

# Social Incentives in Contributions

Field Experiment Evidence from the 2012 U.S. Presidential Campaigns<sup>\*</sup>

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This Draft: May 2014. First Draft: November 2013.

## Abstract

This paper exploits the unique institutional setting of U.S. campaign finance to provide new evidence on social incentives in political participation. We conducted a field experiment in which letters with individualized information about campaign contributions were sent to 91,998 contributors in the 2012 U.S. presidential election. The effect of those letters on recipients' subsequent contributions are examined using administrative data. We find that exogenously making an individual's contributions more visible to her neighbors significantly increased her subsequent contributions if the majority of her neighbors support her same party, but decreased her contributions if the majority of her neighbors support the opposite party. This constitutes evidence that individuals give preferential treatment to neighbors of the same party. In another treatment arm, we randomized the information observed by recipients about their neighbors' contribution behavior. Consistent with existing evidence on social norms, individuals contribute more when neighbors of the same party contribute higher average amounts. Furthermore, we find that individuals also care about the total amounts raised by the same and opposite parties. These findings have implications for the design of optimal disclosure policies, for the understanding of geographic polarization and for fundraising strategies.

*JEL Classification:* C93, D03, D64, D71, D72, D83, H41.

*Keywords:* social incentives, signaling, social norms, field experiment.

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<sup>\*</sup>Ricardo Perez-Truglia wants to give special thanks to Raj Chetty for his advice and encouragement. Funding for the mailing experiment and the post-election survey was generously provided by the Lab for Economic Applications and Policy and the Warburg Funds (Harvard University) and by CEDLAS-FCE-UNLP. Alberto Alesina, Robert Barro, Raj Chetty and Monica Singhal provided valuable feedback in the early stage of the project. We thank James Alt, Dan Bernhardt, Nicolas Bottan, Stefano DellaVigna, Rafael Di Tella, John Friedman, Matthew Gentzkow, Edward Glaeser, Nathan Hendren, Larry Katz, Roger Koenker, László Sándor, Jesse Shapiro, James Snyder, Ugo Troiano, Martin Tetaz, Dustin Tingley and Rodrigo Wagner for their valuable feedback, as well as that of seminar participants. Julian Amendolagine provided excellent research assistance. We also would like to thank Belynda Bady, Jane Calhoun, Dean Gallant, Diane Frishman, Helena Rovner and Norma Truglia for their help while conducting the field experiment and the survey. This project was reviewed and approved in full and in advance by the Committee on the Use of Human Subjects in Research at Harvard University.

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# 1 Introduction

Partisan conflict, growing polarization in Congress and an increasingly divided electorate have become features of American political life (McCarty, Poole and Rosenthal, 2006). The government shutdown of 2013 is a reminder that the uncertainty brought by these divisions can have real economic costs (Azzimonti 2011; Baker, Bloom and Davis, 2012). This growing polarization is also palpable in everyday life, with supporters from both parties becoming more isolated from each other (Cho, Gimpel and Hui, 2013), exacerbating the differences in their views (Glaeser and Sunstein, 2009; Gentzkow and Shapiro, 2011), reducing their trust in government (Layman et al., 2006), imposing efficiency costs in the formation of social networks (Zanella, 2007), and even with manifestations of hostility between citizens following the disclosure of political contributions (Briffault, 2010; La Raja, 2013). The intrinsic social nature of political participation can provide a natural explanation for these phenomena. For example, the preferential treatment towards individuals of similar political beliefs tends to induce geographical partisan segregation, and at the same time it constitutes a disincentive for political participation for local minorities. Both factors contribute to an environment favorable to polarization (Perez-Truglia, 2014). We provide unique evidence on how political participation is affected by social incentives operating in a partisan context. This evidence is based on a field experiment conducted during the 2012 U.S. presidential election that exploits the exceptional institutional setting of campaign finance.

Participation in partisan activities often entails sending a signal to others about one's political preferences. This can be an intended or an unintended consequence of different forms of participation, such as making a campaign contribution, attending a rally, sharing a political cause on social networks, or merely discussing policy issues with others. This high visibility of individual political beliefs implies that participation can be very sensitive to the social environment. For instance, Republicans may act toward their Republican peers better than they act towards their Democratic peers, and Democrats may similarly favor other Democrats. As a result, revealing one's affiliation through political participation can result in more fulfilling social interactions with supporters of the same party, but in harsher day to day dealings with supporters of the opposite party. The social environment may also matter for other reasons. Individuals following social norms will attempt to acquire information about the behavior of their peers, and act accordingly. These mechanisms imply that individual's political participation is shaped by the composition and the participation behavior of others in their reference group. This interaction between the visibility of individual actions and preferences and the social environment has important implications for interpersonal and political conflict, geographic polarization and disclosure policies, among others.

The effects of social incentives have been studied in non-partisan contexts such as charitable giving, voter turnout and energy conservation,<sup>1</sup> but there is much fewer research about the role of social incentives on partisanship. The identification of social incentives is often deemed a cumbersome empirical task (Angrist, 2013), and this is also true in the case of social incentives in political participation. The literature provides numerous pieces of suggestive evidence. For instance, Gimpel, Lee and Kaminski (2006) show that local networks and social influence are key drivers of campaign contributions. However, the direction of causality in these studies remains undetermined. Intuitively, it is difficult to tease apart whether Democrats are more politically active when living near other Democrats, or whether more active Democrats are more likely to live near other Democrats (Perez-Truglia, 2014). As discussed below, the welfare costs of disclosure policies depend crucially on the specific mechanisms that may be operating. Our experimental design overcomes the limitations of the existing observational evidence, and allows to identify the direction of causality and the precise mechanisms at play.

Federal law dictates that campaign committees must report the identity and detailed information of individuals who contribute over \$200 to the Federal Election Commission (FEC). The FEC makes these contribution records not only publicly available but, most importantly, easily accessible online. The data is updated on a monthly basis over the course of an election cycle and all contributions and contributors are listed in a disaggregated manner. On its website, the FEC offers a tool that allows users to search individuals by full name, address and other characteristics (e.g., employer). The database displays personal information as well as amounts contributed, candidates contributed to and the dates of the contributions (see Appendix F for more details). The fact that an individual's contribution is observable to others, and that the individual can observe and thus be influenced by the contributions of others, makes it a very convenient context for studying social incentives in political participation. For our experimental analysis, we selected individuals who had made a contribution to a presidential campaign between April 1, 2011 and April 1, 2012 to form our subject pool. In May 2012, we sent letters with individualized information related to campaign contributions to a random sample of 91,998 of the 191,832 individuals in the subject pool. The letters contained information with non-deceptive exogenous variations specifically designed to test specific hypothesis about how social incentives operate. We then used the FEC data to observe subsequent contributions for all the experimental subjects during the rest of the electoral cycle, and to estimate the effects of the variations on the recipients' contribution patterns.

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<sup>1</sup>See for instance Frey and Meier (2004) and Martin and Randal (2008) for charitable contributions, Gerber, Green and Larimer (2008) for voter turnout, and Allcott (2011) for energy conservation.

The field experiment was designed to measure two broad types of social incentives. The first type, referred to as *signaling effect*, reflects the tendency of the visibility of an individual’s contribution to affect her contribution behavior. Contribution information may signal unobserved characteristics about the contributor, including wealth, political affiliation and strength of that affiliation. To test the presence of this type of effects, we induced experimental variation in the recipient’s perception of the visibility of her own contribution among neighbors. We achieved this by sending letters providing information about the public nature of campaign contribution records and about the FEC website’s online search tool to a random sample of our experimental subjects. We generated variation by randomly assigning the recipients to one of two sub-treatments. Individuals in one sub-treatment received a letter indicating that theirs was the only household in the area randomly chosen to be sent a letter of this type. Individuals in the other sub-treatment received a letter that indicated that their household and other households in the area were randomly selected to receive a letter of this type. The differential effects of these two sub-treatments can be interpreted as the product of differences in the visibility of the recipients’ contributions. We find that making a recipient’s contribution more visible in this way significantly increased her subsequent contributions if the majority of her neighbors supported her same party, but it decreased contributions if the majority of her neighbors supported the opposite party. These effects are not only statistically significant but also large in magnitude. We develop a signaling model where contributions signal an agent’s party and strength of affiliation, which is consistent with these empirical results.

The second type of social incentive, referred to as *informational effect*, reflects the tendency of an individual’s contribution to be affected by her perception of the reference group’s contribution behavior. That is, while signaling effects are the result of feeling observed by others, informational effects are the result of observing others’ contributions. Social norms, a type of informational effects, refer to the tendency of an individual to form beliefs about the “right” amount to contribute based on the contributions made by other individuals in her reference group (Akerlof and Kranton, 2000). However, social norms are not the only reason why an individual might care about contributions made by others. Our experimental design allows us to disentangle among various of these competing mechanisms.

In the second treatment arm, we designed a letter that did not mention the FEC website’s search tool, but instead provided a semi-anonymized list of the names, amount and party contributed for nine contributors from the recipient’s area of residence. We randomly varied the parameters used to select the neighboring contributors to be included in the list. This created non-deceptive exogenous variation along multiple dimensions of the list sent to the subject: e.g., some subjects were mailed lists with a higher number of own-party con-

tributors, while others were mailed lists with higher average contributions by opposite-party contributors, and so on. The results from this treatment arm suggest that an individual's contribution is affected by her perception of others' contribution behavior. Consistent with the formation of a social norm about contributions, individuals give more when neighbors of the same party contribute higher average amounts. This indicates that, when forming the social norm, individuals do not consider everyone's action but only those of same-party supporters, with whom the individual identifies the most (Akerlof and Kranton, 2000). Individuals do not seem to contribute more when party contributions appear even, which serves as evidence against the hypothesis that individuals contribute because they believe they are making a pivotal contribution. The evidence also suggests that individuals contribute less when the perceived total contributions to their own party is higher than that of the opposite party, which can be rationalized as a form of free-riding in warm glow giving (Andreoni, 1989).

The study is motivated by the FEC's disclosure policy, and the experiments are based on the corresponding public database of contributors. However, social incentives affect political participation in many different forms of social interactions, such as face to face contact. There are clear limitations in the ability to collect data on those interactions, and to design studies to capture the importance of social incentives in these contexts. The unique features of the FEC disclosure policy constitute the backbone of the research design, but our aim is not limited to evaluate the FEC's policy or to gauge the impact of a given information campaign. The main purpose of our study is to identify some of the channels through which social incentives affect individual behavior in general, and in this sense we devised our interventions as mechanism experiments to allow us to extrapolate to broader contexts of political participation and individual behavior.

Our paper has implications for political participation, and also more specifically for the understanding of the drivers of individual campaign contributions. This type of contributions represent a large fraction of the resources devoted to funding campaigns in the United States: about 80% of the \$1.7 billion raised in the 2012 presidential race was comprised of individual contributions. The political economy of these contributions and their regulation, such as contribution limits and disclosure policies, are salient topics of public debate and scholarly research (e.g., Campante, 2011; Gilbert, 2013). The analysis of the consequences of individual campaign contributions requires establishing their determinants, and these are not yet fully understood (Ansolabehere et al., 2003). Our evidence indicates that social incentives, such as signaling and social norms, play a significant role in campaign contributions. Moreover, we find that individuals do not seem to contribute more in tight races, which is consistent with the predominant (although contested) view that individuals contribute to campaigns

primarily because of a consumption motive rather than an investment motive (Ansolabehere et al., 2003).

The evidence on social incentives is also a key input for the design of optimal disclosure policies. The cost of collecting and disseminating data has been falling dramatically over the past decades. As a result, both the private sector and the government face the quandry of whether and how to share part of the data with the general public. However, there is little research on the advantages and disadvantages of different disclosure policies.<sup>2</sup> Our evidence indicates that, due to social incentives, disclosure policies can have first-order effects on the actions of the individuals whose information is being disclosed. Most importantly, we find evidence that the public nature of contribution records has a chilling effect on local minorities, presumably because of fear of being treated unfavorably by the supporters of the majority party. These minorities thus face a dilemma between not contributing to the party or causes they support, or risking to face negative repercussions, with negative consequences in terms of their welfare. Reporting all political contribution activity to a regulator such as the FEC is certainly a key aspect of ensuring compliance with the law and fighting corruption. However, the current regulations state that contribution records for even comparatively small amounts are also made publicly available and easily accessible to the general public. This aspect of the regulation has recently sparked a heated debate in the media and among politicians and policy-makers. One of the main justifications for this far-reaching disclosure policy, as argued by the U.S. Supreme Court and several others, is that the full disclosure of campaign contributions may provide valuable information to voters without any significant costs (Gilbert, 2013). Our evidence suggests that, in the case of individual contributions, there are negative unintended consequences to these dissemination efforts, such as a chilling effect on local minorities and their potential marginalization from the political process. While we do not intend to provide a normative argument about the optimal disclosure policy for individual campaign contributions, our evidence contributes to a fuller assessment of the costs of the current implementation of the disclosure policy, including unintended equilibrium effects. The presence of these effects is facilitated by technological innovations that were probably not envisaged nor considered at the time of the enactment of the current legislation in the early 1970s.

Insights on social incentives are also of practical interest to political campaigns, charities and other organizations that seek to improve fundraising strategies. For example, they can identify more cost-effective alternatives to traditional economic incentives. Our experimental findings suggest that contributions can serve as signals of unobserved contributor charac-

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<sup>2</sup>Two exceptions are the recent studies of the effects of disclosure of taxation on reported income in Norway (Slemrod et al., 2013) and Japan (Hasegawa et al., 2014).

teristics. However, as opposed to the rest of the literature, we show that in the context of campaign contributions the partisan composition of the reference group critically affects the optimal message to be sent through the contribution. Our experimental findings also indicate that disseminating unbiased information about others' contribution patterns can increase contributions. While this strategy is routinely employed by organizations to raise funds, our evidence also suggests that some of these information dissemination efforts may be self-defeating, and that the benefits of information provision must be weighted against the potential costs of free-riding effects.

In this respect, our paper is related to a group of laboratory and field experiments that study the role of social incentives in various forms of pro-social behavior. A group of studies find that an individual's donations increases when the individual's behavior is observable to others (Andreoni and Bernheim, 2009; Andreoni and Petrie, 2004; Karlan and McConnell, 2012; DellaVigna et al., 2012) and when the individual is informed about the donations of others (Frey and Meier, 2004; Allcott, 2011; Augenblick and Cunha, 2011). This paper contributes to this literature in two aspects. First, we look at campaign contributions, which is a type of pro-social behavior that was not systematically studied before. Focusing on campaign contributions allows us to overcome some of the limitations of studies of social incentives and charitable giving. In these studies, researchers typically can only observe contributions to a single charity, which reduces the scope of hypotheses to test, ruling out for instance crowding out effects. Moreover, researchers usually observe only relatively small donations. Second, we address remaining questions about the precise mechanisms through which social incentives operate by separating the effects of being observed by others from those of observing the behavior of others.

The paper is organized as follows. Section 2 presents the research design. It discusses the relevant hypotheses and the experimental design to test these hypotheses, as well as the identification strategies to capture signaling and informational effects in the context of our experiment. Section 3 presents the data sources, the outcomes of interest and other details of the implementation of the field experiment. Sections 4 and 5 present the empirical results on signaling effects and informational effects, respectively. Section 6 discusses the implication of these results. The final section concludes.

## 2 Research Design

### 2.1 Relevant Hypotheses

Individual campaign contributions are a form of political participation, and we can expect that social interactions play a role in their determination. The context of this study is the particular setting of campaign finance in the United States, where individual contributions above a relatively low level must be fully disclosed and are publicly accessible on the Internet. Since a large proportion of these contributions are not anonymous, an individual's social context and the incentives she faces in this context may shape this and other forms of political participation. A first type of social incentive is given by the contributor feeling observed by others and therefore acting to influence the behavior of others in those social interactions. We denominate these signaling effects. There is some evidence that individuals condition their behavior based on whether their actions are observed by others in the context of charitable giving and related pro-social behavior. For example, Andreoni and Petrie (2004) find that individuals give more in laboratory experiments when donations are not confidential, and Karlan and McConnell (2012) found higher donations to a university when the amounts given were publicized.<sup>3</sup> In a seminal contribution, Gerber et al. (2008) conducted a field experiment where individuals were sent letters with lists of neighbors and their previous voting turnout history. The letters also promised to publicize the recipient's future voting behavior to her neighbors. The authors find that these letters had a significant positive effect on subsequent turnout, which they interpret as arising from some combination of social norms and feeling monitored by neighbors.<sup>4</sup> A more general interpretation of these findings is that individuals want to act pro-socially as a way of signaling to others that they are generous (Andreoni and Bernheim, 2009).

Compared to charitable giving or voter turnout, campaign contributions present a richer context for the study of signaling effects. While most would consider charitable contributions and voting to be socially desirable, contributing to a given candidate may be viewed differently depending on the political affiliation of the audience. Unlike studies of voter turnout, where the information on the individual's partisan affiliation was not publicized, the dissemination of campaign contributions records reveals an individual's political preferences and the strength of her affiliation. As a result, making a higher contribution to a Democratic presidential candidate may generate positive reactions from Democratic neighbors and neg-

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<sup>3</sup>Other field experiments that measured the role of social pressure include DellaVigna et al. (2012) with door-to-door fundraising, Chetty et al. (2012) with peer review at an academic journal and DellaVigna et al. (2013) with voter turnout.

<sup>4</sup>A number of studies have extended this analysis. For an overview of this literature, see Green and Gerber (2010).



ative reactions from Republican neighbors. Indeed, there is some evidence suggesting that individual care about revealing or concealing their party affiliation to others: some individuals proactively publicize their affiliation with public statements, such as signs in their yards (Makse and Sokhey, 2014), yet others do not want their political affiliation to be known (Gerber et al., 2013).

We can highlight these factors at play with a sketch model. Suppose an individual is considering whether to make a contribution to her preferred political party. Each individual belongs to one reference group, and interacts with other members of the same group, which we refer to as neighbors. In these interactions, some characteristics of the individual are not perfectly observable to her neighbors, such as her affluence, her disposition towards pro-social behavior, her party affiliation and its strength, among many others. The interaction may be more or less beneficial for the individual depending on what the neighbor perceives about her characteristics. For instance, neighbors may have a kinder or more positive attitude toward individuals that are affluent, that exhibit civically-responsible behavior, or that share the neighbor’s political preferences. While these characteristics are not directly observable to neighbors, contributions can be observed with certain probability, for instance because neighbors may look up the individual’s contribution activity using the FEC website’s search tool. Making contributions more visible to neighbors, or changing the perception of how visible these are, should thus affect the individual’s desired contribution level, and this effect should depend on what the neighbor learns from the observed contribution. If contributions signal mostly income or wealth and being perceived as affluent is considered a positive trait, then more visibility should increase contributions. Similarly, if contributions signal pro-social attitudes then more visibility should also increase contributions.

