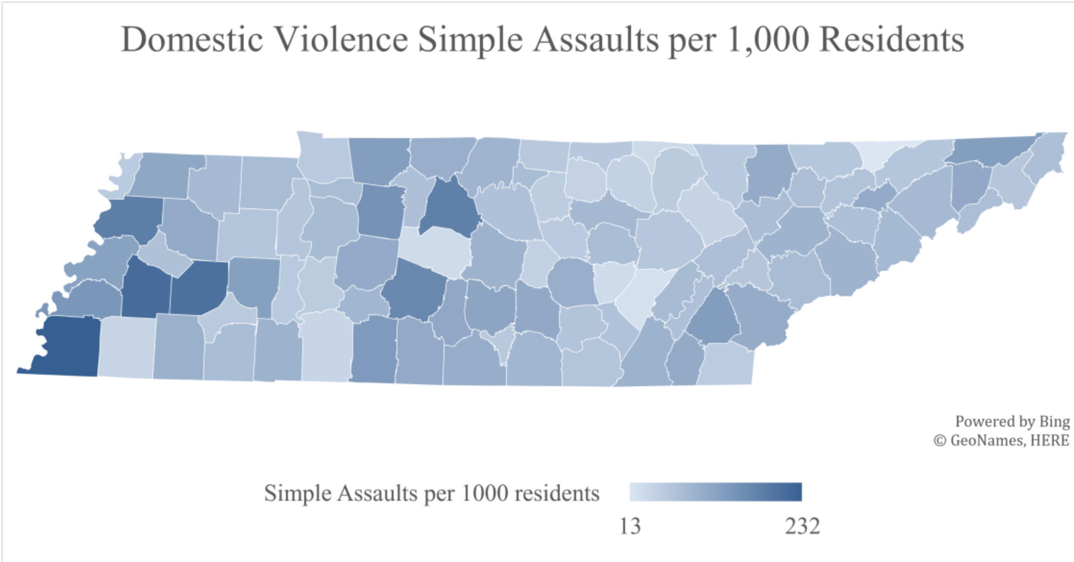
Source: TN Crime Insight – Domestic Violence Data, 2018.

Figure 3.1 Domestic Violence Incidents by Offense Type - Tennessee (2018)



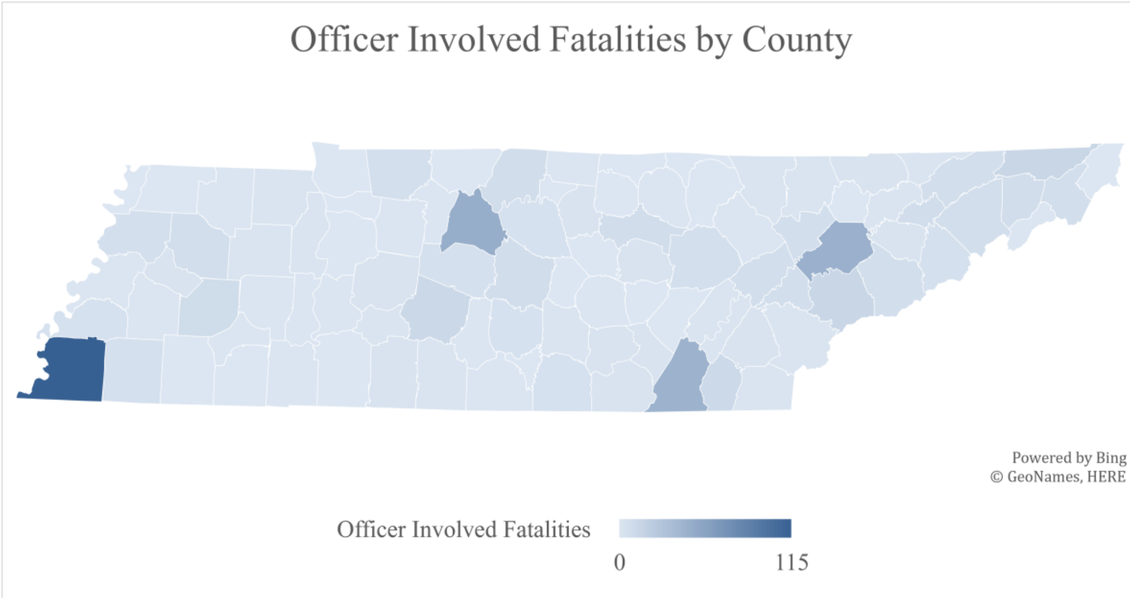
Note: Data from TN Crime Insight – Domestic Violence/Domestic Situation Data 2001-2017.

Figure 3.2 Domestic Violence Assaults by County



Source: TN Crime Insight – Domestic Violence/Domestic Situation Data 2001-2017.

Figure 3.3 Domestic Violence Simple Assaults by County



Note: Data from Fatalencounters.org, 2000-2019.