However, there are many other more efficient ways to signal affluence and pro-social behavior, for instance buying an expensive car or making named contributions to local charities (e.g., Perez-Truglia, 2013). It is thus unlikely that individuals use campaign contributions primarily to signal those traits. Campaign contributions are most likely used as signals of party identification and strength of that identification. Unlike the case of affluence and pro-social behavior, which are usually considered overall positive traits in social interactions, a signal of a stronger affiliation with a given party will result in better attitudes by neighbors of the same party, but in worse treatment by individuals of the opposite party. The signal operates in a partisan environment, and thus its effects will be shaped by the political composition of the reference group. Intuitively, we would expect that increasing the visibility of contributions would raise an individual’s desired contribution if most neighbors support the individual’s same party, but decrease her desired contributions if most neighbors identify with the opposite party. This is the basic intuition underlying the formal signaling model

presented in Appendix I. This model shows that the effect of an increase in visibility of contributions on an individual's contribution is increasing in the share of neighbors that sympathize with the individual's own party. The model also illustrates that this prediction is robust to the introduction of homophily (i.e., a higher likelihood of interacting with more like-minded neighbors) and unaffected by equilibrium effects.<sup>5</sup> This model is an application of a more general principle usually denominated as conformity (Bernheim, 1994).

A second type of social incentives is given by the individual's reaction to the behavior of others, rather than being observed by others, which we denominate informational effects. There is evidence from the literature on charitable giving that an individual's donations change when she receives information about others' contributions. For example, Frey and Meier (2004) implemented a field experiment in which they sent information about past donations from fellow students to a randomly-assigned sample of potential donors to a university's social fund. Those who were told that a high percentage of students had donated in the past gave more than those who were told that only a small fraction contributed.<sup>6</sup> This type of findings can be interpreted as the tendency of individuals to make contributions following a social norm about the right amount that one should contribute, and that, in turn, individuals form beliefs about that social norm based on the observation of contributions made by others in their reference group.<sup>7</sup>

Social norms in charitable giving imply that one's contributions could be affected by donations by other individuals in general. In the partisan context of campaign contributions, we could expect that one's behavior would be influenced primarily by that of others in one's reference group. An individual's desired contributions, thus, should increase according to the perceived average amount contributed to her own party. It may also increase, to a lesser extent, according to the perceived average amount contributed to her opposite party, if the individual - beyond partisan considerations - uses the behavior of all individuals to shape beliefs about social norms. Besides the average amount contributed by others, the social norm may also make an individual's contribution sensitive to the number of individuals contributing to each party. For example, a higher participation by individuals of the same party may prompt the individual to participate more herself. To distinguish between these poten-

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<sup>5</sup>The irrelevance of equilibrium effects arises from the assumption that contributors are a relatively small share of the population. For example, Opensecrets.org estimates that only about 0.53% of the U.S. adult population gave over \$200 in campaign contributions during the 2012 election cycle.

<sup>6</sup>Similar findings were reported by Martin and Randal (2008) with contributions to an art gallery and by Allcott (2011) in the context of energy saving measures.

<sup>7</sup>Another interpretation is that charitable donations by others may signal the quality of the charitable organization (Karlan and List, 2012). A priori, this explanation is not likely to apply to the case of presidential campaign contributions. Most contributors have beliefs about the candidates, and while these may be either biased or unbiased, they are probably unlikely to be significantly affected by the observation of the contributions made by others.

tially simultaneous effects, we need to generate variation not only on the average amounts contributed but also on the number of contributors to each party.

Besides social norms, information about the contributions of others can affect an individual’s own contribution through other channels. For example, the investment motive refers to the notion that individuals contribute because of the perception that, with some probability, her marginal contribution will change the election outcome (Ansolabehere et al., 2003). This probability is similar to the probability of being a pivotal voter in a model of voter turnout (e.g., Dhillon and Peralya, 2002). A rational individual should expect the marginal effect of a single contribution (or that of a single vote) to be virtually zero,<sup>8</sup> but contributions from some individuals may still be induced by this investment motive. This implies that individuals should be more likely to contribute when campaign contributions are perceived to be more even (i.e., both parties raised similar amounts). This may in turn generate correlations that could be spuriously attributed to social norms. The identification challenge is that social norms may be confounded by several other competing mechanisms. Our experimental setting was devised to test the hypothesis of social norms while at the same time controlling for alternative channels.

To the best of our knowledge, the only existing experimental evidence related to informational effects in campaign contributions was provided by Augenblick and Cunha (2011). They compared contributions by potential Democratic donors who received a postcard from a Democratic candidate with a picture of the candidate and a message. They find that the messages both the messages “*Small Republican contributions have been averaging \$28*” and “*Small Democratic contributions have been averaging \$28*” elicited higher contributions than a message that did not mention others’ contributions, which they interpret as a result of a combination of social norms and competition. Our experimental setting attempts to disentangle these effects.

## 2.2 Experimental Design

### 2.2.1 Context of the Field Experiment

Our field experiment is based on the records of individual contributions published by the FEC. We selected contributors who had made a contribution to a presidential campaign between April 1, 2011 and April 1, 2012 to form our subject pool. We sent letters with individualized information related to campaign contributions to a random sample of individuals in this subject pool, with the remaining constituting the experiment’s control group. Finally, using

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<sup>8</sup>This argument is particularly strong in the context of to a presidential campaign, where the average individual contribution of a few hundred dollars is infinitesimal when compared to the several hundreds of millions of dollars raised by each candidate.

the same FEC records, we measured the effect of the information contained in the mailings on the subsequent contribution behavior of the recipients in the period between the receipt of the letter and the end of the election cycle. Figure 1 provides a timeline of the intervention - this and other implementation details are discussed at length in Section 3.

Our field experiment consisted of three categories of letters, Website, List and Placebo, with subcategories of treatments within each letter type. These letters had a common format and heading. Besides these common characteristics, each type of letter contained different information and was designed to test specific mechanisms through which social incentives operate. The key tests for the presence of social incentives do not rely on comparison of post-treatment outcomes between individuals who were sent a given type of letter and those in the control group who received no letter. Instead, the research design relies on the comparison of outcomes among recipients of a given letter type, but with randomly assigned subtle variations in the information contained in the letters.

### 2.2.2 Identification of Signaling Effects

The purpose of the Website letters was to generate exogenous individual-level variation in the belief that the recipient’s contributions could be observed by others, while holding everything else constant. A post-election survey (described in Section 3.4 below) provides some insights about contributors’ knowledge of the FEC disclosure policy that are valuable for understanding the experimental design. Figure 2.a shows that, among subjects in the control group, a large majority of respondents (86%) agreed with the statement that contribution records were a matter of public information, while the remaining 14% reported that they believed that the contribution records were confidential.<sup>9</sup> Moreover, when asked about how confident they were about their response, only 11% of those who said that contribution information was public stated that they were unsure about their answers. The results from this survey also indicate that knowledge about the disclosure policy is even higher than knowledge about contribution limits, another important aspect of campaign finance regulations.<sup>10</sup> Even though at the time of the experiment we had no survey evidence, we expected such high awareness about the publicity of contribution records. Campaign committees must request detailed individual information from contributors, for which they must explain that this information is required by the FEC.

The fact that a vast majority of contributors already knew about the public nature of contribution records implied that sending a contributor information about the FEC disclo-

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<sup>9</sup>See Appendix C for the exact wording of this question and of the response options.

<sup>10</sup>When asked about contribution limits, 70% of respondents selected the correct amount (\$2,500) from a list, while 20% selected the *Don’t Know* option (see Figure C.1 in the Appendix).

sure policy should have had a limited effect on subsequent contributions. Results from our post-election survey, however, suggest that there was significant potential in informing the contributor’s neighbors about the publicity and the ease of access of individual contributions: Figure 2.b indicates that 40% of respondents considered that the majority or the vast majority of their neighbors believed that the contribution records were confidential. This probably reflects the fact that contributors believe that only some of their neighbors are contributors, and that non-contributors are not as aware about the public nature of these records. Moreover, it should be stressed that our Website letters contained a link to the FEC’s website search tool: recipients could know that the records were public (or think that they neighbors knew about this), but they may have not known (or thought that they neighbors did not know) that they were so easily accessible.

We introduced exogenous variation in the visibility of the recipient’s contributions by sending two variations of the Website letter: Website-Self and Website-Neighbors. We tested the presence of signaling effects by comparing the impact of these two subcategories of the Website treatment. Samples of these types of letters are shown in Appendices A.1 and A.2. The two types of letter differed in one crucial dimension, but were identical in all other respects. Both included a table with a list of the individual and five neighbors who contributed between April 1, 2011 and April 1, 2012, with the party and the amount given by each of those listed. The five neighboring contributors shown in the list correspond to those who are nearest to the recipient, a relatively short distance on average.<sup>11</sup> The recipient of the letter was always the second contributor in the list, highlighted and identified by full name - other contributors were only identified by their last name initial and their first name. We included this short list of contributors and contributions as a way to draw the recipient’s attention to the content of the letter, but also, by providing verifiable information (including their own contributions to date), the list should have reinforced the perception that individual contributions were public. The second paragraph of the letter identified the FEC as the data source and indicated that the name, address and other details about contributions were freely accessible online, along with a link to the FEC’s website search tool and an indication that one could use this website “to see which candidates or political parties your neighbors, friends, family and co-workers are contributing to.”

The only difference between Website-Self and Website-Neighbors was in the message prominently displayed in a box located right below the list of contributors, which read:

**Website-Self:** “Your household was the only household randomly chosen from your area to receive a letter of this type.”

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<sup>11</sup>The median pairwise distance between the recipients and those five neighbors was 0.35 miles.

**Website-Neighbors:** “Your household and other households in your area were randomly chosen to receive a letter of this type.”

The two sub-treatments thus differed in the information provided to the recipient about whether other households in their area would receive a similar letter or not. Moreover, this information was non-deceptive: we sent only one Website-Self letter and multiple Website-Neighbors letters in the corresponding geographic areas.<sup>12</sup> Note that households were explicitly told that they were randomly selected, so that they would not make any inferences from having been chosen to receive the letter.

Receiving a Website letter may affect the recipient’s contributions in a number of ways. For example, the individual may learn about the contribution patterns of others in her area, or she may use the link provided in the letter to access the FEC website’s search tool and look up contributions by neighbors, friends or celebrities. Since the two types of letters were identical in every other respect, any difference in reactions to the Website-Neighbor and the Website-Self treatments must be due to the fact that a recipient of a Website-Neighbors was more likely to believe that her neighbors would use the FEC website’s search tool to find out about her contributions. In other words, the only difference between Website-Neighbors and Website-Self is that the former makes the recipient’s contribution more visible to neighbors (or that it increases the recipient’s perception that others may view her contribution activity).

The discussion of signaling effects in the previous subsection highlighted the importance of the composition of the public potentially receiving the signal in the context of political participation and, more specifically, campaign contributions. For this reason, the empirical analysis presented in Section 4 below relies on the difference in outcomes between recipients of the Website-Neighbor and the Website-Self letters interacted with the partisan composition of the recipient’s area of residence.

### 2.2.3 Identification of Informational Effects

The purpose of the List letters was to provide information about the behavior of other contributors in the recipient’s area. A sample of the List letter is presented in Appendix A.3. This type of letter presented a list of presidential campaign contributions made by the recipient and nine other individuals from the recipient’s area during the period April 1,

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<sup>12</sup>Specifically, we divided the United States into disjointed geographical areas of similar population based on agglomerations of 9-digit ZIP codes. These areas were randomly assigned to one of two groups. In areas assigned to the Website-Self treatment, exactly one household (randomly selected among all household in the area) was sent a letter of this type. In the areas assigned to the Website-Neighbors treatment, we randomly selected two households to be sent these letters. We assigned twice as many areas to the Website-Self than to the Website-Neighbors type so that the expected number of households receiving each sub-treatment was the same.

2011 to April 1, 2012, with the party and the amount contributed by each of those listed. A key difference between the Website letter and the List letter is that in the latter there was no mention of the FEC website’s online search tool.<sup>13</sup> The recipient’s own contribution and her full name were included (and highlighted) at the top of the list. This was meant to draw the recipient’s attention and to highlight the credibility of the study, given that the individual was probably aware of her own past contributions, and could recognize the information to be true. As in the Website letters, other contributors were only identified by their last name initial and their first name. The letter listed contributions to Democrats first and to Republicans next, with each entry within these groups ordered from highest to lowest amount. This sorting was meant to make it easier to read the information on the table.

The list contained in this type of letters was the main component of the informational treatment. This list contained subtle random variations in the information included: some had more Republican contributors, some had higher amounts for Democrats, and so on. We devised an algorithm to select the other contributors that would be displayed in the list in the recipients’ letters in a way that allowed us to induce non-deceptive variation. The letters themselves were not deceptive because the lists displayed a subset of the 30 contributors closest to the recipient, which always satisfied the included statement that the list was composed of “10 individuals from your neighborhood.” The algorithm was constructed as follows. We first obtained the geo-location for all individuals listed in our baseline FEC database. Then, for each individual  $i$  in the List treatment group, we identified the 30 closest contributors,  $L_i$ , which according to our definition are the individual’s neighbors.<sup>14</sup> Each list of ten contributors in the letters included the recipient and a subset of nine neighbors from  $L_i$ . These nine neighbors were selected by first ordering the list of 30 neighboring contributors according to a composite index, and then selecting the top nine contributors from the ordered list. The value of the composite index for a given neighbor  $j$  was a function of  $j$ ’s party,  $Party(j)$ , and of the amount contributed by  $j$  during the preceeding twelve-month period,  $Amount(j)$ :

$$Index_i(j) = \theta_i^D \cdot 1 [Party(j) = DEM] + \theta_i^A \cdot Amount(j)$$

The parameters  $\{\theta_i^D, \theta_i^A\}$  are the recipient-specific weights assigned to each of those dimensions. Taking those parameters as a given, the list of the top nine neighbors is denoted

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<sup>13</sup>The information provided by the List treatment is probably not the only information about contributions that recipients would obtain. The letter might have prompted recipients to find out more about their neighbors’ contributions, by visiting the FEC website (or other sites providing similar information) even if we did not include a link to this search tool. The possibility that some individuals acquired information on their own implies an under-estimation the real effect of the information provided by our treatment.

<sup>14</sup> $L_i$  is constructed based on pair-wise distances as the crow flies. The median pairwise distance between the recipients and their 30 closest contributors was about one mile. These neighboring contributors were selected from the full sample from FEC records, not only from our selected subject pool.

by  $g(L_i; \theta_i^D, \theta_i^A)$ . The weight given to the political party component was randomly selected from three possible values:  $\theta_i^D = -c_p, 0, c_p$ , with  $c_p > 0$ . Similarly, the weight given to the contribution amount was randomly selected from three possible values:  $\theta_i^A = -c_a, 0, c_a$ , with  $c_a > 0$ . The list of nine neighbors given by the parameter values  $\theta_i^D = 0$  and  $\theta_i^A = 0$  was defined as the baseline list:  $g(L_i; 0, 0)$ .

This composite index induced exogenous variation in the list printed on each letter. Table 6 presents three possible lists of nine neighbors generated by different combinations of the weights. The left panel presents the baseline list of the nine nearest neighboring contributors. The center panel presents the list obtained when  $\theta_i^D$  takes the negative value. Compared to the baseline list (left panel), some of the Democratic neighbors leave the list and are replaced by Republican neighbors. The right panel, in turn, presents the case in which  $\theta_i^A$  takes a positive value. Again compared to the baseline list, some Republican contributors are replaced by other Republicans with higher contributions (the same would be the case for Democrats). We calibrated the probability distribution of the parameter values so that the experimental variation in the key dimensions of the list, the average amounts contributed, and the number of contributors to each party, were orthogonal. Most importantly, we calibrated the distribution of the parameters so that the lists were not biased on an average sense.

We do not rely on comparisons between post-treatment contributions by individuals who received the List letters and those who did not receive any letters. Instead, we rely on the exogenous differences in the information contained in the lists mailed to different individuals. The intuition of the estimation of these effects can be conveyed by an example. Suppose that we only varied one dimension - some individuals were mailed the lists of their closest neighboring contributors with average contributions \$500, while others received lists with higher average contributions of \$600. The estimate is obtained from a regression of the post-treatment contribution on a variable that reflects the difference between the baseline list and the one with the exogenous variation in the amount shown - in this case, this variable takes the value 0 for the recipients shown the \$500 average contribution and the value 100 for the recipients shown the \$600 average contribution. If the estimated coefficient on that variable is 0.1, it would imply that each additional dollar in average contributions shown by the letter caused the recipient to contribute an additional ten cents. We can generalize this intuition for the case when we simultaneously randomize multiple dimensions of the information set. Let  $f^k(\cdot)$  represent any statistic  $k$  from a given list (e.g., the mean of contributions in the list to the recipient's own party). For our analysis, we regress the recipient's post-treatment contributions (or probability of contributing) on  $\Delta f_i^j = f^j(g(L_i; \theta_i^D, \theta_i^A)) - f(g(L_i; 0, 0))$ :

$$Y_i = \sum_j \beta_j \cdot \Delta f_i^j + \delta X_i + \varepsilon_i \quad (1)$$



The term  $\Delta f_i^j$  is the value of the particular statistic in the list shown to the individual compared to the value that would have been shown if we had sent her the baseline list instead. Since the variation in  $\Delta f_i^j$  is driven entirely by the random assignment of  $\{\theta_i^D, \theta_i^A\}$ , the coefficient on  $\Delta f_i^j$  can be interpreted as the causal effect of the  $f_i^j$  shown in the list on the recipient’s post-treatment contributions.

A further objective of this treatment arm was to establish whether any potential informational effects could be confounded or, on the contrary, augmented by signaling effects. For example, informing an individual that a higher share of her neighbors support her own party may increase her contributions, but that finding can be rationalized in terms of the signaling model. To minimize the potential of these confounding effects, we designed the List letters so that the recipients did not feel exposed to their neighbors. We omitted any reference to the FEC’s website search tool, and we included in the mailed lists only the last name initials of contributors from a broader geographical area than in the Website mailings. Moreover, a feature of these letters was designed to specifically test for the presence of signaling effects. We randomly assigned subjects to one of two variations of the List treatment: List-Once (illustrated in Appendix A.3) and List-Update (Appendix A.4). The only difference between these two variations was that in the List-Update letter we stated that an updated list with contributions by neighbors may be sent at the end of the election cycle, whereas the List-Once letters specified that a letter of this type would not be sent again in the future. If the effect of the list letters worked through the visibility of the recipient’s contribution, we should see large differences between these two sub-treatments.