Figure 3.4 Officer Involved Fatalities by County

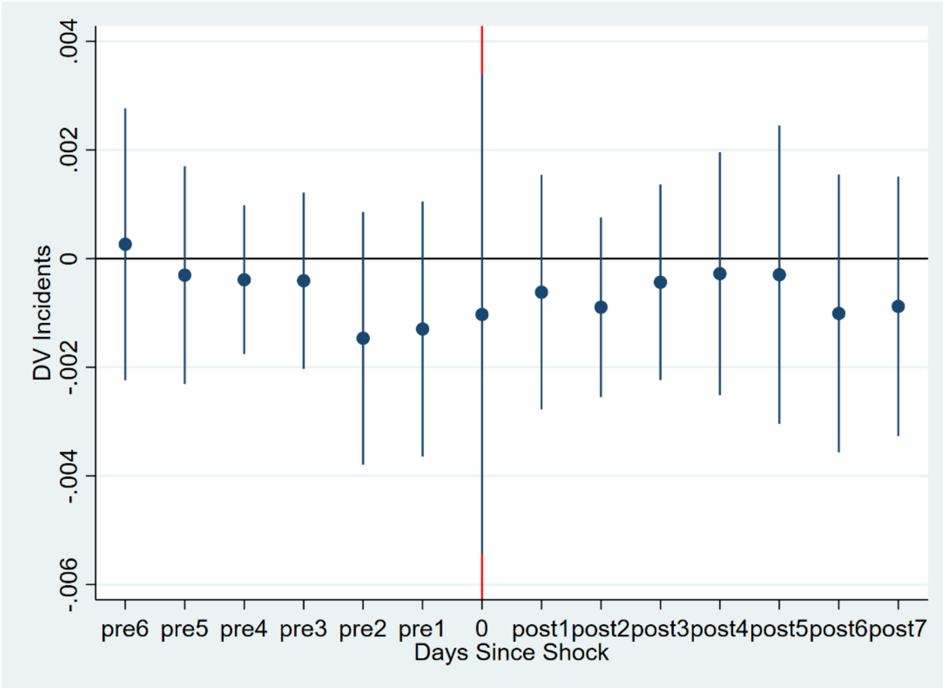


Figure 3.5 14-day Event Study – Simple Assaults per 1000 residents

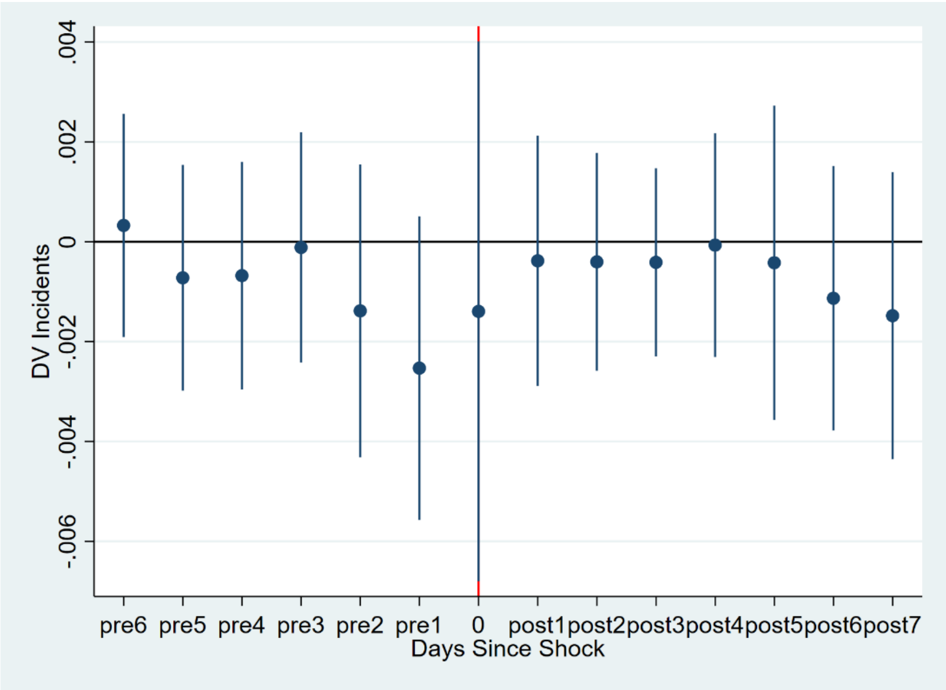


Figure 3.6 14-day Event Study – All Assaults – per 1000 residents

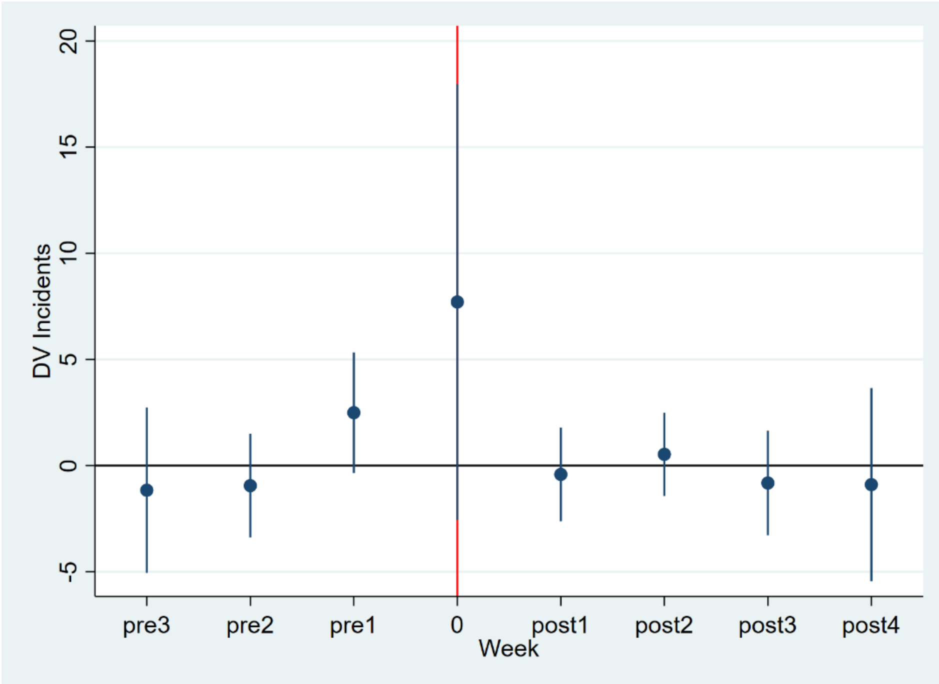


Figure 3.7 8-week Event Study – Simple Assault

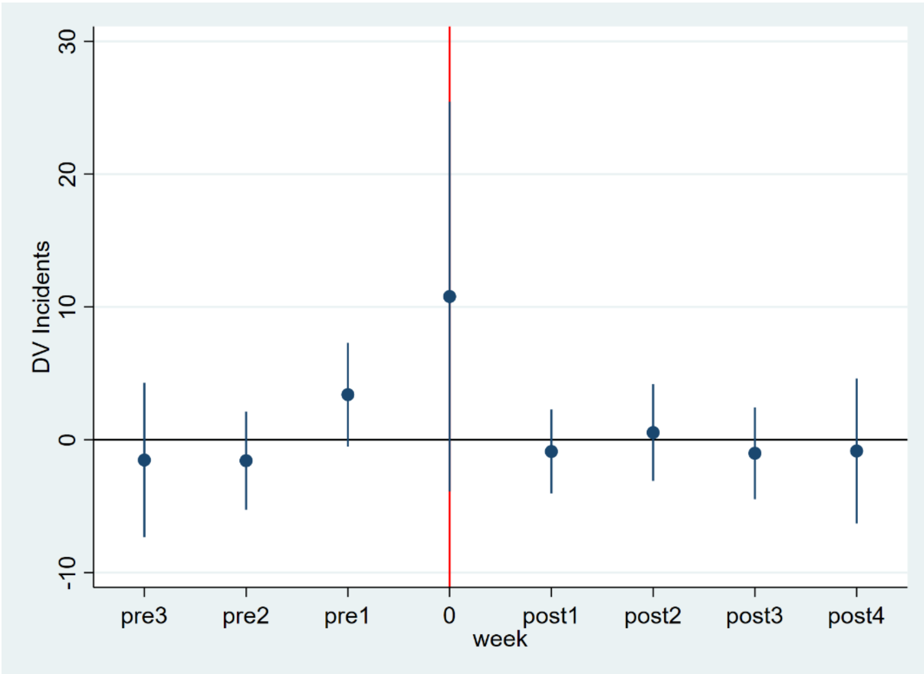
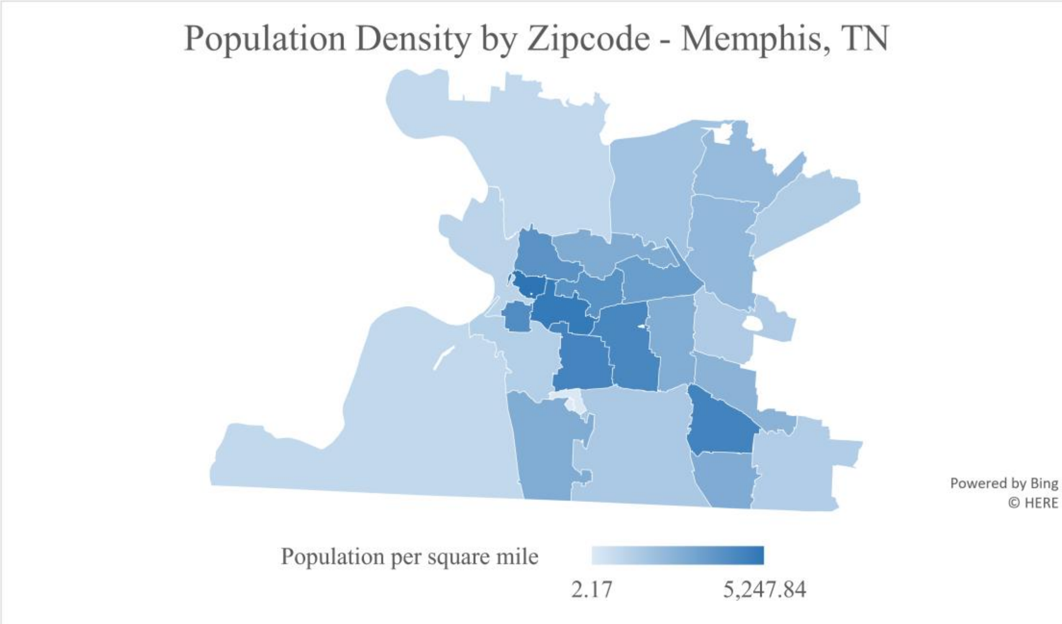
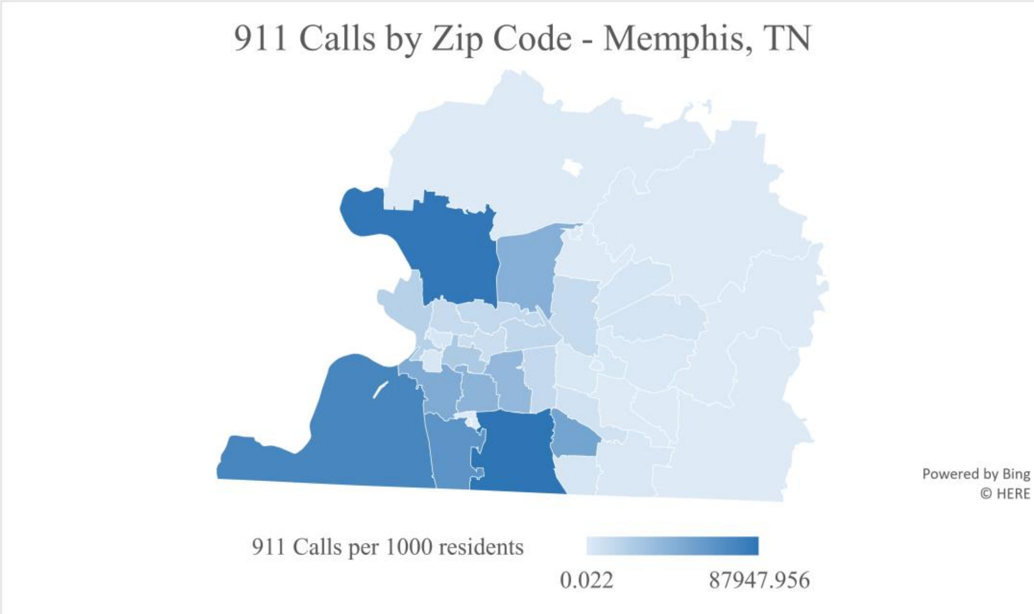