A related question is whether the dissemination of information about itemized contribution records had a systematic effect on contributions. For example, from the perspective of designing a disclosure policy, the effect of this information may be evidence that individuals are learning new relevant information from the contribution behavior of others (e.g., contribution norm). The average effect of being sent a the List letter, relative to the Control group, may be suggestive of the effect of disseminating unbiased information about contribution records. However, the effect of receiving a letter with itemized contribution records of other people in the area may respond to a variety of reasons unrelated to the contribution records per se. For instance, simply receiving a letter about campaign contributions may remind individuals about their commitment to contribute, and this could have a positive effect on future contributions independently of the information provided in the body of the letter. Alternatively, receiving a letter from a research team may have an effect of its own, for example by making the individual think that campaign contributions are more important than previously thought. In anticipation of these potential concerns, we also devised a Placebo letter as a treatment arm. The outside of the mailpiece, the inside header and footer, and

the paragraph explaining the general purpose of our mailing were identical to those of the other two treatments. However, instead of providing any meaningful information about the disclosure policy on individual campaign contributions (as in the Website letters), or about the recipient’s neighbors contributions (as in the List letters), the Placebo letter contained standard regulatory information about contribution limits, taken verbatim from the FEC’s regulations. We did not expect this information to have an effect on contributions, because these regulations are generally well known,<sup>15</sup> and, most importantly, because contribution limits were not binding for virtually all of the individuals in our subject pool. If the Placebo letters generated any effect, it would suggest that part of the differences between the Website and List treatments and the Control group could be the result of some of the confounding factors mentioned above.

#### **2.2.4 Distinguishing Between the Effect of Receiving a Letter and the Effect of the Information Contained in the Letter**

The letters from our experiment were sent during the presidential campaign, when most of our subjects probably received several other mailings related to the upcoming election. A substantial share of experimental subjects - most likely a majority - may not have read or even opened the letters we sent to them. The empirical strategies discussed so far provide estimates of the average intention to treat effect (ITT), that is, the impact of having been selected to the group that would receive a letter compared to those receiving other letters (or no letters at all for the control group). The magnitude of this ITT may be relevant in some contexts, such as for fundraisers who want to design mailing strategies to raise contributions. However, the economic question motivating our analysis is to establish the extent and magnitude of social incentives in the contexts of campaign contributions. Our experimental mailings only constitute instruments to uncover these mechanisms. The underlying economic question requires establishing the impact of the information contained in the letter, which would allow us to quantify the magnitude of the social mechanisms at play.

The effect of the information provided in the letter is the average treatment effect on the individuals who actually read the letters, and this is a multiple of the ITT effect. The Environmental Protection Agency (EPA) estimates that about 50% of unsolicited mail is discarded without even being opened. If we assume that half of the individuals who were sent a letter actually read it, the treatment effect for those who read the letter would be twice the intention to treat effect.<sup>16</sup> The EPA figure provides a conservative upper bound to

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<sup>15</sup>See for example Figure C.1.a in the Appendix.

<sup>16</sup>Note that the treatment effects for individuals who read the letter could be different than the treatment effects for individuals who did not read the letter (in the counterfactual case of reading it) if these two groups

the share of experimental subjects who read the letters. A lower bound for the proportion who read the letters and paid attention to their content can be derived from the number of unique visitors to the project’s website, which amounted to about 5% of the total number of letters we sent. These lower and upper bounds, however, are not very informative, since they imply scale-up factors for the intention to treat effects ranging from 2 to 20. Motivated by this limitation of the data, we conducted a post-election survey (described in Section 3.4 below) that, among other purposes, could provide a better estimate of this scale-up factor. These results are discussed in Section 4.3.

### 3 Data Sources and Details about the Implementation of the Field Experiment

#### 3.1 Data Sources and Definition of the Subject Pool

The starting point of our subject pool was a subsample from the FEC contribution records consisting of 280,456 unique individuals who made over \$200 in contributions to a presidential campaign committee from April 1, 2011 to April 1, 2012, as listed in the FEC database. This sample was obtained from the FEC’s public records as of April 25, 2012.<sup>17</sup> While the FEC’s records are remarkably comprehensive and complete, there were still some instances of missing or inconsistent information. Since the number of individuals in this initial sample was substantially higher than the number of subjects needed for our experiment, we took a highly conservative approach by limiting the subject pool to individuals with the highest quality of available information (e.g., quality of address information). Additionally, we applied a number of arbitrary criteria, such as excluding contributors from Washington D.C. and excluding individuals geographically isolated from other contributors. Appendix G provides further details about the criteria used to select the subject pool.

After applying these criteria, our final subject pool included 191,832 individuals.<sup>18</sup> Table

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of individuals differ in key characteristics (e.g., if those prone to reading unsolicited mail are more sensitive to social incentives).

<sup>17</sup>For the sake of simplicity, we only consider direct individual contributions to presidential committees, which excludes other types of contributions, such as a loan to a candidate. Even though we did not include contributions to other committees (e.g., PACs, SuperPACs) when forming the subject pool, we do analyze effects on those other types of contributions in the robustness checks of our main results.

<sup>18</sup>This sample also excludes 1,002 individuals who were sent letters but that were later on deemed undeliverable or redirected by USPS. The results are robust to alternative treatments of those observations. We took several measures to clean the address information from the FEC database, including geocoding, cross-checking information across different records for the same individual, and matching the data to the USPS National Change of Address database. Our mailing provider indicated that even carefully cleaned databases of addresses usually result in about 5% of undeliverable letters, so in this respect our efforts were successful.

1 presents summary statistics of individual characteristics from our experimental sample of earlier contributors (first column) compared to all contributors to presidential campaigns from the 2012 election cycle (second column) and to the general U.S. population (third column). The comparison between the first two columns indicates that the average contributor in our sample was fairly representative of all contributors in the 2012 presidential election insofar as they exhibit similar socio-economic characteristics, including racial composition and income. There are, however, some differences in contribution patterns between the two groups. Our subject pool contains a lower share of contributors to the Obama campaign. This is due to the fact that our subjects were early contributors and, because of the Republican primary, Republican candidates started their campaigns earlier. Our subject pool also has higher average contributions, which can be partly attributed to the fact that Republicans, who are over-represented in the experimental sample, contributed higher amounts. Finally, the comparison of the first two columns with the third column illustrates the well documented fact that contributors are significantly different from the average U.S. citizen in several ways: e.g., contributors are more likely to be males, white and more likely to live in urban and wealthier areas.

Out of the 191,832 individuals in the subject pool, 99,834 were assigned to the control group and were not sent any letters; each of the remaining 91,998 were randomly assigned to be sent one of three types of letters: Website, List or Placebo. Within each of those treatment groups, part of the information contained in the letter was also randomly assigned. The random assignment to the control and treatment groups was conducted at the household level,<sup>19</sup> and it was stratified at the 3-digit ZIP code (ZIP-3) level, except for the Website treatments, whose stratification method is described in Section 4. Table 2 presents summary statistics for a number of pre-treatment characteristics for each of the treatment types, including the amount of pre-treatment contributions and the party contributed to. As expected due to random assignment, the treatment groups are balanced in their observable characteristics. The last column reports the p-values from a test where the null hypothesis is that the means of the row variable for the six groups are equal. These tests indicate that the differences across treatments are not only very small but also statistically insignificant.

### 3.2 Timing of the Experiment and Outcomes of Interest

Figure 1 provides a timeline of the key events in both the election cycle and the implementation of our field experiment. The letters were sent on May 6, 2012. We chose this date for a number of reasons. Power calculation exercises based on contributions during

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<sup>19</sup>That is, all household members were assigned to the same treatment group. About 96% of the households in the subject pool included only one contributor.

the 2008 election cycle indicated that sending the letters in May would be optimal, primarily because the expected post-treatment probability of making contributions would be close to 50%. Moreover, the Republican National Committee declared Mitt Romney the party’s presumptive nominee a week earlier on May 2, 2012. Sending the letters when each party had a single presidential candidate simplified the outcome variable as we would not have to compare contributions from the same individual to different candidates. The outcomes of interest throughout our study (unless stated otherwise) are the individual campaign contributions made to the Obama or Romney committees from the time of delivery of our letter (as indicated by USPS records) to the end of the election cycle, December 31, 2012. We label these contributions from individuals in our subject pool “post-treatment.” The pre-treatment amounts, which we employ for the purpose of falsification tests, correspond to the total contributions made from April 1, 2011, to the date when the letters were delivered.<sup>20</sup>

Table 3 describes the contribution patterns before and after treatment for individuals in our subject pool. The top panel presents detailed statistics for the pre-treatment period, during which 100,541 individuals (52% of the subject pool) contributed to Obama, 24,910 (13%) to Romney, and 66,381 (35%) to other Republican candidates. On average, individuals contributed \$523. The average amount contributed pre-treatment was higher for Republican than for Democratic contributors (again, this was expected given that the Republican candidates had a primary election campaign). The bottom half of Table 3 presents similar statistics for post-treatment contributions. In the post-treatment period, 49% of our sample of earlier contributors made at least one contribution to either Obama or Romney, and for those making at least one post-treatment contribution, the average amount was about \$589, only slightly higher for Republican (\$610) than for Democrats (\$567).<sup>21</sup> There were, however, substantial partisan differences in the likelihood of making a post-treatment contribution: while 76% of pre-treatment Obama contributors made at least one post-treatment contribution, only 38% of Romney supporters and 12% of contributor to other Republican candidates did the same. In order to account for these differences, we include measures of the pre-treatment contributions as control variables in the regression analysis. In practice,

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<sup>20</sup>See Appendix G for details about how we define the date of delivery for each individual according to USPS records. For individuals in the control group, the date dividing pre- and post-treatment contributions corresponds to the median date when the letters were delivered in their 5-digit ZIP code. Our main results are largely unchanged if we instead define pre- and post- contributions using the date when the letters were mailed.

<sup>21</sup>When the dependent variable is the amount contributed, we use an interval regression model to take into account the censored nature of the outcome. Note that if a Republican did not make a pre-treatment contribution to Romney, the fact that she appears as not making a post-treatment contribution to Romney is consistent with having made a contribution to Romney below \$200. We use the interval regression model instead of a simple censored regression model to take that information into account. In practice, the interval regression model and the censored model yield very similar results.

the results are similar if we do not include these control variables.

### 3.3 Details about the Mailpieces

All the letters from the three treatment types (Website, List and Placebo) shared basic characteristics. Appendices A.1-A.5 show samples of the letters for different treatment types and sub-types. The mailpiece consisted of a single sheet of paper that folded and sealed to make letter-sized mailpiece, like a flyer (see sample in Appendix A.6). All letters included the same header (“Boston, April 25th 2012”) and the same last paragraph: “This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. You can find more information about this project, including contact information, on our website.” The letters included the web address of the project’s website, shown in Appendix B, which provided basic information about the research project, and contact information to reach the research team and the University’s Institutional Review Board. The main purpose of the website was to provide contextual information about our study to interested subjects, and to dissipate any doubts about its legitimacy, emphasizing its academic and non-partisan nature. Although the website provided some general information about the main research objective, to avoid the contamination of the experimental results, it did not provide any details about the precise hypotheses to be tested, nor about the existence of several different treatment types.<sup>22</sup>

### 3.4 Post-Election Mail-In Survey

Data for the key outcome variable, the recipient’s post-treatment contributions, was obtained from the FEC administrative records. Additionally, we collected complementary information from a subsample of our subjects by means of a mail-in survey sent after the election (so it would not interfere with the experimental results). Our first purpose for conducting the survey was to complement the experimental evidence with an observational analysis of information not available through administrative sources. For instance, the survey was designed to provide insights on the degree of knowledge about the FEC disclosure policy. The second objective was to exploit the survey data in combination with the experimental results to scale-up the intention to treat effects into treatment effects on the treated, as discussed in Section 2.2.2.

The survey was sent by mail on December 6, 2012, one month after the 2012 presidential

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<sup>22</sup>We directed individuals who were interested in receiving a debriefing brochure (a non-technical summary of the study’s main hypotheses and results) to send an email to an email address. We sent the brochure only after the data collection process was completed.

election day. The intended recipients, 34,966 in total, were a random sample of individuals from the Control group (one third) and from the Website treatment group (two thirds). The envelope contained a letter and the survey on two sheets of paper, and a smaller prepaid business-reply envelope. The recipient was asked to fill out the the survey and mail it back in the provided envelope by dropping it in a USPS mailbox. As an incentive for participation, the letter informed recipients that there were lottery prizes for individuals who responded and mailed back the survey before January 31, 2013. The survey instrument included questions on gender, age, five questions about knowledge of campaign finance law, and a final subjective question about how much an individual should contribute to presidential campaigns. We received 9,414 responses, which implies a response rate of 21.21%. Appendix C presents the survey instrument and discusses further details about this survey and its response rate.

## 4 Empirical Results: Signaling Effects

### 4.1 Main Results

The hypothesis that signaling effects are one of the social incentives driving contributions implies that these contributions are used as signals of party and strength of affiliation. Crucially, these signals operate in a partisan context, and thus the effect of increasing the visibility of an individual’s contributions should depend on the political composition of the individual’s reference group, comprised by other people that the individual is likely to interact with, including but not limited to family members, friends, neighbors and co-workers. While ideally we would use information on social interactions to define the individual’s reference group, since such data is not available we had to rely instead on a geographic proxy. This is one of the most widely-used approaches in the literature on social interactions.<sup>23</sup> We defined groups by the individual’s 3-digit ZIP code of residence. The political composition of each ZIP-3 was computed as the share of contributors to the individual’s party over the three previous presidential campaigns. This same measure of reference groups is used throughout all the experimental results, although we also discuss robustness checks using alternative definitions of the political composition of the area.<sup>24</sup>

The results are derived from regressions of individual post-treatment contributions on the difference between the effects of the Website-Neighbors and Website-Self interacted with

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<sup>23</sup>For example, for a discussion on the use geographic proxies for reference groups in the context of signaling see Perez-Truglia (2013).

<sup>24</sup>The advantage of using contribution data instead of electoral results is that we have the exact location for each contributor, which allows a finer measure of geographic party composition. Electoral results are only available at the county level. In any case, our party composition variable, based on share of contributors, and an alternative, based on electoral results, have a very high linear correlation of 0.88 at the county level.

the share of own-party contributors in the ZIP-3. The panels in Figure 3 present the results from different specifications in graphical form. The main results on signaling effects are given by Figure 3.a, which shows the differential effect between Website-Neighbors and Website-Self on the amount contributed post-treatment, for different values of the share of own-party contributors in the ZIP-3. As previously mentioned, the difference between Website-Neighbors and Website-Self captures the effect of increasing the visibility of the recipient’s contributions (i.e., the signaling effect). Consistent with the prediction from the model of party signaling, this effect is increasing in the share of own-party individuals in the ZIP-3, as manifested in the positive slope of the regression line in Figure 3.a. Moreover, the effects at the two extremes of the share of own-party indicates that the increase in the visibility of contributions captured by the difference between Website-Neighbors and Website-Self has positive and significant effects when the majority of neighbors support the individual’s own party, and negative effects when the majority of neighbors support the opposite party. These results can be interpreted as the difference in post-treatment contributions between two individuals of the same affiliation living in the same area, one of which received a Website-Neighbors and the other a Website-Self letter. Alternatively, they can be interpreted as the difference in contributions in the counterfactual case of reassigning a recipient from a Website-Self to a Website-Neighbors letter. Figure 3.a suggests that the signaling effect reduced post-treatment contributions by about \$65 in areas where the individual’s own party represented only 20% of contributors, while the signaling effect increased contributions \$25 in areas where the own party represented 80% of contributors. These effects are not only statistically significant but also economically significant relative to the average post-treatment contributions (\$589), specially since these are intention to treat effects (we discuss the scaling-up of these results in Section 4.3 below).

Figure 3.b presents similar estimates with the probability to make a post-treatment contribution (instead of the amount) as the outcome of interest (i.e., the extensive margin). The dependent variable is a dummy variable that takes the value 100 if the individual made at least one contribution in the post-treatment period. Again consistent with the prediction from the model of party signaling and with the results from the previous panel, the signaling effect is increasing in the share of own-party individuals in the ZIP-3, as manifested in the positive slope of the regression line in Figure 3.b. In terms of the intuition discussed above, the counterfactual of reassigning a recipient from a Website-Self to a Website-Neighbors letter would reduce her probability to make a post-treatment contribution by about 2.1% in areas where the recipient’s own party represented only 20% of contributors, while this signaling effect would increase her probability of contributing by about 1.7% in areas where her own party represented 80% of contributors. These are non-neglibible effects when compared



to the baseline probability of making post-treatment contributions of 48.9% in the control group. As analyzed in Section 4.3 below, the magnitude of the effects is substantially larger once we take into account that the majority of the letters were not read by the recipients.

Note from Figures 3.a and 3.b that the point at which the signaling effects become zero is approximately where the share of own-party neighbors is 0.5. This finding is consistent with the model of party-signaling in Appendix I, in which agents are equally likely to interact with individuals of the own or of the opposite party, and where they value those interactions symmetrically.<sup>25</sup> Moreover, it should be stressed that all of our experimental subjects, by definition, had already made contributions above \$200 to a presidential campaign, and they already appeared on the public FEC records. The fact that our letters had an effect on subsequent contributions implies that individuals consider that others will take into account the intensive margin of their contributions.

One alternative mechanism that could cast some doubts on our interpretation of these results as signaling effects is leading-by-example. According to this conjecture, individuals may give more if they feel observed because they believe that others will follow their lead by contributing more. This is unlikely to be driving our results for at least two reasons. First, an individual's contribution is insignificant with respect to total contributions, so the marginal effect of one contribution on the social norm is practically zero. Second, leading-by-example predicts that the impact of increasing visibility should be zero when the share of own party individuals is zero, and it should increase as this share increases. On the contrary, the evidence in Figures 3.a and 3.b indicates that increasing visibility actually decreases contributions when the share of own party individuals in the recipient's area is zero. Therefore, leading-by-example could - at most - explain only part of the estimated effects.