Figure 3.8 8-week Event Study – All Assault



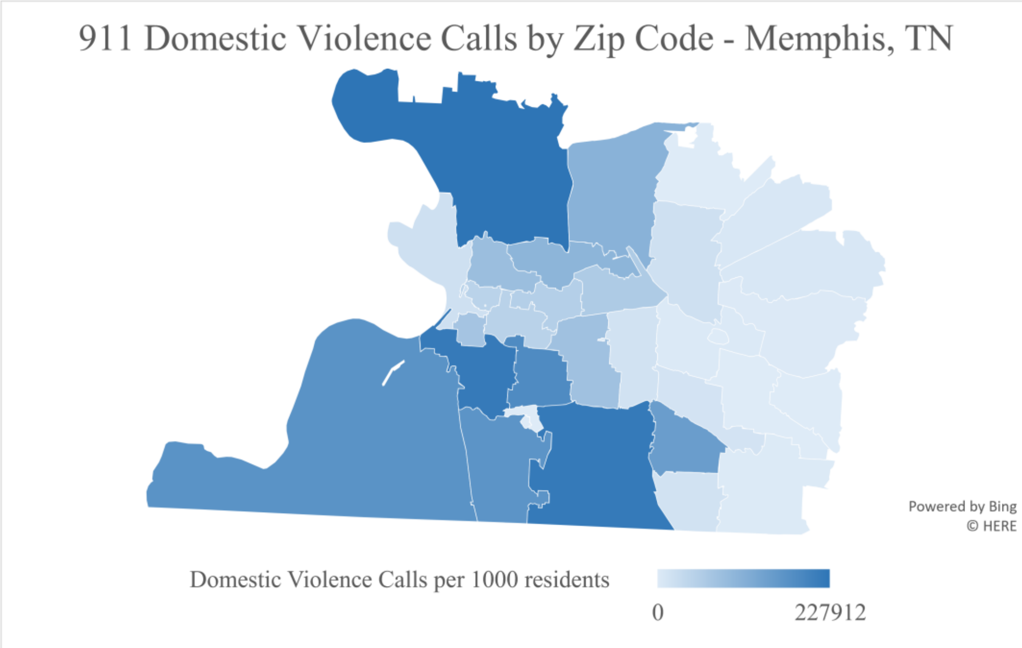
Note: Data from U.S. Census Bureau, American Community Survey, 2017 ACS 1 – Year Estimates, Demographic and Housing Estimates, DP05.

Figure 3.9 Population by Zip Code – Memphis, TN



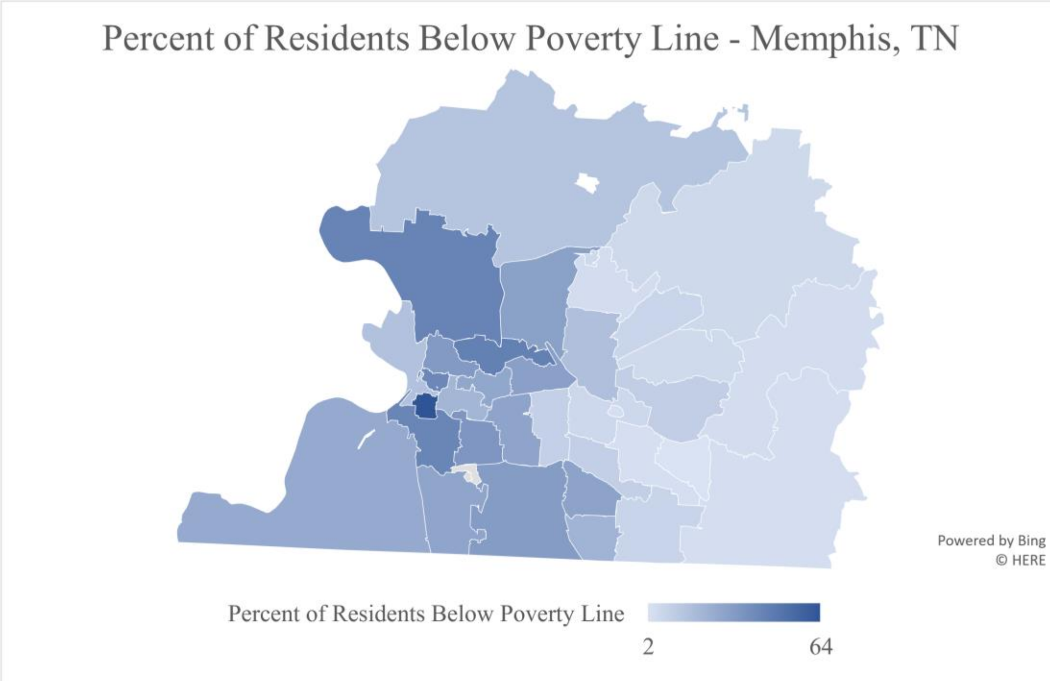
Note: Data from FOIA 911 Calls for Service – Memphis, TN.