While we stressed that the interpretation of our field experiment relied in the comparison of subtle variations of our treatments, for completeness Figure 3.c depicts the difference in post-treatment contributions between the Website-Neighbors treatment and the control group, while Figure 3.d shows the differences between Website-Self and the control group. The comparison between these two figures illustrates the very different reactions induced by the Website sub-treatments. The effects of the two sub-types of Website letters are completely different: while Website-Neighbors decreased contributions for individuals surrounded by a majority of neighbors of the opposite party, Website-Self increased contributions for in-

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<sup>25</sup>However, it is possible that there is an asymmetry in party-signaling effect that is compounded with other types of signaling effects and, thus, is not captured. For example, if contributions constitute signals of other desirable individual characteristics beyond party affiliation (e.g., being wealthy, or having more pro-social attitudes), this would imply that the true signaling effects are more negative for every level of the share of own-party individuals.

dividuals in the same situation.

Since a vast majority of contributors already knew about the disclosure policy (Figure 2) and the letter clearly stated that neighbors would not receive similar letters, the difference between Website-Self and the control group is most likely not due to signaling. The difference between these two groups can be attributed to several different channels. For example, it may due in part to the information contained in the table of contributors included in the letter. Indeed, the greater average contributions for those in the Website-Self group relative to the control group is consistent with the finding from Section 5 that disseminating unbiased information about contribution records increases contributions. The fact that this effect is smaller in areas with a majority of same-party individuals is also consistent with the evidence on free-riding effects reported in Section 5.

Figure 3 also presents two falsification tests. Figure 3.e depicts the same estimates of signaling effects as in Figure 3.a (i.e., the difference between Website-Neighbors and Website-Self interacted with the political composition of the recipient’s area) using pre-treatment contributions, instead of post-treatment contributions, as the dependent variable. As expected from the balanced nature of our random assignment process, the regression line is nearly flat. Figure 3.f presents the effects of the Placebo letter interacted with the political composition of the recipient’s area on the post-treatment amount contributed. Any significant effect of the Placebo letters on recipient’s contributions or any significant difference in this effect as a function of the political composition of the recipient’s area would indicate that our mailings had direct effects on contributor’s behavior, regardless of the information contained in the letter. The treatment effects depicted in Figure 3.f indicate that this was not the case: the Placebo letters had no average or heterogeneous effects on contributions.

The results presented so far indicate an effect on both the intensive and extensive margins of contributions (i.e., on amounts contributed post treatment but also on the probability of making contributions in this period). Most recipients of our mailing appeared in the FEC public records before receiving our letter (or would appear soon afterward),<sup>26</sup> which implies that their political affiliation was already a matter of public record before our field experiment. The presence of signaling effects discussed in this section is thus all the more remarkable, since the experiment could not modify the basic fact that the subject’s party affiliation constituted public information. The presence of these effects can be interpreted in part as evidence that individuals care about the perceived strength of their affiliation revealed by a marginal contribution. If anything, then, this implies that the results presented

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<sup>26</sup>There is a lag between the publication of the itemized contribution information disseminated by the FEC on a monthly basis during the election cycle and the appearance of those contributions in the FEC’s online searchable database. Since we based our experiment on the published records, it is possible that a recipient of one of our letters would only appear on the searchable database some time after receiving the mailing.

in this section constitute an under-estimation of the effects of increasing the visibility of contributions when potential contributors have no prior public records of contributions (or of party affiliation). Moreover, in terms of the implications for disclosure policy, if the publicly available records did not exist in the first place, the impact of an increase in visibility (for instance, by establishing a public database of contributions) could be substantially higher.

## 4.2 Heterogeneous Effects and Robustness Tests

Table 4 presents the baseline results in regression form along with some additional robustness checks. Column (1) presents the results for the baseline specification depicted in Figure 3.a, where the dependent variable is the amount contributed post-treatment. The coefficient on Website-Neighbors - Website-Self, -91.2 is the difference in dollars that corresponds to the intercept in Figure 3.a: i.e., the signaling effect when the share of the recipient’s party in her area is 0. The coefficient of the interaction of this variable with the share of own-party individuals, 146.9, corresponds to the difference between the two extremes of the signaling effects (i.e., when the share of own-party individuals is 1 and 0). Column (6) corresponds to the same falsification test depicted in 3.f, where the dependent variable is the amount contributed pre-treatment. As expected, the corresponding coefficients are small and statistically insignificant. Column (2) in Table 4 uses the same baseline specification from column (1) but excluding the additional individual controls (e.g., gender, ethnicity). A comparison between the results in columns (1) and (2) indicates that the inclusion of the control variables have virtually no effect on the estimates.

The results presented so far included interactions with the political composition of the recipient’s area of residence (the ZIP-3 area). We used this as a proxy of the recipient’s reference group since we do not have information on interpersonal networks. The regression presented in column (3) of Table 4 adds to the baseline specification the interaction of the signaling effect with the average share of own-party individuals in the ZIP-3’s adjacent to the recipient’s area of residence. The coefficient of the interaction between the signaling effect and the political composition of this broader geographical area is small and not significant, while the signaling effect and the interaction with the recipient’s ZIP-3 are still significant and very similar to the baseline specification in column (1). We interpret this as evidence that the recipient’s ZIP-3 is a good proxy for her reference group, and that only immediate surroundings matter for social incentives such as signaling effects.

A potential concern with our results might be that the political composition of the area of residence may also reflect underlying differences in other observable and unobservable characteristics, and that our analysis might confound differences in political composition with these

other factors. Column (4) in Table 4 presents another variation of the baseline specification in which we added an interaction with the share of low-income households in the ZIP-3, allegedly an important dimension of variation between areas of residence. The coefficients for the main signaling effects in column (4) are practically identical to those in column (1), and the coefficient of the additional interaction is close to zero and statistically insignificant (although it must be interpreted with caution because it is imprecisely estimated). We conducted a number of other unreported robustness checks, such as including interactions with other characteristics of the individual’s area of residence, and using alternative definitions for the share of own-party individuals in the area. We did not find any substantial differences with our baseline results in these alternative specifications.

Finally, Table 5 presents results for signaling effects with alternative outcome variables and with group-level heterogeneity. Since we compare across groups or dependent variables that have different baseline probabilities, we estimated a Logit model for the probability of making a post-treatment contribution and report the signaling effects in a semi-elasticity form. We report the elasticities for two cases: recipients with a 20% share of own-party individuals in their area, and recipients with an 80% share of own-party in their area. Column (1) presents the baseline results for this specification, with the probability of making a post-treatment contribution to a presidential campaign as the dependent variable. The semi-elasticities indicate that signaling effects reduced this probability by about 6.4% when the share of own-party was 20%, while they increased the probability of contributing by 4.6% when the share of own-party was 80%.

Column (2) reports the signaling effects on the probability of making post-treatment amounts contributed to other committees as the dependent variable (e.g., campaigns for state governor, excluding presidential committees, also obtained from the FEC public records). The point estimates are very similar in magnitude to the estimates for the presidential campaigns, but the coefficients are not statistically significant. This results may be due to the low baseline level of this variable, at about 11%, compared to 49% for the presidential campaign contributions. This implies that there is less statistical power to identify the effects on this alternative outcome. In any case, we cannot reject the null that the respective coefficients in columns (1) and (2) are equal. This is consistent with our framework, since contributions to a non-presidential campaign should signal party affiliation to a similar extent than contributions to presidential campaigns.

Columns (3) and (4) from Table 5 present the signaling effects on post-treatment contributions in two periods - before September 1, 2012 (about four months after our treatment), and from September 1 to December 31, 2012, the last four months of the post-treatment period. The post-treatment contributions are roughly equally distributed for these two time

periods. The evidence suggests that the mailing elicited signaling effects mainly during the first four months after it was sent. This finding may reflect that recipients, after some months, forgot that we sent mailings to their neighbors. However, while the effects on the second half of the post-treatment period were smaller than in the first half, we cannot reject the null hypothesis that there were non-zero but smaller effects during that period.

The last six columns from Table 5 present the signaling effects for different subgroups of individuals. Note that, due to the statistical power available, group-level differences in signaling effects will be statistically significant only if very large. The point estimates in columns (5) and (6) suggest that Republicans in our sample were more sensitive to signaling effects, but the differences in coefficients are not statistically significant. Columns (7) and (8) show that there were no statistically significant differences in signaling effects between male and female contributors. Last, it is possible that people in more densely populated areas interact differently with neighbors with respect to those in less dense areas, and thus may be less (or more) sensitive to signaling effects. However, columns (9) and (10) show that there are no statistically significant differences in signaling effects between areas with above- and below-median population density.

### 4.3 Assessing the Magnitude of the Signaling Effects with Data from the Post-Election Mail-In Survey

As discussed previously, a substantial share of experimental subjects - most likely a majority - may not have read the letters we sent to them. Our estimates thus measure the intention to treat effects (ITT) of sending these letters. The treatment effect on the treated (i.e., the effect of the letter on the individuals who actually read it) is a multiple of the intention to treat effect. The key parameter mediating between these two estimates is the reading rate  $r$ , the proportion of the experimental subjects who read the letters. The intention to treat effects (ITT) can be scaled up into treatment effects on the treated (TOT) using the inverse of  $r$ :  $TOT = \frac{1}{r}ITT$ .

Our mailing was sent in the middle of the presidential campaign, when potential voters were receiving on a daily basis large amounts of unsolicited physical and electronic mailings soliciting campaign contributions and providing information about the candidates and the election. Even though we made efforts for our mailpiece to stand out, so were the candidates' campaign committees, and our letters had a high probability of being discarded unread. This scale-up factor between ITT and TOT effects played a key role in the power calculation analysis previous to our experiment. Intuitively, if we assumed a reading rate half as high as the one we selected, we would have needed to send twice as many letters for a given underlying

effect size. The EPA estimates that about half of unsolicited mailings are discarded without being read, but given the context of the campaign we required more conservative estimates. Consultations with mass-marketing experts provided estimates of reading rates ranging from 10% to 25% in these specific conditions, and we used 15% for our power calculations, which explains why our field experiment involved such an unusually large number of subjects. Since the analysis of the results would benefit greatly from an objective and precise measurement of this reading rate, we carried out a post-election mail-in survey with this goal in mind.

The results from the mail-in survey provide plausible estimates the reading rate  $r$ . Since the main information provided by our Website letters was the publicity of individual campaign contribution records, we could expect that an individual who read our letter should have a higher awareness about the public nature of these records. Figure 4 presents the distribution of beliefs about the publicity of contribution data for survey respondents from the control group and the Website treatment group. The Website letter does not seem to have modified the perception of those for respondents who reported to be very sure or somewhat sure that the contribution information is confidential. However, we can expect a larger effect for those who reported to be unsure about the public nature of contribution records. The results from Figure 4 confirm this intuition. Respondents who had received a Website letter had a lower probability (with respect to the control group) to report that they were unsure about whether contributions were confidential or public, and a substantially higher probability of reporting to be very sure that contribution records were public. This is reflected in the lower density in the two middle bars and the higher density in the rightmost bar in Figure 4 for the Website group with respect to the control group. While 15.8% of the individuals in the control group reported to be unsure about the publicity of the data, this proportion was 19.2% for those in the Website group, or 21.5% higher. This effect of the information in our treatment provides an estimate of the implicit reading rate of  $r = 0.215$  (with a 90% confidence interval between 0.146 and 0.284), and a scale-up factor of 4.6.<sup>27</sup> This estimate is within the range provided by the mass-mailing experts which we used for our power calculations.<sup>28</sup>

These estimates constitute a key input for the interpretation of the signaling results. The ITT signaling semi-elasticity (Column 1, Table 5) indicated that a counterfactual re-assignment of recipients from the Website-Self to the Website-Neighbors letters would increase the probability of making post-treatment contributions by 4.6% for those residing in

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<sup>27</sup>This estimate is based on the assumption that the effect of the Website letter was to reduce the proportion of those being unsure about their answers. Appendix D presents alternative (and less conservative) estimates of the reading rate based on the post-election mail-in survey.

<sup>28</sup>The mail-in survey's response rate, 21.2%, was relatively high, which may suggest that the reading rate for the original treatment letters was close to that figure or higher. However, the conditions of the survey mailing were very different to those of the experimental mailing and the mail-in survey envelope was less likely to be discarded as junk mail. Appendix D provides a more detailed discussion.

areas with 80% of supporters of their own party, and would reduce this probability by 6.4% for those in areas with only 20% of supporters of their own party. Since 49% of our subject pool made contributions in the post-treatment period, these ITT effects of sending letters resulted in changes in the probability of contributing of around 5% which, while not exceedingly large, are still not negligible. The scale-up factor of 4.6, however, indicates that the TOT effect of reading the letter had a larger effects of around  $4.6\% \times 4.6 = 21.16\%$  for those in areas with 80% of supporters of their own party and  $-6.4\% \times 4.6 = -29.44\%$  for those in areas with 20% of supporters of their own party.<sup>29</sup> We can qualify the results obtained with these simple estimates of the reading rate - see Appendix D. Even with a reading rate half as large (and a scale-up factor half the size) as in our estimates, the signaling effects would still imply large changes in contribution behavior.

These large scaled-up effects indicate that independently of the specific setting of our experiment, changes in the visibility of one’s contributions affect one’s contributions substantially. This in turn implies that the underlying social incentives have first order effects on contributions and, potentially, on political participation in general.

## 5 Empirical Results: Informational Effects

### 5.1 Main Results

The second arm of our field experiment was designed to capture another type of social incentive, informational effect, which refers to the tendency of an individual’s contribution to be affected by her perception of the reference group’s contribution behavior. While signaling effects are the result of feeling observed by others, informational effects are the result of observing others’ contributions.

This part of the experiment rely on the List letters. Unlike the Website letters, the List letters did not mention the FEC website’s search tool. Instead, they provided a semi-anonymized list of the names and contributions of ten contributors from the recipient’s area of residence (including the recipient). The selection of the nine neighboring contributors followed a specific random assignment mechanism that allowed us to vary the information presented to the recipient in a non deceptive way. This created non-deceptive exogenous variation along multiple dimensions: e.g., some recipients were mailed lists with a higher number of own-party contributors, while others were mailed lists with higher average contributions by opposite-party contributors, and so on. The econometric model discussed in Section 2.2.3

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<sup>29</sup>These calculations assume that the reading rate does not vary with the share of own-party neighbors. The results are very similar if we relax this assumption.

allows us to use this exogenous variation to study how the recipient reacted to information about the contribution behavior of others in their area.

Table 7 presents the baseline regression results. The independent variables are different dimensions of the information on the contributions of neighbors listed in the letter. These dimensions include the average amount contributed to the recipient’s own party ( $\bar{c}_{own}$ ) and to the opposite party ( $\bar{c}_{opp}$ ), the number of contributors in the list to the recipient’s party ( $N_{own}$ ), and the total amounts contributed by neighbors of own- and opposite- party ( $\sum c_{own}$  and  $\sum c_{opp}$ ). The coefficients on these variables are identified using only the experimental variation in those variables, as described in Section 2.2.3. For the sake of exposition, all the independent variables that refer to amounts are expressed in hundreds of dollars. The dependent variable in columns (1) through (6) is the dollar amount contributed post-treatment, and the estimates are computed by means of an interval regression model.

The specification in Column (1) includes  $\bar{c}_{own}$  and  $\bar{c}_{opp}$  as regressors. The coefficient on  $\bar{c}_{own}$  indicates that for each \$100 increase in this variable there is a statistically significant increase in the recipient’s own contributions of about \$2.5. This evidence is consistent with the social norms mechanism, which predicts that individuals will contribute more if they perceive higher average amounts contributed by those in their reference group. The coefficient on  $\bar{c}_{opp}$  indicates that, on the contrary, the recipient is not affected by the average contribution made by neighbors of the opposite party. This result suggests that, when forming social norms, individuals pay attention to the behavior of supporters of their own party, but not supporters of the opposite party.

The economic significance of these estimates of the social norms effect needs to be qualified by two considerations. First, as in the case of signaling effects, these are intention to treat estimates, since we do not know which recipients actually received and read the letter, and this implies that the actual treatment on the treated effects are a multiple of the latter. For instance, according to the calculations from subsection 4.3, the treatment effect on the treated may be 4.6 times the intention to treat effect. This would imply that for each \$100 increase in  $\bar{c}_{own}$  the recipients who actually read the letter increased their contributions by \$11.5 (i.e.,  $2.5 \times 4.6$ ), resulting in a elasticity of 0.115 between the average amount observed and the amount contributed. Second, this elasticity of 0.115 would be a reduced form elasticity between the information provided,  $\bar{c}_{own}^{provided}$ , and  $c$ , the contribution made. The structural parameter of interest is the elasticity between the perceived social norm,  $\bar{c}_{own}^{norm}$ , and  $c$ . The estimated reduced form effect,  $\frac{dc}{d\bar{c}_{own}^{provided}}$  is the multiplication of two effects:  $\frac{dc}{d\bar{c}_{own}^{provided}} = \frac{dc}{d\bar{c}_{own}^{norm}} \times \frac{d\bar{c}_{own}^{norm}}{d\bar{c}_{own}^{provided}}$ . Since  $\frac{d\bar{c}_{own}^{norm}}{d\bar{c}_{own}^{provided}}$  is most likely between 0 and 1, the structural elasticity of interest,  $\frac{dc}{d\bar{c}_{own}^{norm}}$ , is a multiple of the reduced-form elasticity reported in the table,  $\frac{dc}{d\bar{c}_{own}^{provided}}$ .

The post-election mail-in survey also provides some complementary suggestive evidence



about the importance of social norms as drivers of campaign contributions. Figure 5 shows the relationship between respondents' actual contributions (from the FEC records) and their survey response to a subjective question about how much an average-income individual should contribute to a presidential campaign. The strong positive relationship suggests that social norms are an important factor for understanding differences in contribution behavior. Furthermore, the elasticity between the actual contribution and the self-reported contribution norm is 0.11, which is remarkably close to the 0.115 elasticity reported above.

Column (2) from Table 7 presents the results from a specification that includes as regressors  $\bar{c}_{own}$  and  $\bar{c}_{opp}$  along with the number of individuals in the list who contributed to the recipient's party,  $N_{own}$ . The standard prediction from social norm theory is that a higher  $N_{own}$  should increase the recipient's contribution, insofar the individual feels pressured to do what the majority does. The negative and statistically significant coefficient of  $N_{own}$  suggests that the effect goes in the opposite direction. For each additional individual of the same party shown on the list, the recipients reduced contributions by \$6.22. This effect is comparable to the effect of a decrease of about \$225 in average contributions to the own-party contributors in the list. The effects of  $\bar{c}_{own}$  and  $N_{own}$  are quantitatively similar: a one standard deviation increase in  $N_{own}$  has an effect on the recipient's contribution almost equal to the effect of a one standard deviation decrease in  $\bar{c}_{own}$ .

It is possible that the individual cares about the number of contributors from her own party indirectly because she cares about the total amounts contributed to each party, or about the difference between the two parties. To explore this possibility, the specification reported in column (3) includes as regressors the sum of contributions to the recipient's own and opposite parties,  $\sum c_{own}$  and  $\sum c_{opp}$ , instead of  $N_{own}$ . In this specification, the marginal effect of  $\bar{c}_{own}$  is the composition of the direct effect through  $\bar{c}_{own}$  and the indirect effect through  $\sum c_{own}$ . The recipient's contribution increases with the total amount raised by the opposite-party and decreases with the total amount raised by the own-party. Since we cannot reject the hypothesis that the absolute values of the two coefficients are equal, column (4) presents another specification where the main independent variable is the difference  $\sum c_{own} - \sum c_{opp}$ . The coefficient on this variable is negative and statistically significant at the 5% level.

The recipient seems less willing to contribute when her own party is doing better (in total amounts as reported in the list) than the opposite party. One potential explanation is given by the investment motive: i.e., if the individual contributes because she thinks that she may make the pivotal contribution to win the election (or the "contribution race"), then she should be most willing to contribute when the total amounts contributed to each party are close to each other. The specifications reported in columns (5) and (6) from Table 7 test this prediction. The specification in column (5) introduces as a regressor the absolute value of the

difference between the total amounts contributed to the recipient’s own and opposite-party. The coefficient on this variable is small and not statistically significant.

A potential issue with this specification, however, is that individuals may react to the total contributions to each party at the local level because they extrapolate these local figures to the national level.<sup>30</sup> In these cases, an individual’s reaction to the information provided should vary with her location. For instance, if a recipient was expecting the Democratic party to win by a large margin in her local area, showing her that the contribution race is even in her area should make her infer that the Republican party is winning by a large margin at the national level. Column (6) presents the results of a specification with an alternative definition of the evenness of the campaign that accounts for this possibility. The dependent variable of interest is obtained using first the difference of  $\sum c_{own} - \sum c_{opp}$  between the list sent and the baseline list, and then computing the absolute value of this difference. Intuitively, this variable takes the value zero when the race in the list shown to the recipient is equally even with respect to the value in the baseline list (which proxies for the individual’s prior belief). The coefficient on this variable is also small and not statistically significant. The investment motive does not seem to be driving the effect the information we provided on subsequent campaign contributions, at least in terms of the two alternative indicators used in columns (5) and (6) of Table 7 (this result is robust to a number of alternative specifications, not reported).

Since the evidence is not compatible with the investment motive, we can test other conjectures to explain why the recipient seems less willing to contribute when her own party is doing better than the opposite party (i.e., decreasing with  $\sum c_{own} - \sum c_{opp}$ ). It is possible that individuals contribute because they think that supporting their candidate is the right thing to do, and this moral imperative would make them feel bad if they contributed less. In this case, an individual would feel less guilty about not contributing if the candidate already raised more money than the competitor (i.e., with higher  $\sum c_{own} - \sum c_{opp}$ ) because the candidate would not need the additional contribution. This would be equivalent to free-riding in a model of warm glow giving (Andreoni, 1989). Alternatively, individuals may contribute less because they perceive that their marginal contribution could persuade others to act accordingly, and they derive utility from that thought. Due to decreasing marginal returns to campaign spending, if her candidate raised more contributions than the competitor, the individual would derive less utility and thus feel less prone to make the marginal contribution.<sup>31</sup>

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<sup>30</sup>For evidence on how individuals extrapolate from local information see for example Cruces et al. (2013).

<sup>31</sup>Augenblick and Cunha (2011) find that a message about Republican average donation increased the (Democrat) recipients’ contributions more than a message about average amounts given by Democrats, which the author interpret as a competition effect. Their evidence is also consistent with our interpretation.

We also report the effect of the informational treatment on the probability of making post-treatment contributions. The specification in column (7) from Table 7 is the same as in column (2), with a dependent variable that takes the value 100 if the individual made at least one post-treatment contribution. The coefficients from column (7) have the same sign as the corresponding coefficients from column (2), but none of these coefficients are statistically significant. This result may imply that informational effects are relevant for the intensive margin but not for the extensive margin of contributions. However, there is substantially more variation in amounts contributed than in the probability to contribute, so the results for the extensive margin are less precisely estimated. Finally, the specification in column (8) presents the results of the usual falsification test, where the dependent variable is the amount contributed pre-treatment. As expected, all of the coefficients are close to zero and none of them are statistically significant.

We can qualify some of the discussion of the effects of the average contribution from members from the same party ( $\bar{c}_{own}$ ) and from the difference in total amounts contributed ( $\sum c_{own} - \sum c_{opp}$ ). While the positive effect of  $\bar{c}_{own}$  on contributions is consistent with social norms, there are still other competing explanations. For example, if an individual contributes because she expects favors in return of her contribution, then the utility she expects to derive from her contribution should depend on how much she gave relative to how much other supporters of her own party contributed (assuming that a higher ranking in the distribution of contributions would ensure a higher priority in the distribution of favors). Similarly, a given contribution amount is less likely to buy a favor if there is a higher number of contributors to the same party,  $N_{own}$ , which could explain the negative effect of this variable on post-treatment contributions. Our evidence establishes that social context is important for contributions, but it can only suggest some of the potential mechanisms at play. New research designs are needed to distinguish more precisely between these and alternative competing channels.

A characteristic of this experiment deserves a final comment. The findings are remarkable in the sense that our List letters did not provide descriptive statistics about contributions - i.e., we did not include suggestive messages like “the average contribution in your neighborhood was \$500.” The letters provided itemized information about neighbors’ contribution records, and the individuals were free to process and incorporate this information as they wished or could. This setting overcomes the common criticism waged against experiments with information provision, since the findings do not rely on the experimenter prompting subjects to pay attention to a given feature of information.

## 5.2 Heterogeneous Effects and Robustness Checks

Table 8 presents results using alternative outcome variables and allowing for group-level heterogeneity. We report results for the specification with regressors  $\bar{c}_{own}$ ,  $\bar{c}_{opp}$  and  $\sum c_{own} - \sum c_{opp}$ .<sup>32</sup> Columns (1) and (2) from Table 8 compare the informational effects by contribution type: the outcomes in column (1) correspond to presidential contributions, while the outcome in column (2) corresponds to the amount contributed to other committees (e.g., campaigns for state governor), also obtained from the FEC public records. Since the baseline probability is much lower for the latter outcome, the coefficients are very imprecisely estimated, and we cannot draw any meaningful conclusions from the comparison between columns (1) and (2). Columns (3) and (4) consider the contributions during two post-treatment sub-periods: before and after September 1, 2012. The effect of  $\bar{c}_{own}$  is similar for the contribution made during the two periods, implying that our letter had a lasting effect on the contribution norm. The effect of  $\sum c_{own} - \sum c_{opp}$ , on the contrary, is only present for contributions made during the first half of the post-treatment period. One potential explanation for this finding is that, as the election neared, the recipients obtained new information about the total contributions to the two presidential campaigns that overrode the information provided in our letter. Indeed, information about the total contributions raised by both campaigns was periodically reported on and discussed in the media during the election cycle.

Columns (5) through (10) from Table 8 present evidence on further potential heterogeneous effects, captured by the interaction of the variables of interest with a group dummy (e.g., whether the contributor is Democratic). The results in columns (5) and (6) indicate that we cannot reject the null hypothesis that the coefficients are jointly the same for individuals of the two parties. The results in columns (7) and (8), in turn, show that we cannot reject the null hypothesis that the coefficients are the same for women and men. However, due to statistical power limitations, we cannot reject moderate differences between these subgroups.

The design of the experiment incorporated the possibility that the estimates of informational effects could be confounded or augmented by an effect of the letter on the visibility of the recipient’s contribution (i.e., by signaling effects). One of the goals of semi-anonymizing the records shown in the List letter and not including information about the FEC website’s search tool was to minimize the effects of this letter on the perceived visibility of the recipient’s contribution. As a way of test formally whether signaling effects were confounding the estimates of informational effects, we included two variations of the List letter, List-Once and List-Update. The difference between List-Once (sample in Appendix A.3) and List-

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<sup>32</sup>The specification, reproduced from column (4) in Table 7, is used as the baseline because it seems to best fit the data. However, the results are similar with alternative specifications.

Update (Appendix A.4) is that in the latter we specified that we may send more letters at the end of the election cycle with updated information.<sup>33</sup> If the List letter had an effect on post-treatment contributions because of changes in the perceived visibility of the recipient’s contribution, this effect should have been higher in the List-Update sub-treatment. The results in columns (9) and (10) in Table 8, however, indicate that this was not the case. The difference between the coefficients for the two sub-treatments is not statistically significant, suggesting that the information contained in the list did not have different effects between the List-Once and List-Update sub-treatments. This is suggestive evidence that the findings on informational effects are not being confounded or augmented through an indirect effect of the List letter on visibility.

### 5.3 Average Effect of Disseminating Unbiased Information about Contribution Records

The average difference in post-treatment contributions for individuals who received a List letter and individuals who did not provides an approximate measure of the effect of disseminating unbiased information about contribution records, since by design our letters on average were not biased. Table 9 presents a series of comparisons of the average post-treatment amounts contributed in the Placebo and List treatments and in the control group. The results in column (1) show that, compared to the control group, sending an individual a Placebo letter did not induce a significant change in post-treatment contributions. On the contrary, sending an individual a List letter increased contributions on average by about \$17, which is equivalent to about 3% of the average amount contributed post-treatment (note that this is just the intention to treat effect). The results in column (5) indicate that the letters did not have a significant effect on the extensive margin, which is consistent with the lack of informational effects on this margin (Table 7). Column (6) presents the result from a further falsification test, showing that the pre-treatment contributions are statistically indistinguishable between the control group and the List and Placebo treatment groups. Columns (2) to (4) present specifications where the treatment dummies are interacted with some pre-treatment characteristics. The results in Column (2) shows that the effect of the List treatment was statistically indistinguishable between the List-Once and List-Update sub-treatments, whereas the results in column (3) indicate that there are no statistically

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<sup>33</sup>To reinforce the message that an updated list of neighbors’ contributions would be sent at a later point in time, the table in the List-Update variation included two columns – one labeled “Past contributions: April 1, 2011 to April 1, 2012”, with past contributions as in List-Once, and a second column with the same format but with empty cells for each individual in the list and with the header “Future contributions to be reported: May 1, 2012 to December 1, 2012”.

significant differences between the informational effects for Republican and Democratic contributors.

Column (4) from Table 9 provides some suggestive evidence about the reason why the List letter had a positive average effect on subsequent contributions. Given the evidence on informational effects, one compelling hypothesis is that the unbiased information contained in the List letter corrected systematic biases in the recipients' belief about the contributions of others.<sup>34</sup> For example, if everyone had the same prior belief about what others were contributing, recipients from areas with higher average contributions should have a more positive reaction to the List letter. Following this intuition, column (4) presents the effect of the interaction of the List treatment with some characteristics of the contribution patterns in the individual's area, as measured in the baseline lists. If the List letter corrected systematic biases in perceptions about others' contributions, we should expect more positive effects in areas with higher average contributions by own-party neighbors (due to the effect of  $\bar{c}_{own}$ ), and more positive effects in areas where the total contributions to the own-party are surpassed by the total contributions to the opposite-party (due to the effect of  $\sum c_{own} - \sum c_{opp}$ ). The coefficients on the interaction terms reported in column (4) from Table 9 provide evidence supporting this hypothesis.

## 6 Implications of the Findings

### 6.1 Implications for the FEC Disclosure Policy

Even though it was not designed primarily for this goal, our evidence can shed some light on a general question about the consequences of information disclosure by third-parties, such as governmental or non-governmental organizations. In the case of campaign contributions, the FEC makes an active effort to disseminate aggregate and individual level information about contributions through its website. Reporting political contribution activity to a regulator such as the FEC is certainly a key aspect of ensuring compliance with the law. In fact, the widespread use of new technologies with digital filing and online reporting implies a near zero marginal cost of reporting contributions, which implies that the arbitrary lower limit of \$200 does not fulfill any practical role.

However, the current FEC regulations and practices have also established that all the information for reported contributions (i.e., for a total of \$200 or more), including all the details of the contributors, must not only be reported, but are also publicly available and,

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<sup>34</sup>Alternatively, our mailing may have made it clear to recipients that researchers were studying campaign contributions, which may have increased their perception of the importance of these contributions and thus indirectly affected their behavior.

most importantly, easily accessible for the general public. The implications of this policy have changed in the light of the technological developments of the recent decades. Most of the tools available today to access and disseminate this disaggregated information were not available when the original legislation was established (the Federal Election Campaign Act of 1971), and even the \$200 limit per election cycle at that time represents a substantially higher amount in 2014 dollars (about \$1,159.4 according to the BLS' inflation calculator). The publication of itemized details of individual contributions in electronic format, which has been used by third parties to create maps and other original forms to access the data, and the existence of the FEC's website online search tool, all contribute to the heightened visibility of individuals' contribution activity. This specific context is thus shaped by a combination of the legal requirement of making contributions public with an active policy of making this information extremely accessible. For example, voter turnout records are publicly available, but they are not as easily accessible as contribution records.

There are both costs and benefits to the existing disclosure policy. There may be real negative welfare effects in terms of chilling the speech of local political minorities and reducing the political participation of members of these minorities. Our results indicate that the disclosure policy affects contributions to political campaigns, and induces a reduction in contributions from local minorities. Besides our empirical results, there is anecdotal evidence on the harassment of contributors to certain causes or candidates, for instance against supporters of ballot proposition 8 in 2008 in California, which banned same-sex marriage (Briffault, 2010; La Raja, 2013). Chilling effects are probably not circumscribed to contributions to political campaigns, and may be present in many other forms of political participation.

This evidence also highlights a brighter side of the disclosure: if individuals react to the information about others' contributions, they must be learning something of interest from this information. The current laws and regulations seem to be mainly based on the informational advantages of full disclosure. The spirit of this argument is clearly expressed in the Supreme Court's 1976 *Buckley v. Valeo* ruling: "Disclosure provides the electorate with information (...) in order to aid the voters in evaluating those who seek federal office. It allows voters to place each candidate in the political spectrum more precisely than is often possible solely on the basis of party labels and campaign speeches" (cited by Gilbert, 2013). Our evidence on informational effects partially supports this view. Our findings indicate that individuals who received unbiased information about others' contributions were significantly more likely to contribute. Even though the evidence is only suggestive, one plausible explanation is that the List letters increased contributions because they corrected systematic biases in the beliefs about the contribution behavior of others. Thus, if a more open disclosure policy acts by disseminating unbiased information, it will have a positive effect on contributions, although

its effect on aggregate welfare is ambiguous - higher participation may not necessarily be positive because of the zero-sum nature of campaign spending. On the other hand, the value of information may also stem from a reduction in corruption given by decentralized monitoring, although there is no clearcut evidence to support this case.

In any case, the positive value of information does not necessarily stem from the fact that the identity, personal details and all itemised individual contributions are freely and easily available online. In fact, the lists in our letters were anonymized (displaying the name but only the initial of neighboring contributors' last names). Valuable information for voters may be conveyed in more aggregate ways or by different means, and the objective of decentralized monitoring can still be attained without unfettered anonymous access to all individual information.

The discussion about optimal disclosure would benefit from distinguishing first between the desirable requirement of reporting all contributions to the FEC and the level of publicity and accessibility of those detailed individual records. It would also benefit from distinguishing between large and small donors. While there may be an informational value in revealing contributors giving large amounts, it is harder to ascertain that information about small contributions has an informational value for voters. This does not mean that the optimal disclosure implies full anonymity of contributors, but that these welfare costs should enter the cost-benefit analysis of disclosure policies and regulations. Our evidence suggests that some of these negative effects, which are amplified by the combination of old policies and new technologies, might be mitigated, for instance by imposing a small cost (monetary or otherwise), or requiring those accessing the individual contribution information to register (and making these records public).

Our experiment allowed us to establish the presence and quantify the magnitude of social incentives on individual campaign contributions. While we can ascertain the existence of these social incentives, a limitation of our experiment for a full counterfactual analysis of the FEC's disclosure policy is that our evidence does not allow us to construct more relevant counterfactuals. For instance, nearly all recipients of our mailing already appeared in the FEC public records before we sent our letters. By affecting future contributions, the letters could modify the signal about the strength of the recipient's affiliation, but her political identification was already a matter of public record. Our experiment thus does not allow us to construct a counterfactual of what contribution patterns would look like without the FEC's website search tool, nor to gauge precisely the effect of alternative disclosure policies. A further limitation relates to the evidence on signaling effects. The presence of these effects indicates that individuals expect different costs and benefits when interacting with members of their reference group when they identify with the minority or the majority party, but we



cannot gauge the contribution (or cost) to individual welfare of these costs and benefits. These depend on the nature of the costs and benefits of these social interactions - i.e., only brief unpleasant encounters from time to time, or events with higher expected disutility, such as not neighbors not talking to the individual after learning their political preferences. Moreover, we cannot establish if individual's expectations of these costs and benefits in social interactions are realistic and well-founded, or exaggerated.

## 6.2 Implications for Fundraising

Our findings may be useful for fundraisers for political campaigns, charitable giving and other pro-social activities, such as environmentally-responsible behavior. Campaign managers disseminate all types of information in an effort to increase contributions. For example, in the case of our mailing campaign, the average effect of the List treatment (about \$17) is an order of magnitude higher than the cost of sending each letter (about \$0.25). Our experimental results indicate that a message about a high average contribution by individuals of the same party can increase contributions by the recipient.

Our evidence, however, also suggests that fundraisers should be careful when disseminating information, because some of these efforts may be self-defeating. We also found that a message about a high number of contributors to the same party, or a higher total amount raised through other donors, can have a negative effect on the contribution of the recipient. We interpreted this as an effect analogous free-riding in warm glow giving (Andreoni, 1989). However, our evidence is not conclusive about whether this or alternative mechanisms are at play behind this free-riding effect, and whether it would also be present in the non-partisan context of charitable giving.