Figure 3.10 911 Calls by Zip Code – Memphis, TN



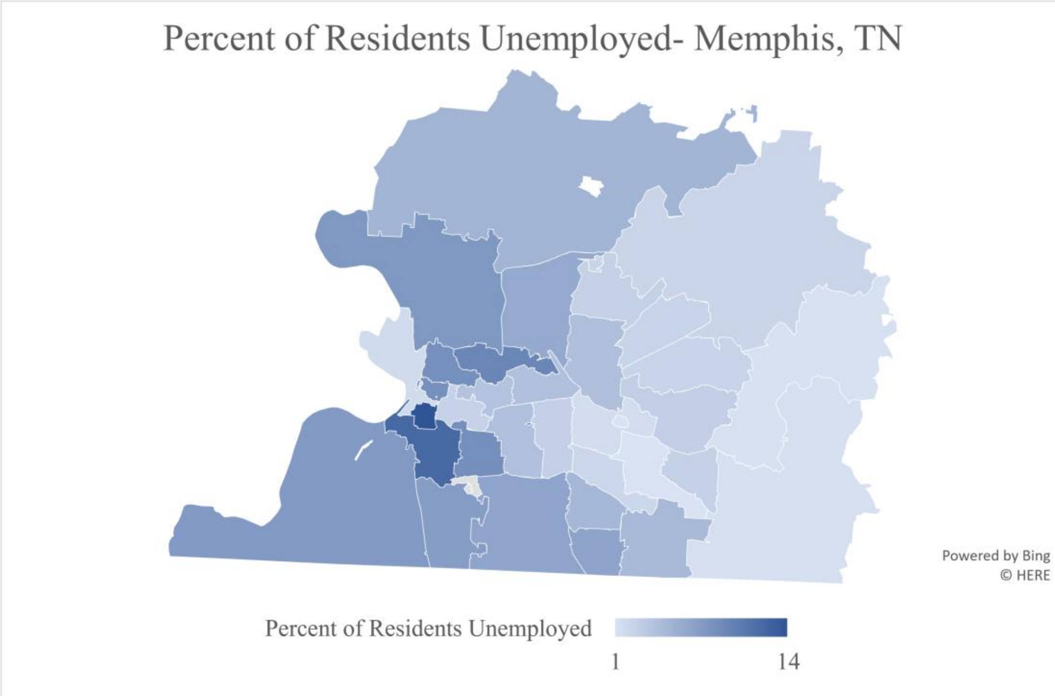
Note: Data from FOIA 911 Calls for Service – Memphis, TN.

Figure 3.11 911 Domestic Violence Calls by Zip Code – Memphis, TN



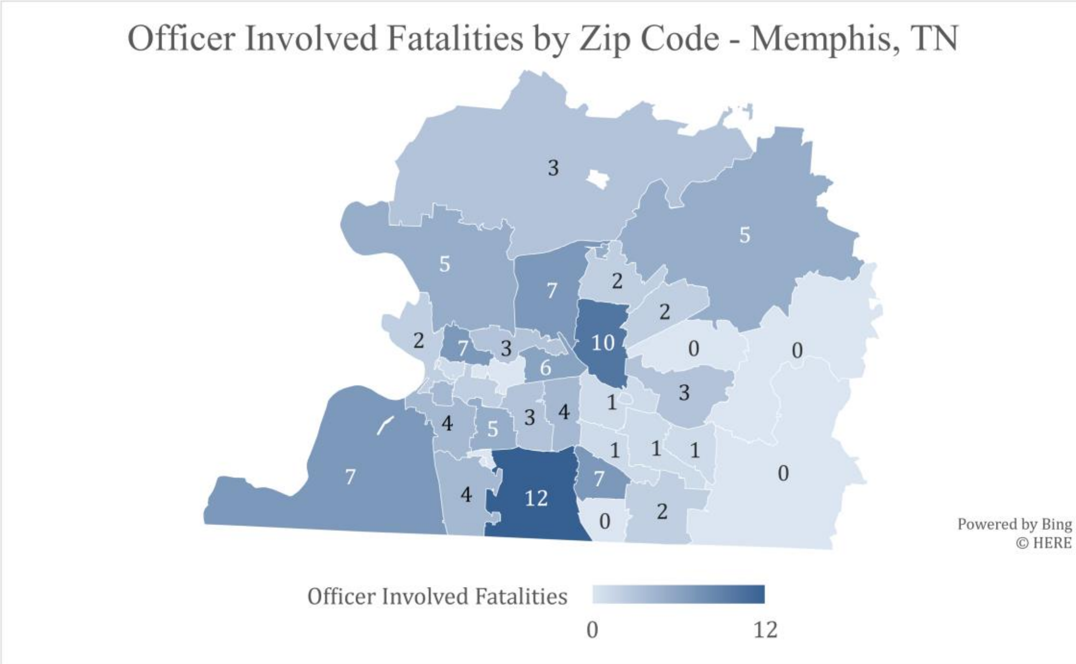
Note: Data from U.S. Census Bureau, American Community Survey, 2017 ACS 1 – Year Estimates, Demographic and Housing Estimates, DP05.

Figure 3.12 Percent of Residents Below Poverty Line – Memphis, TN



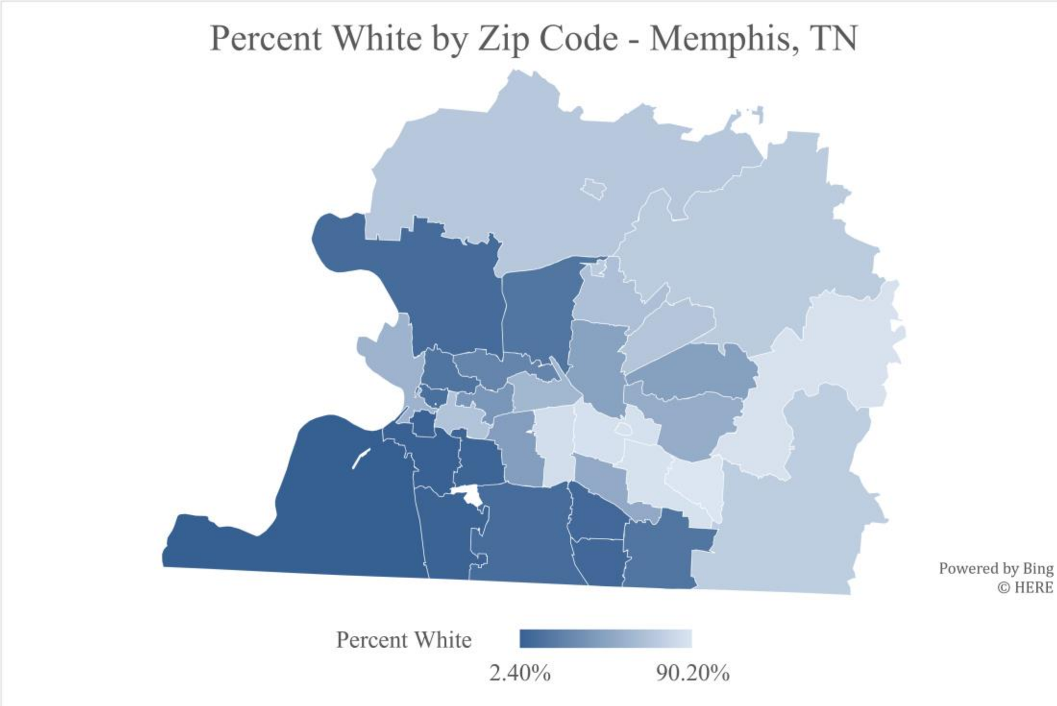
Note: Data from U.S. Census Bureau, American Community Survey, 2017 ACS 1 – Year Estimates, Demographic and Housing Estimates, DP05.

Figure 3.13 Percent of Residents Unemployed – Memphis, TN



Note: Data from FOIA 911 Calls for Service – Memphis, TN.

Figure 3.14 Officer Involved Fatalities by Zip Code – Memphis, TN



Source: 2019 Poverty Fact Sheet. Elena Delavega, PhD, MSW, School of Social Work, University of Memphis, & Gregory M. Blumenthal, PhD, GMBS Consulting