## 6.3 Implications for Geographic Polarization

An individual's social context is a potentially important determinant of political participation, which involves attending political events and social interactions in general which are specific to the individual's environment (Huckfeldt, 1979). The social incentives at work in the context of campaign contributions and this intrinsic social nature of political participation can explain some of the growing geographic polarization in the U.S., i.e., the degree to which party identification is unevenly distributed across the country (Bishop, 2009). Perez-Truglia (2014) disentangles the effects of residential sorting from those of social incentives (such as conformity effects) by relying on an event study of campaign contributions by individuals moving before and after the 2012 election cycle. This analysis finds evidence of substantial conformity effects: living in a zipcode with a 10% higher share of supporters of the same

party increases an individual’s probability of making contributions by about 1.1%, which in turn implies that geographic polarization in campaign contributions in the 2012 presidential election was 18.5% higher than it would have been in the absence of social incentives of this type.

Geographic polarization can also be exacerbated by mechanisms that encourage the participation of supporters of the local majority party and discourage participation of supporters of the local minority. Our field-experimental evidence is compatible with effects of this type. It indicates that preferential treatment of like-minded individuals in social interactions makes individuals from the local minority less willing to make campaign contributions, and individuals from the local majority more likely to contribute. Moreover, social incentives may also affect geographic polarization indirectly. Supporters of the local majority party may be exposed to higher solicitation through local networks, and more interaction with like-minded individuals can induce narrowness of viewpoint and thereby increase disproportionately the participation of individuals from the local majority (Glaeser and Sunstein, 2009; Bishop, 2009). The preferential treatment towards individuals of similar political beliefs tends to induce geographical partisan segregation, and at the same time it constitutes a disincentive for political participation for local minorities. Individuals may anticipate the presence of effects of this type and choose to live in areas with like-minded neighbors (Bishop, 2009; Cho et al., 2013), thereby generating a vicious circle. While our study examines campaign contributions, the evidence suggests that similar effects could operate with respect to other forms of observable political participation, including openly discussing politics or political issues, or even running for office. A limitation of our results, however, is that it only provides indirect evidence on the precise mechanisms through which these social effects operate. For instance, we can only postulate that the driving force behind signaling effects is the expected utility (or disutility) from the interaction with like-minded individuals (or with those with opposing views), but our experiment was not designed to capture these details of social interactions.

## 7 Conclusions

There is a growing body of research on the role of social incentives in contributions and pro-social behavior. In this study, we presented original field experiment evidence on social incentives in the context of campaign contributions. Our research design harnessed the institutional context of public disclosure of campaign contributions in the United States. We presented a novel experimental design that disentangles the multiple mechanisms through which social incentives operate. The distinction made between these mechanisms proved to be important, particularly as some mechanisms had effects in opposite directions.

A first set of results on signaling effects indicate that feeling observed by others significantly affects individuals' contributions. Unlike in the cases of voter turnout and charitable giving, individuals who make campaign contributions face a double audience that may approve or disapprove of their actions. We found that higher visibility increases contributions for individuals supporting the majority party in the areas, but decreases contributions for individuals supporting the minority party in the area.

A second set of results, about informational effects, indicate that an individual's contributions are also affected by her perception of others' contribution patterns. An individual contributes more if she observes a higher average contribution by neighbors who support her same party, which is consistent with the existence of a social norm. Individuals do not react to information about the average contributions of supporters of the opposite party. We found no evidence that individuals contribute because they believe to be making a pivotal contribution, which would have been compatible with an investment motive for campaign contributions. Finally, we found that individuals contribute less when their own party is raising higher total contributions than the opposite party, which we interpret as a form of free-riding in warm-glow giving.

The evidence presented in this paper has implications in several areas. First, our findings are relevant for disclosure policies - both for campaign contributions and for other areas of public life. Technological change is constantly reducing the cost of collecting and disseminating information. The government, but also the private sector, must decide how to disclose this data. Our evidence indicates that, due to social incentives, disclosure policies are non-neutral: i.e., the individual's behavior depends on whether her actions are being disclosed or not. Our results provide some insights on how to evaluate the effects of disclosure policies in the context of campaign contributions, but our experimental design can also be used to study the impact of disclosure policies in other contexts, such as charitable giving or tax evasion.<sup>35</sup> Our original evidence can also suggest new ways in which social incentives could be exploited to design better fundraising strategies. For example, one of the experimental findings suggests that disseminating unbiased information about contributions in a given area may be a cost-effective way of increasing contributions. The evidence also warns about potential self-defeating dissemination efforts: e.g., conveying the message that the total amount raised is high will decrease the individual's desired contribution through the moral free-riding effect. Finally, our results can also shed some light on the effects of social incentives on geographical political polarization in the U.S.

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<sup>35</sup>The distinction between different social mechanisms allows for a better design of disclosure policies. For example, if the FEC wanted to avoid signaling effects in campaign contributions without affecting the informational effects, it could semi-anonymize the contribution records published on its website.

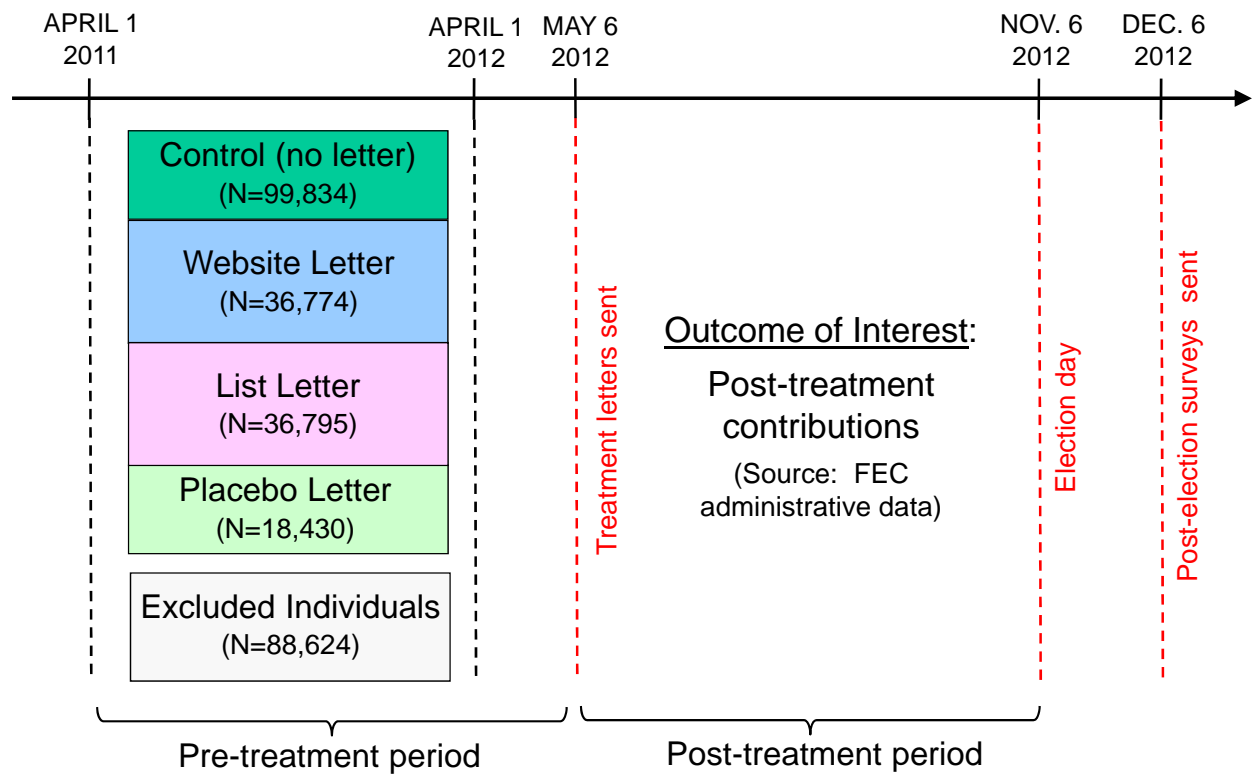
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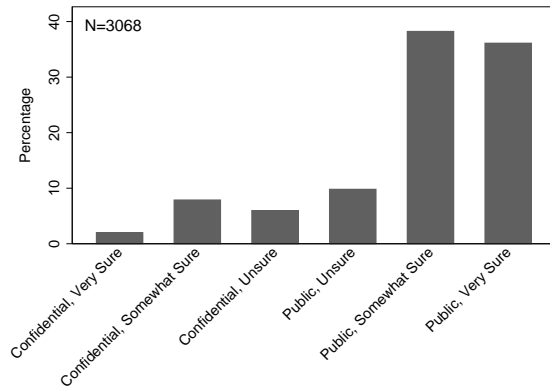
Figure 1: Timeline of the Field Experiment



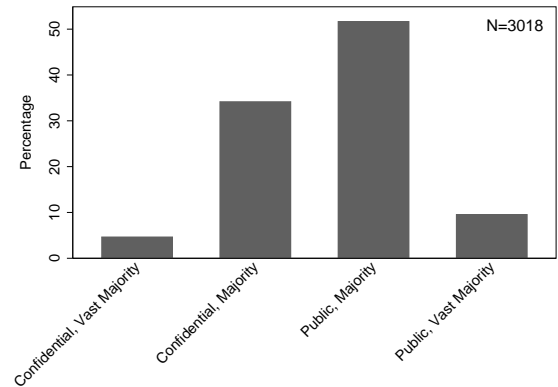
Notes: See Section 2 for more details about the experimental design.

Figure 2: Contributors' Perceptions about Confidentiality of Contribution Records (Post-Election Mail-In Survey)

**a.** Respondents' perceptions about whether contribution records are confidential/public:



**b.** Respondents' perceptions about the proportion of their neighbors that believe that contribution records are public/confidential:

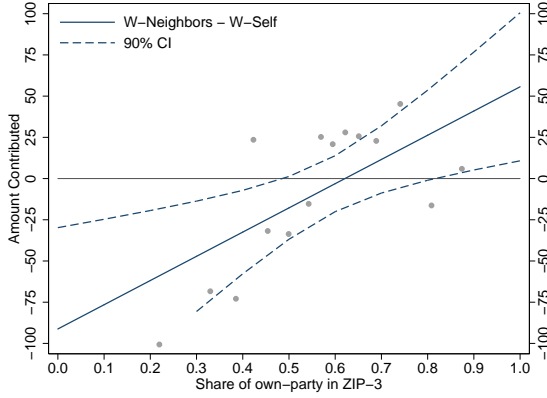


Notes: The data corresponds to the responses to our post-election mail-in survey. The figures are based on responses from individuals in the experiment's Control group (i.e., individuals that did not receive any mailings). Panel a combines the answers to two questions: a question about the disclosure policy of information on individual contributions (see question 4 from the questionnaire in Appendix C) and a further question on the respondent's confidence on the answer provided to the previous question (see question 5 from the questionnaire in Appendix C). Panel b is based on responses to a question about what the respondent thinks that her neighbors believe about the disclosure policy, where the four possible responses range from "A vast majority of your neighbors believe that information on individual campaign contributions is public" to "A vast majority of your neighbors believe that information on individual campaign contributions is confidential" (see question 7 from the questionnaire in Appendix C).

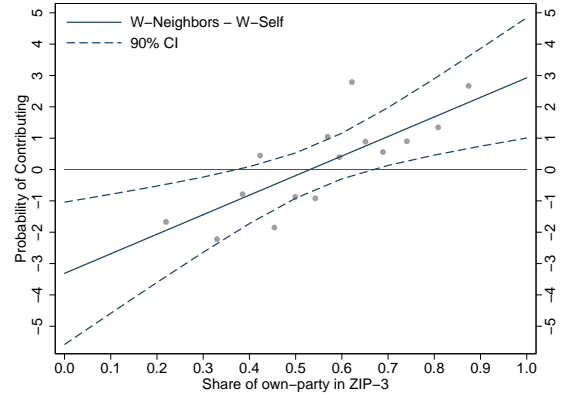


Figure 3: Signaling Effects: Experimental Results in Graphical Form

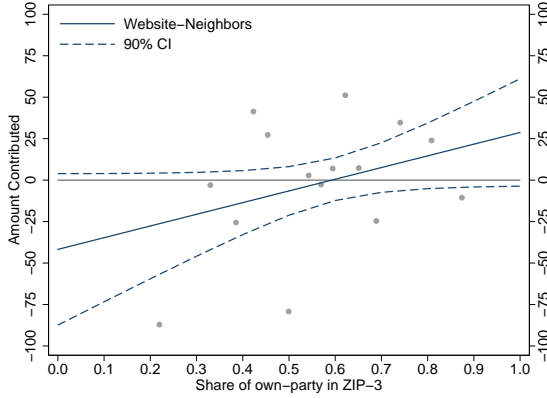
a. Signaling effect (W-Neighbors - W-Self)



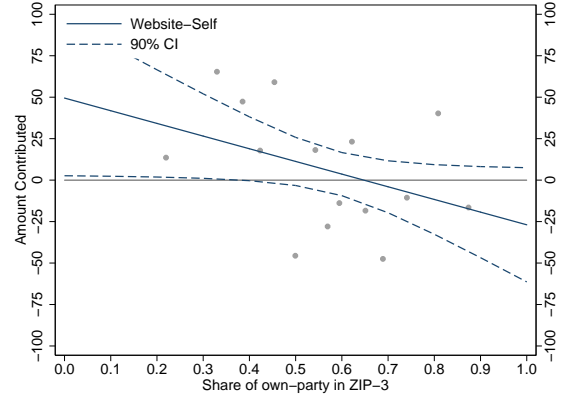
b. Signaling effect, extensive margin



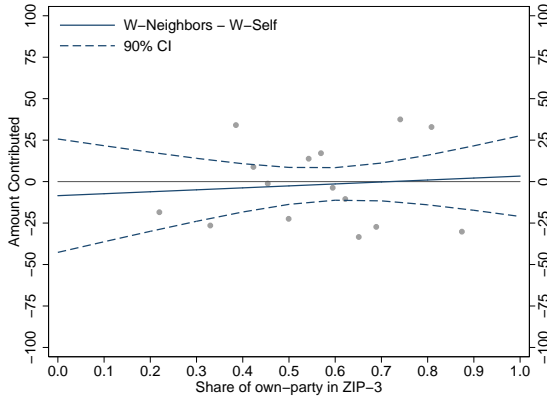
c. Effect of Website-Neighbors letters



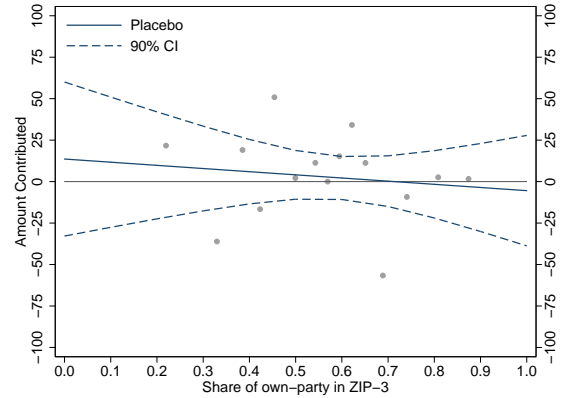
d. Effect of Website-Self letters



e. Signaling effect, pre-treatment contributions

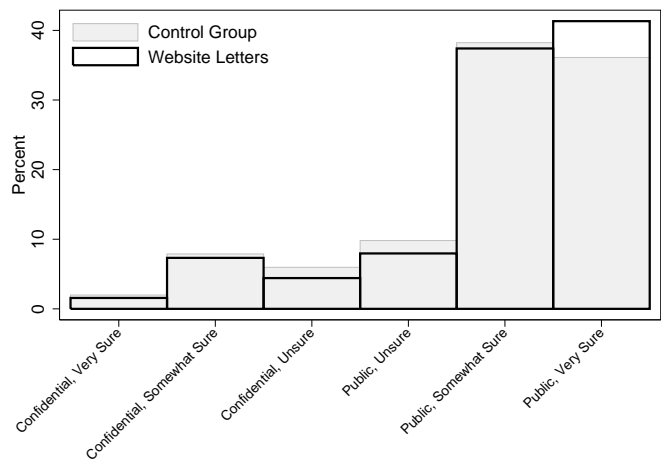


f. Effect of Placebo letters



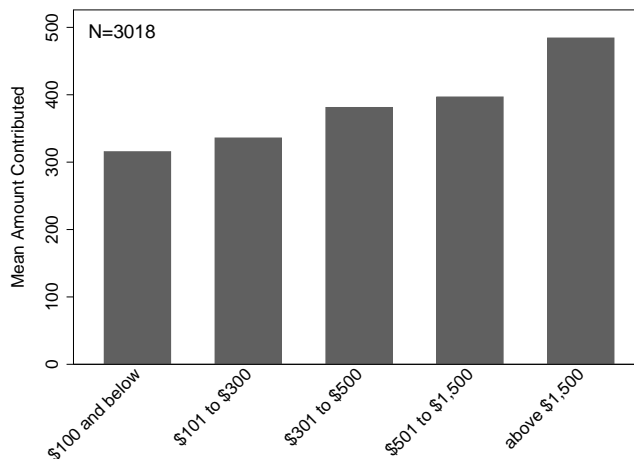
Notes:  $N = 154,707$ . Each regression line and its confidence interval were estimated from a regression of the relevant outcome (e.g., amount contributed post-treatment) on treatment dummies, the share of own-party contributors in the 3-digit ZIP code (ZIP-3) and the interaction between these variables, and individual-level controls. Interval regressions are used when the amount contributed is the dependent variable, and OLS for all others. Standard errors are clustered at the ZIP code/party level. For more details about the specification, see the notes to Table 4. The dots represent binned scatter plot versions of the partial regression plots.

Figure 4: Effect of Website Letter on Belief that Contribution Records are Confidential/Public (Post-Election Mail-In Survey)



Notes: Histograms are based on responses to the post-election mail-in survey. Control Group refers to respondents who did not receive any letters during the experimental stage, while Website Letters refers to respondents who received a letter of type Website. This measure of perceptions about the publicity of contribution records combines the answers to a question about the type of disclosure policy (question 4 from Appendix C) and another question about the respondent’s confidence on that answer (question 5 from Appendix C).

Figure 5: Relationship between self-reported contribution norm and actual amount contributed (Post-Election Mail-In Survey)



Notes: The data corresponds to the responses to our post-election mail-in survey and to respondents' contributions matched from FEC records (see Table 3 for some descriptive statistics of this data). The figures are based on responses from individuals in the experiment's Control group (i.e., individuals that did not receive any mailings). The x-axis corresponds to the quintiles of the distribution of responses to the survey question about how much individuals "should" contribute to a presidential campaign (question 8 from Appendix C). The y-axis represents the average amount actually contributed by respondents during the presidential election cycle.

Table 1: Comparison of Individual Characteristics for Individuals in the Subject Pool, for All Contributors in the 2012 Election Cycle and for the General U.S. Population

	Subject Pool	Contributors	US Average
Percent Democrat	52.59 (49.93)	64.55 (47.84)	51.40
Mean amount contributed (\$)	811.69 (880.61)	559.39 (846.59)	
Percent male	59.37 (48.27)	55.15 (48.82)	49.14
Percent white	79.14 (21.68)	78.77 (22.20)	62.99
Percent black	11.95 (14.34)	12.01 (14.52)	12.07
Population density, ZIP-5	6177.33 (15505.87)	6360.17 (16136.04)	3907.85
Mean income (\$), ZIP-5	105009.24 (114364.60)	98097.34 (113653.43)	55241.02
Observations	191,832	1,070,098	

Notes: Average individual characteristics (standard deviations in parenthesis). The first column corresponds to individuals who made contributions to presidential campaigns from April 1, 2011 to April 1, 2012 and were selected for the field experiment according the criteria described in Section 2. The second column corresponds to all individuals who made contributions to presidential campaigns during the 2012 election cycle (the subject pool in the first column is a subset of this group). The third column corresponds to country-averages using the ZIP code level 2010 U.S. Census data. Data on contributions from the FEC public records, which includes individuals contributing over \$200 to a campaign committee. The FEC database does not report information about the gender or the ethnicity of individual contributors. However, we constructed proxies for these variables based on information provided by the U.S. Bureau of the Census, which reports the joint distribution of first names and gender, and the joint distribution of last names and ethnicities. Population density and mean income come from 2010 U.S. Census data. The U.S. average share of democrats corresponds to the share of Democrat votes in the 2008 presidential election.

Table 2: Balance of Observable Individual Characteristics across Treatment Groups

	(1) Control	(2) Placebo	(3) Website Self	(4) Website Neighbors	(5) List Once	(6) List Update	(7) Difference Test P-value
Percent Democratic	52.59 (0.16)	52.67 (0.37)	53.33 (0.37)	52.53 (0.37)	52.46 (0.37)	52.00 (0.37)	0.24
Mean amount contributed (\$)	524.24 (2.00)	520.15 (4.51)	516.55 (4.56)	517.57 (4.70)	528.91 (4.74)	526.30 (4.76)	0.29
Percent male	59.26 (0.16)	59.59 (0.37)	58.66 (0.37)	59.68 (0.37)	59.54 (0.37)	59.91 (0.37)	0.19
Percent white	79.12 (0.07)	78.99 (0.17)	79.17 (0.17)	79.20 (0.17)	79.20 (0.17)	79.32 (0.17)	0.82
Percent black	11.91 (0.05)	12.11 (0.11)	11.86 (0.11)	11.98 (0.11)	12.10 (0.11)	11.90 (0.11)	0.40
Percent hispanic	4.12 (0.05)	4.16 (0.11)	4.03 (0.11)	4.06 (0.11)	3.84 (0.10)	3.99 (0.11)	0.23
Observations	99834	18430	18314	18459	18396	18399	

Notes: Averages for different pre-treatment individual-level characteristics for treatment groups. Standard errors in parenthesis. The last column reports the p-value of a test in which the null hypothesis is that the mean is equal for all the treatment groups. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data). Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau.

Table 3: Pre-Treatment and Post-Treatment Contribution Patterns for Experimental Subject Pool

	All	Pre-treatment contribution to		
		Obama	Romney	Other
<i>Pre-Treatment</i>				
Total amount contributed to all candidates (\$)	523.12 (632.41)	370.19 (359.92)	650.86 (683.29)	706.80 (842.07)
Percent contributed to Obama/Romney	65.75 (47.46)	100.00 (0.00)	100.00 (0.00)	1.01 (10.02)
Amount to Obama/Romney (\$), if >0	417.94 (403.94)	370.19 (359.92)	610.13 (502.37)	477.42 (447.60)
<i>Post-Treatment</i>				
Percent contributed to Obama/Romney	49.01 (49.99)	76.20 (42.59)	38.42 (48.64)	11.80 (32.26)
Amount to Obama/Romney (\$), if >0	588.84 (684.37)	567.12 (643.43)	609.89 (727.69)	775.66 (941.03)
Observations	191832	100541	24910	66381

Notes: Average contribution behavior (standard deviations in parenthesis). The pre-treatment period ranges from April 1, 2011 to the date of receiving the letter, and the post-treatment period ranges from the date of receiving the letter to December 31, 2012. The other Republican candidates are: Bachman, Cain, Gingrich, Huntsman, Paul, Pawlenty, Perry and Santorum. Data on amount contributed and recipient party from FEC public records.

Table 4: Signaling Effects: Robustness Checks

	Post-Treatment Contribution				Pre-Treatment
	(1)	(2)	(3)	(4)	(5)
	Amount	Amount	Amount	Amount	Amount
W-Neighbors - W-Self	-91.237** (37.473)	-97.580** (38.710)	-88.175** (42.195)	-90.137 (75.526)	-15.305 (22.828)
Interaction with:					
Share own-party in ZIP-3	146.902** (61.381)	150.948** (63.651)	157.048* (86.320)	146.781** (61.917)	24.987 (37.193)
Share own-party in adj. ZIP-3			-16.123 (98.256)		
Share low-income in ZIP-3				-2.233 (128.722)	
Observations	155,037	155,037	155,037	155,037	155,037
Controls	Yes	No	Yes	Yes	No
Regression	Interval	Interval	Interval	Interval	OLS

Notes: \* significant at the 10% level, \*\* at the 5% level, \*\*\* at the 1% level. Standard errors clustered at the ZIP code/party level. The dependent variable is the amount in dollars contributed to the Obama or Romney presidential campaign after receiving the letter, except for column (6) in which the dependent variable is the amount contributed before receiving the mailing (pre-treatment). All regressions include observations from the Control group and from the Placebo and Website treatment groups. The key independent variables are the treatment-type dummies interacted with ZIP-3 characteristics (e.g., share of own-party contributors). W-Neighbors - W-Self is the difference between the coefficients on the treatment dummies for Website-Neighbors and Website-Self. The share own-party in ZIP-3 stands for the share of own-party contributors to presidential campaigns in the ZIP-3 in the three previous presidential election cycles. The “Share own-party in adj.-ZIP-3” refers to the same variable averaged over the ZIP-3s adjacent to the ZIP-3 where the individual resides. The “Share low-income in ZIP-3” refers to the share of income-earning adults with income below \$30,000, according to U.S. Census Bureau data for 2010. Controls refers to individual-level control variables: a party dummy, amount contributed pre-treatment to each candidate, gender, race dummies and the date when received the mailing. Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).

Table 5: Signaling Effects: Treatment Effects on Alternative Outcomes and Group-Level Heterogeneity

Signaling semi-elasticity (prob. of contributing):	By Cont. Type		By Cont. Date		By Party		By Gender		By Pop. Density	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
20% own-party in ZIP-3	-0.064** (0.032)	-0.057 (0.068)	-0.104*** (0.038)	-0.031 (0.038)	-0.025 (0.025)	-0.111 (0.070)	-0.061 (0.045)	-0.065 (0.045)	-0.054 (0.043)	-0.065 (0.053)
80% own-party in ZIP-3	0.046** (0.021)	0.061 (0.050)	0.066*** (0.023)	0.003 (0.024)	0.023** (0.011)	0.055 (0.065)	0.035 (0.033)	0.055** (0.028)	0.058** (0.025)	0.023 (0.040)
Sub-group	Pres.	Other	<Sep-1	>Sep-1	DEM	REP	Male	Female	>Median	<Median
Observations	155,037	155,037	155,037	155,037	155,037	155,037	155,037	155,037	155,037	155,037

Notes: \* significant at the 10% level, \*\* at the 5% level, \*\*\* at the 1% level. Standard errors clustered at the ZIP code/party level. The semi-elasticity denotes the proportional change in the probability of making a contribution in the post-treatment period from re-assigning the recipient from the Website-Self to the Website-Neighbors treatment. The semi-elasticity is computed for two values of the share of own-party contributors in the ZIP-3: 20% and 80%. The dependent variable is always a dummy variable for whether the individual contributed at all to either Obama or Romney after receiving the mailing, except in column (2) where it corresponds to contributions to other committees. Columns (3) and (4) consider contributions made during two different timeframes of the post-treatment period, before and after September 1, 2012. In columns (9) and (10) the sub-groups are defined according to the population density in the individual's ZIP-3, computed with 2010 data from the U.S. Census Bureau. The semi-elasticities are computed using the coefficients from a Logit regression, where the key independent variables are the treatment-type dummies interacted with ZIP-3-characteristics (share of own-party contributors in the ZIP-3). The group-specific effects are computed by interacting the main independent variables with the group-dummy (e.g., Democratic vs. Republican), except in columns (1) through (4) where each sub-group corresponds to the use of a different dependent variable. The regression specifications follow the same criteria described in the note to Table 4. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).



Table 6: Informational Effects: Sample Treatment Lists Generated with Different Parameter Values

<b>Baseline</b> ( $\theta_i^D = 0, \theta_i^A = 0$ )			<b>Low DEM</b> ( $\theta_i^D < 0, \theta_i^A = 0$ )			<b>High Amount</b> ( $\theta_i^D = 0, \theta_i^A > 0$ )		
Contributor	Amount	Party	Contributor	Amount	Party	Contributor	Amount	Party
<b>G., R.</b>	<b>\$1,000</b>	<b>DEM</b>	<b>G., R.</b>	<b>\$1,000</b>	<b>DEM</b>	<b>G., R.</b>	<b>\$1,000</b>	<b>DEM</b>
<b>W., D.</b>	<b>\$500</b>	<b>DEM</b>	<b>S., L. Y.</b>	<b>\$500</b>	<b>DEM</b>	<b>H., J. B.</b>	<b>\$1,000</b>	<b>DEM</b>
<b>S., L. Y.</b>	<b>\$500</b>	<b>DEM</b>	<b>A., S.</b>	<b>\$200</b>	<b>DEM</b>	<b>P., R.</b>	<b>\$700</b>	<b>DEM</b>
<b>W., T. K.</b>	<b>\$500</b>	<b>DEM</b>	<b>B., R.</b>	<b>\$200</b>	<b>DEM</b>	<b>W., D.</b>	<b>\$500</b>	<b>DEM</b>
<b>A., S.</b>	<b>\$200</b>	<b>DEM</b>	<b>W., S. B.</b>	<b>\$1,100</b>	<b>REP</b>	<b>S., L. Y.</b>	<b>\$500</b>	<b>DEM</b>
<b>B., R.</b>	<b>\$200</b>	<b>DEM</b>	<b>O., T. F.</b>	<b>\$800</b>	<b>REP</b>	<b>W., T. K.</b>	<b>\$500</b>	<b>DEM</b>
<b>W., S. B.</b>	<b>\$1,100</b>	<b>REP</b>	<b>B., M. A.</b>	<b>\$400</b>	<b>REP</b>	<b>W., S. B.</b>	<b>\$1,100</b>	<b>REP</b>
<b>B., M. A.</b>	<b>\$400</b>	<b>REP</b>	<b>A., E. A.</b>	<b>\$250</b>	<b>REP</b>	<b>O., T. F.</b>	<b>\$800</b>	<b>REP</b>
<b>A., E. A.</b>	<b>\$250</b>	<b>REP</b>	<b>H., V.</b>	<b>\$200</b>	<b>REP</b>	<b>B., M. A.</b>	<b>\$400</b>	<b>REP</b>

Notes: This is an example of how the algorithm generates different lists of 9 neighbors from a given sample of the recipient's 30 closest contributors. See Section 5 for a detailed description of the algorithm.

Table 7: Informational Effects: Main Regression Results

	Post-Treatment Contributions							Pre-Treatment
	(1) Amount	(2) Amount	(3) Amount	(4) Amount	(5) Amount	(6) Amount	(7) P(Amount>0)	(8) Amount
$\bar{c}_{own}$	2.452* (1.436)	2.757* (1.440)	4.642** (2.052)	4.032*** (1.562)	2.694* (1.494)	2.456* (1.439)	0.048 (0.049)	1.028 (0.915)
$\bar{c}_{opp}$	-0.145 (0.914)	-0.667 (0.951)	-1.131 (1.280)	-1.450 (1.077)	-0.075 (0.931)	-0.146 (0.914)	-0.055 (0.043)	-0.288 (0.610)
$N_{own}$		-6.217** (2.821)					-0.180 (0.113)	1.071 (1.760)
$\sum c_{own}$			-0.546 (0.352)					
$\sum c_{opp}$			0.295 (0.286)					
$\sum c_{own} - \sum c_{opp}$				-0.408** (0.171)				
$ \sum c_{own} - \sum c_{opp} $					-0.091 (0.177)			
$ \sum c_{own} - \sum c_{opp} ^*$						-0.016 (0.196)		
Observations	155,059	155,059	155,059	155,059	155,059	155,059	155,059	155,059
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Regression	Interval	Interval	Interval	Interval	Interval	Interval	Linear	Linear

Notes: \* significant at the 10% level, \*\* at the 5% level, \*\*\* at the 1% level. Standard errors clustered at the ZIP code/party level. The dependent variable in columns (1) through (5) is the dollar amount contributed post-treatment. The dependent variable in column (6) takes the value 100 if the individual made at least one contribution post-treatment and 0 otherwise. The dependent variable in column (7) is the dollar amount contributed pre-treatment. All the independent variables except  $N_{own}$  are expressed in hundreds of dollars.  $\bar{c}_{own}$  (conversely,  $\bar{c}_{opp}$ ) corresponds to the average contribution of all the individuals in the list who contributed to the recipient's own (opposite) party.  $N_{own}$  is the number of individuals in the list who contributed to the recipient's party.  $\sum c_{own}$  ( $\sum c_{opp}$ ) is defined as  $\bar{c}_{own}$  ( $\bar{c}_{opp}$ ) but refers to the sum of contributions instead of the average contribution. See Table F.1 for descriptive statistics for all these independent variables. These independent variables are included in the regression as the difference between the value computed with the list sent to the recipient and the corresponding value computed in the baseline list, except  $|\sum c_{own} - \sum c_{opp}|^*$  for which we first compute the difference of  $\sum c_{own} - \sum c_{opp}$  between the list sent and the baseline list, and then use the absolute value of this difference. All regressions include observations from the Control group and the Placebo and List treatment groups. The individual-level control variables are: dummies for Placebo and List, party dummy, amount contributed pre-treatment to each candidate, gender, race dummies and the date when received the mailing. Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).

Table 8: Informational Effects: Treatment Effects on Alternative Outcomes and Group-Level Heterogeneity

	By Cont. Type		By Cont. Date		By Party		By Gender		By Once/Update	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.: Amount										
$\bar{c}_{own}$	4.032*** (1.562)	10.113 (14.038)	2.910** (1.280)	2.204* (1.224)	5.180* (2.756)	2.851 (1.909)	3.203 (2.145)	4.492** (2.251)	3.439 (2.198)	4.634** (2.197)
$\bar{c}_{opp}$	-1.450 (1.077)	-8.226 (7.902)	-1.068 (0.828)	-0.619 (0.876)	1.388 (3.244)	-1.839 (1.146)	-0.804 (1.369)	-2.236 (1.648)	-1.367 (1.542)	-1.536 (1.469)
$\sum c_{own} - \sum c_{opp}$	-0.408** (0.171)	-0.880 (1.260)	-0.464*** (0.137)	-0.043 (0.135)	-0.537 (0.351)	-0.418** (0.200)	-0.508** (0.234)	-0.324 (0.253)	-0.302 (0.240)	-0.514** (0.240)
Sub-group	Pres.	Other	$\leq$ Sep-1	$>$ Sep-1	REP	DEM	Female	Male	Once	Update
Observations	155,059	155,059	155,059	155,059	155,059	155,059	155,059	155,059	155,059	155,059

Notes: \* significant at the 10% level, \*\* at the 5% level, \*\*\* at the 1% level. Standard errors clustered at the ZIP code/party level. The dependent variable is the amount contributed to either Obama or Romney after receiving the mailing, except in column (2) where it corresponds to contributions to other committees. Columns (3) and (4) consider contributions made during two different timeframes of the post-treatment period, before and after September 1, 2012. In column (9) and (10) the sub-groups correspond to whether the individual was assigned to the sub-treatment mailing type List-Once or List-Update. The group-specific effects are computed by interacting the main independent variables with the group-dummy (e.g., Democratic contributor), except in columns (1) through (4) where each sub-group corresponds to the use of a different dependent variable. The regression specifications follow the same criteria described in the note to Table 7. Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).

Table 9: Informational Effects: Average Intention to Treat Effects of List Letter on Contributions

	Post-Treatment					Pre-Treatment
	(1) Amount	(2) Amount	(3) Amount	(4) Amount	(5) P(Amount>0)	(6) Amount
Placebo	2.646 (7.939)	2.646 (7.939)	2.650 (7.937)	2.756 (7.938)	0.124 (0.327)	-3.827 (5.350)
List	17.033*** (6.034)	18.012** (8.239)	19.823*** (6.336)	26.237*** (8.524)	0.379 (0.245)	2.231 (4.362)
List * Update		-1.956 (10.698)				
List * REP			-8.365 (14.146)			
List * ( $\bar{c}_{own} > \$500$ )				6.502 (13.165)		
List * ( $\sum c_{own} - \sum c_{opp} > 0$ )				-21.182* (12.124)		
Observations	155,059	155,059	155,059	155,059	155,059	155,059
Controls	Yes	Yes	Yes	Yes	Yes	No
Regression	Interval	Interval	Interval	Interval	Linear	Linear

Notes: \* significant at the 10% level, \*\* at the 5% level, \*\*\* at the 1% level. Standard errors clustered at the ZIP code/party level. The dependent variable is the amount contributed after receiving the mailing, with the exception of the dependent variable in column (5), which takes the value 100 if the individual made at least one contribution post-treatment and 0 otherwise, and the dependent variable in column (6), which is the dollar amount contributed pre-treatment. All regressions include observations from the Control group and from the Placebo and List treatment groups. The coefficients for List and Placebo correspond to the dummy variables for the corresponding treatment groups. ( $\bar{c}_{own} > \$500$ ) is a dummy variable that takes the value 1 if the baseline list of 9 neighbors for the individual has an average own-party contribution above \$500. ( $\sum c_{own} - \sum c_{opp}$ ) is a dummy variable that takes the value 1 if the baseline list of 9 neighbors for the individual has a higher own-party total contribution than opposite-party total contributions. REP is a dummy variable that takes the value 1 if the individual contributed to the Republican party. The regression includes the usual set of individual-level control variables: party dummy, amount contributed pre-treatment to each candidate, gender, race dummies and the date when received the mailing. Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).