Figure 3.15 Percent White by Zip Code – Memphis, TN

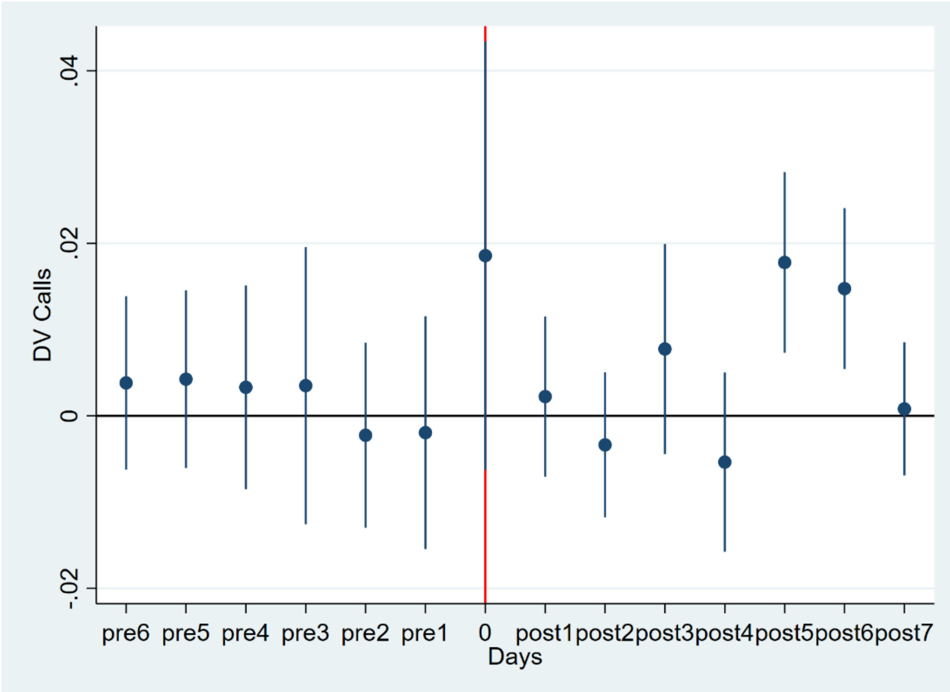


Figure 3.16 14-Day Event Study – 911 Domestic Violence Calls

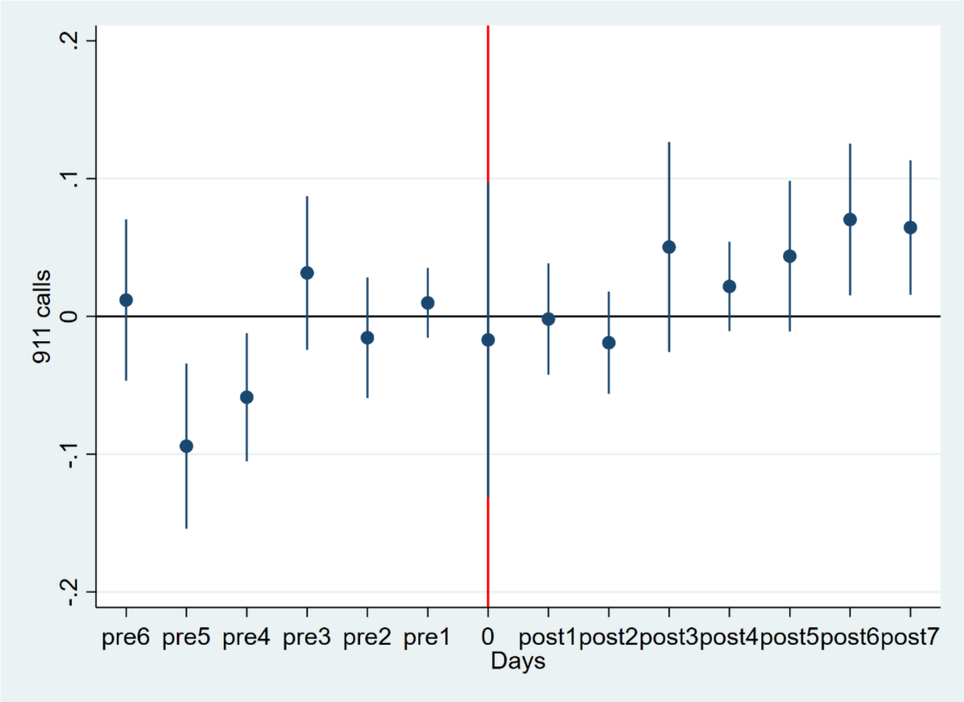


Figure 3.17 14-Day Event Study – 911 Calls

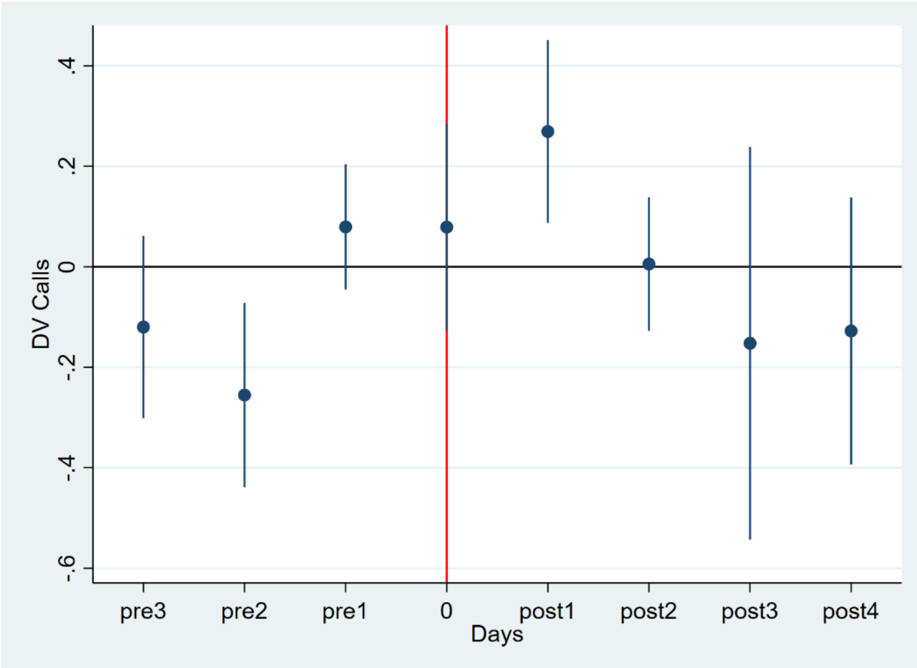


Figure 3.18 8-Week Event Study – Domestic Violence Calls

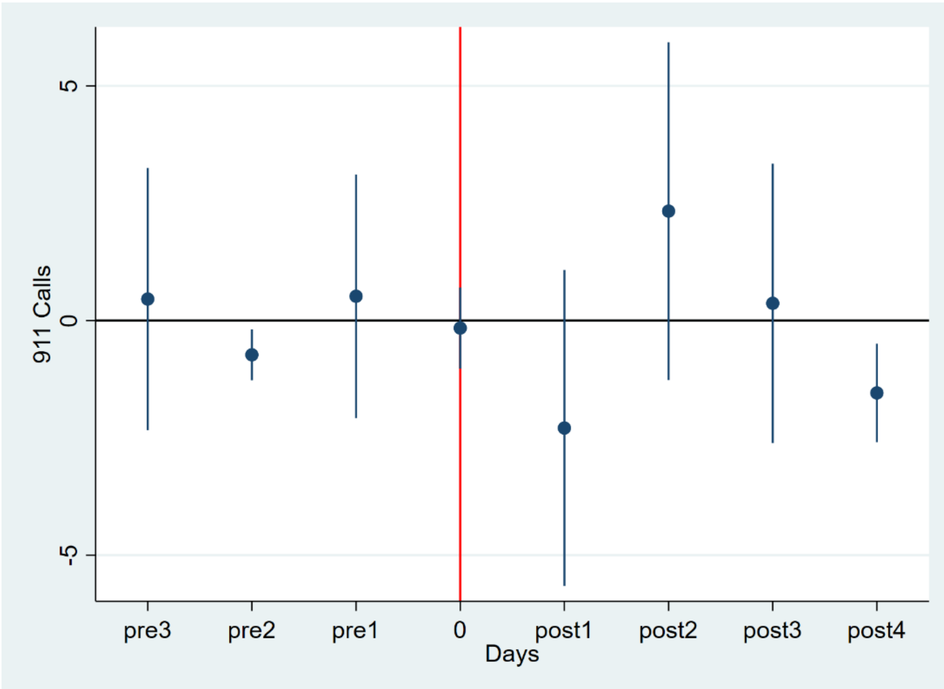


Figure 3.19 8-Week Event Study – 911 Calls

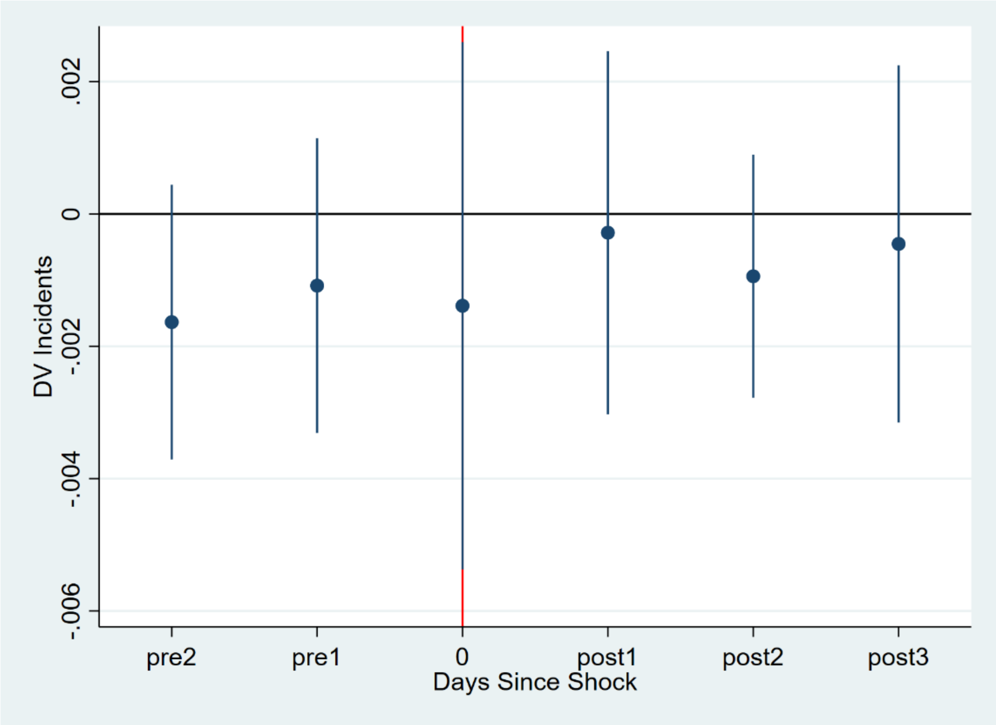


Figure 3.A.1 3-Day Event Window – Simple Assaults per 1000 residents

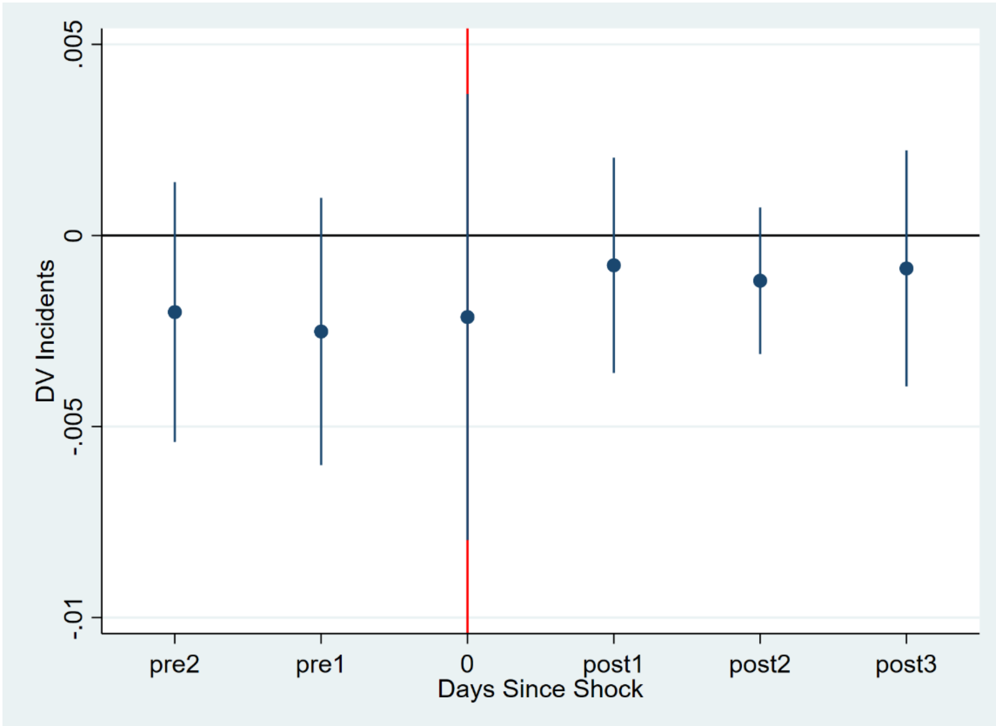


Figure 3.A.2 3-Day Event Window – All Assaults per 1000 residents

Vita

Adrienne Welch Sudbury is an economist interested in behavioral and public economics. She earned her B.S. in Economics at the University of Tennessee, Chattanooga. While pursuing her M.S. and Ph.D. in Economics at the University of Tennessee, Knoxville, she has won multiple awards related to her research and teaching including the Haslam College of Business Award for Outstanding Doctoral Student Teacher, the Charles B. Garrison Award for Excellence in Teaching, and the J. Fred and Wilma Holly Award for Excellence in Presented Research.

After graduation, Adrienne is moving to Farmville, VA where she has accepted an Assistant Professor position in the Department of Economics and Accounting at Longwood University.