## A Further Details on Treatment Letters

As mentioned above, our field experiment included three main treatment types (Website, List and Placebo) as well as sub-types. Appendices A.1-A.5 show samples of the letters for different treatment types and sub-types. All these letter types shared basic characteristics. They all included the same header (“Boston, April 25th 2012”) and the same last paragraph: “This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. You can find more information about this project, including contact information, on our website.” The letters included the web address of the project’s website, shown in Appendix B, which provided basic information about the research project, and contact information to reach the research team and the University’s Institutional Review Board. The main purpose of the website was to provide contextual information about our study to interested subjects, and to dissipate any doubts about its legitimacy, emphasizing its academic and non-partisan nature. Although the website provided some general information about the main research objective, to avoid the contamination of the experimental results, the website did not provide any details about the precise hypotheses to be tested, nor about the existence of several different treatment types. We directed individuals who were interested in receiving a debriefing brochure (a non-technical summary of the study’s main hypotheses and results) to sent an email to an email address. We sent the brochure only after the data collection process was completed.

The mailing consisted of a single sheet of paper that folded and sealed to make a letter-size mailpiece. The outside of the mailpiece, a sample of which is shown in Appendix A.6, was also the same for all treatment types. The design reflected two objectives. First, we wanted to maximize the credibility of the content. The outside of the mailpiece had the non-profit postage as well as the sender’s Harvard address, in order to increase the recipient’s confidence in the origin of the letter. We also wanted to maximize the recipient’s interest in the letter and avoid it being discarded as junk mail. For this reason, we included a personalized message on the front (smaller font) and on the back (larger font) of the outside of the mailpiece. This message included the name of the recipient and indicated that the letter contained information about campaign contributions. Since all recipients had made contributions in the past, a personalized letter referring to this topic should have piqued the recipient’s interest. However, in the middle of the election cycle these contributors probably received a great deal of unsolicited mail related to the campaign, so we expected that a majority of our letters would be discarded without even being opened.

## A.1 Sample Letter: Website-Self

Boston, April 25<sup>th</sup> 2012

Dear John,

This letter is part of an effort to disseminate information about political campaign contributions made by individuals from your neighborhood:

Name of contributor	Amount - Party contributed to	Name of contributor	Amount - Party contributed to
S., ANITA	\$600 – DEM	H., ROBERT L	\$300 – DEM
<b>DOE, JOHN</b>	<b>\$375 – DEM</b>	L., EDMOND	\$2,500 – REP
T., WILLIAM JR	\$1,000 – REP	G., LISA	\$1,000 – REP

**YOUR HOUSEHOLD WAS THE ONLY HOUSEHOLD RANDOMLY CHOSEN FROM YOUR AREA TO RECEIVE A LETTER OF THIS TYPE**

The above table contains a list of the total campaign contributions to presidential candidates made by 6 individuals from your neighborhood in the period from April 1, 2011 to April 1, 2012, according to the public records published by the Federal Election Commission.

**Your full name, address and details about your campaign contributions are freely available to anyone with Internet access.** You can search for individual contributions by first and last name, or by zip code, using the following tool from the website of the Federal Election Commission:

[www.fec.gov/finance/disclosure/norindsea.shtml](http://www.fec.gov/finance/disclosure/norindsea.shtml)

**You can use this website to see which candidates or political parties your neighbors, friends, family and co-workers are contributing to.** Access to the data is anonymous.

This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. We will not send any more letters about past or future contributions to your household or to your neighbors. You can find more information about this project, including contact information, on our website:

**Information Dissemination on Campaign Contributions**

**[www.campaign-information.info](http://www.campaign-information.info)**

## A.2 Sample Letter: Website-Neighbors

Boston, April 25<sup>th</sup> 2012

Dear John,

This letter is part of an effort to disseminate information about political campaign contributions made by individuals from your neighborhood:

Name of contributor	Amount - Party contributed to	Name of contributor	Amount - Party contributed to
S., ANITA	\$600 – DEM	H., ROBERT L	\$300 – DEM
<b>DOE, JANE</b>	<b>\$375 – DEM</b>	L., EDMOND	\$2,500 – REP
T., WILLIAM JR	\$1,000 – REP	G., LISA	\$1,000 – REP

**YOUR HOUSEHOLD AND OTHER HOUSEHOLDS IN YOUR AREA WERE RANDOMLY CHOSEN TO RECEIVE A LETTER OF THIS TYPE**

The above table contains a list of the total campaign contributions to presidential candidates made by 6 individuals from your neighborhood in the period from April 1, 2011 to April 1, 2012, according to the public records published by the Federal Election Commission.

**Your full name, address and details about your campaign contributions are freely available to anyone with Internet access.** You can search for individual contributions by first and last name, or by zip code, using the following tool from the website of the Federal Election Commission:

[www.fec.gov/finance/disclosure/norindsea.shtml](http://www.fec.gov/finance/disclosure/norindsea.shtml)

**You can use this website to see which candidates or political parties your neighbors, friends, family and co-workers are contributing to.** Access to the data is anonymous.

This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. We will not send any more letters about past or future contributions to your household or to your neighbors. You can find more information about this project, including contact information, on our website:

**Information Dissemination on Campaign Contributions**

**[www.campaign-information.info](http://www.campaign-information.info)**

## A.3 Sample Letter: List-Once

Boston, April 25<sup>th</sup> 2012

Dear John,

This letter is part of an effort to disseminate information about political campaign contributions made by individuals. According to the public records of the Federal Election Commission, **this is a list of the political campaign contributions to presidential candidates made by 10 individuals from your neighborhood:**

Last name initial and first name of contributor	April 1, 2011 to April 1, 2012	
	Amount contributed	Party contributed to
DOE, JOHN	\$250	REP
M., CHARLES	\$1,000	DEM
C., SUSAN	\$500	DEM
D., ANN	\$500	DEM
B., CAROL	\$250	DEM
L., ANNE	\$212	DEM
W., CHARLOTTE T.	\$200	DEM
W., MELANIE	\$2,500	REP
P., JAMES	\$2,000	REP
H., PATRICK	\$750	REP

**THIS IS THE ONLY TIME WE WILL CONTACT YOU. WE WILL NOT SEND A LIST OF FUTURE CONTRIBUTIONS TO YOUR HOUSEHOLD OR TO OTHER HOUSEHOLDS IN YOUR AREA.**

This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. You can find more information about this project, including contact information, on our website:

**Information Dissemination on Campaign Contributions**

**[www.campaign-information.info](http://www.campaign-information.info)**



## A.4 Sample Letter: List-Update

Boston, April 25<sup>th</sup> 2012

Dear Jane,

This letter is part of an effort to disseminate information about political campaign contributions made by individuals. According to the public records of the Federal Election Commission, **this is a list of the political campaign contributions to presidential candidates made by 10 individuals from your neighborhood:**

Last name initial and first name of contributor	Past contributions: April 1, 2011 to April 1, 2012		Future contributions to be reported: May 1, 2012 to December 1, 2012	
	Amount contributed	Party contributed to	Amount contributed	Party contributed to
DOE, JANE	\$495	DEM		
F., BEATRICE	\$1,000	DEM		
W., FREDRICA	\$712	DEM		
L., ANN	\$250	DEM		
D., GENIEVE	\$2,500	REP		
V., PAUL	\$2,500	REP		
D., KATHERINE	\$2,500	REP		
W., SETH	\$2,500	REP		
R., ERIC	\$1,000	REP		
E., STUART	\$500	REP		

**WE WILL SEND AN UPDATED LIST OF FUTURE CONTRIBUTIONS TO SOME HOUSEHOLDS.  
SOME OF YOUR NEIGHBORS MAY RECEIVE SUCH A LIST.**

This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. You can find more information about this project, including contact information, on our website:

**Information Dissemination on Campaign Contributions**

**[www.campaign-information.info](http://www.campaign-information.info)**

## A.5 Sample Letter: Placebo

Boston, April 25<sup>th</sup> 2012

Dear Jane,

This letter is part of an effort to disseminate information about political campaign contributions made by individuals. Your household was randomly chosen to receive this information.

According to the Federal Election Commission, the limits on campaign contributions for individuals are as follows:

- \$2,500 to each candidate or candidate committee per election.
- \$30,800 to national party committee per calendar year.
- \$10,000 to state, district & local party committee per calendar year.
- \$5,000 to any other political committee per calendar year.
- \$117,000 overall biennial limit (\$46,200 to all candidates and \$70,800 to all PACs and parties).


This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. You can find more information about this project, including contact information, on our website:

**Information Dissemination on Campaign Contributions**

**[www.campaign-information.info](http://www.campaign-information.info)**

## A.6 Outside of the Mailing

### Outside of the mailing - front

<b>DEAR JOHN: IMPORTANT INFORMATION ABOUT POLITICAL CAMPAIGN CONTRIBUTIONS</b>	<p>Harvard University 108 Littauer Center Cambridge, MA 02138</p> <p style="text-align: right;">Nonprofit Org. US Postage PAID Permit No. 492 04104</p>
<div style="text-align: right;"> 36</div> <p>002*01*****3-DIGIT 040</p> <p><b>TO:</b> JOHN DOE 123 MAIN ST SPRINGFIELD, VA 22150-1234</p>	

### Outside of the mailing - back

<b>DEAR JOHN: IMPORTANT INFORMATION ABOUT POLITICAL CAMPAIGN CONTRIBUTIONS</b>
--

## B Text Displayed on the Website Mentioned in the Letters

Welcome to our website. We are a group of researchers at Harvard University studying political campaign contributions made by individuals. With that goal, we are sending out personalized mailings about campaign contributions in the U.S. If you received a letter and have any questions about the information provided to you, or our research, please feel free to email us at [link](#) and we will get back to you as soon as possible.

The purpose of our research project is to study the implications of the public's awareness about the open nature of campaign contributions. The ultimate goal is to understand the different mechanisms through which the open nature of this information may affect contributions. We hope that the research will shed light on the advantages and disadvantages of alternative disclosure policies, which we believe is a very important issue. If you are interested in receiving information about the results of the studies we are conducting, just send us a blank email to [link](#) and we will send information about our work as soon as our studies are finished. This is part of a strictly academic project, and our research is not affiliated with any candidate or political party. All the information that we used in our mailings is publicly accessible through the website of the Federal Election Commission (FEC). This website includes a search tool with which anyone can access information about individual contributions by donor name ([link](#)). This research team at Harvard includes Ricardo Perez-Truglia, a PhD student in Economics ([link](#)), and Dr. Guillermo Cruces ([link](#)). Mr. Perez-Truglia's primary thesis advisor is Professor Nadarajan Chetty. You may write to Ricardo Perez-Truglia directly at the above address and you may also reach his faculty thesis advisers by writing to [link](#).

All individuals who received a letter about campaign donations were randomly selected by an automated computer program from the public records of the FEC. The information provided in the letter was available from public records and was selected without regard to party affiliation. The FEC explicitly allows the use of information about individual campaign contributions for academic research such as this project. The specific activities identified as permissible by the Federal Election Commission include the use of individual contributor information for bona fide academic research projects that do not involve the sale or use of that information for a commercial purpose or for soliciting contributions (see FEC Advisory Opinion No. 1986-25). Our research project has no commercial or political objective and is in compliance with the rules regulating the use of contribution information. For more information, please see the FEC's "sale and use brochure" ([link](#)). This project was reviewed and approved in advance by Professor Chetty and by the Committee on the Use of Human Subjects in Research, a research ethics committee (also known as an "institutional review board" or "IRB") at Harvard University. Complaints or problems concerning any research project may, and should, be reported if they arise. The Committee can be reached via email ([link](#)) or by telephone ([link](#)).

Thank you again for your visit to this website and for your interest in our research.

*Ricardo Perez-Truglia and Guillermo Cruces (the research team)*

## C Further Details on Post-Election Mail-In Survey

We collected complementary information from a subsample of our subjects by means of a mail-in survey sent after the election. The survey was sent by mail on December 6, 2012, one month after the 2012 presidential election day. The intended recipients, 34,966 in total, were a random sample of individuals from the Control group (one third) and from the Website treatment group (two thirds). In order to maximize statistical power, we excluded from the survey sample those contributors whose largest pre-treatment contribution went to Ron Paul, because of their very low baseline rate of post-treatment contributions (below 2%). The survey instrument is presented in the following pages.

The envelope contained a letter and the survey on two sheets of paper, and a smaller prepaid business-reply envelope. The recipient was asked to fill out the survey and mail it back in the provided envelope by dropping it in a USPS mailbox. The individual could keep the separate letter, which contained details about the survey (e.g., confidentiality of the responses) as well as contact information for the research team.

We received 9,414 responses, which implies a response rate of 21.21%. Most of the respondents mailed back the completed survey over the two months following the delivery date of the mail-in survey. It should be noted that there were significant differences in response rates for key sub-groups of the population. Most notably, the response rate for Democrats, at about 27%, was substantially higher than that of Republicans, at about 12%. The information from the survey discussed in the paper thus over-represents Democrat contributors.

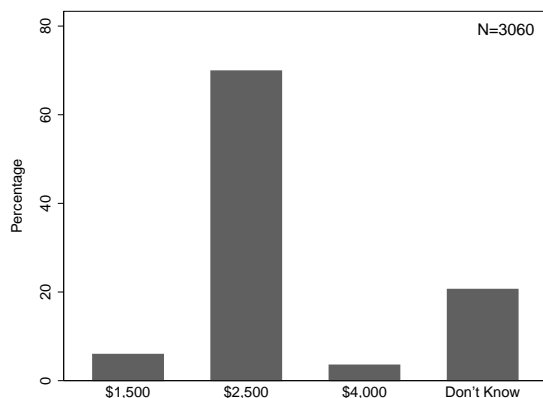
As an incentive for participation, the letter informed recipients that there were lottery prizes for individuals who responded and mailed back the survey before January 31, 2013. Half of the recipients were randomly assigned to be eligible for ten lottery prizes of \$100 each, while the other half were eligible for ten lottery prizes of \$200 each. The purpose of randomizing the stakes of the lottery was to provide some orthogonal variation in response rates that could be exploited to correct potential selection biases in the mail-in survey respondent pool. The response rate was only half a percentage point higher in the group eligible to the higher lottery prize, which implies an increase in the response rate of approximately 2.6%. This effect is relatively small and not statistically significant (p-value of 0.16). The fact that contributors did not react to this economic incentive probably reflects the fact that most of the recipients were relatively well-off. Nevertheless, we must note that the odds of receiving a prize were low, which could have resulted in very small differences in the perceived expected value of the prize. Given the small size of the effect of the lottery prize, it does not provide useful variation for the analysis.

As noted in the text of the paper, Figure 2.a shows that, among subjects in the control

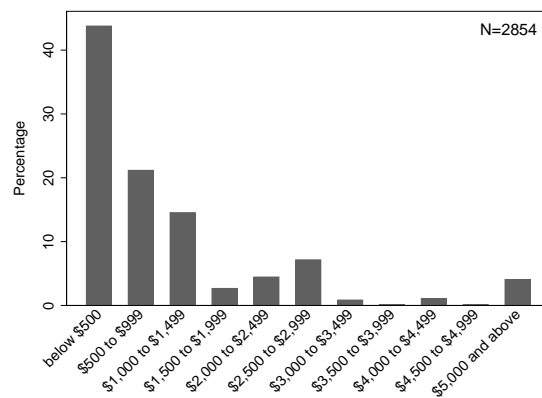
group, a large majority (86%) of respondents agreed with the statement that contribution records were a matter of public information, while the remaining 14% reported that they believed that the contribution records were confidential. The exact statement of this question was: *“The law says that the identity of contributors to political campaigns, the amounts contributed and the identity of recipients are all public information, and that information can be easily obtained by anyone with Internet access.”* Strictly speaking, some contribution records are a matter of public record while others are not (e.g., records for contributions of \$200 or less are not reported to the FEC). We chose a very general phrasing for the statements in the survey since we wanted to elicit a general awareness about the publicity of this information rather than specific knowledge about campaign finance regulations. Since the survey recipients made contributions over \$200, we expected them to agree with the statement that contributions are a matter of public records.

Figure C.1: Knowledge about Contribution Limits and Contribution Norms (Post-Election Mail-In Survey)

**a.** According to the law, what is the maximum contribution an individual can make to a campaign committee per election?



**b.** How much do you think a politically engaged individual with an average income should contribute to a presidential campaign per election cycle (every four years)?



Notes: The data corresponds to the responses to our post-election mail-in survey. The figures are based on responses from individuals in the experiment's Control group (i.e., individuals that did not receive any mailings). Panel a presents the frequency distribution for the four options on a question about the respondent's knowledge of the maximum contribution level per committee (see question 3 from the questionnaire in Appendix C). Panel b presents the distribution of responses to the survey question which asked recipients to state how much one "should" contribute to a presidential campaign (see question 8 from the questionnaire in Appendix C).

# C Survey Instrument

002\*01\*\*\*\*\*3-DIGIT 040  
JOHN DOE  
123 MAIN ST  
SPRINGFIELD, VA 22150-1234

Cambridge, November 10th 2012

Dear John Doe,

We are researchers from Harvard University who are carrying out a non-partisan study about campaign contributions in the United States. This study includes a short survey designed to find out how much citizens know about the rules and regulations applying to individual campaign contributions.

We need your help for this study. We kindly ask you to take two minutes of your time to fill out this short, confidential survey and send it back to us in the pre-paid and pre-addressed envelope enclosed with this letter. You do not need to affix a stamp to the envelope. You can simply drop the letter into any US Postal Service mailbox. Of course, your participation is completely voluntary.

As a token of our gratitude for helping us with our research, all of those responding to this survey and mailing it back to us will automatically enter a lottery for 10 prizes of \$100. Your chances in the lottery do not depend on your responses to the questions in the survey – Winners will be randomly chosen among all of those sending the survey back to us.

In the other side of this page you can find more information regarding the purpose of our research and the lottery prizes. You may keep this letter for your records – You do not need to send it back to us.

Sincerely,

**Ricardo Perez-Truglia and Guillermo Cruces**

The research team

Email: [rtruglia@fas.harvard.edu](mailto:rtruglia@fas.harvard.edu)

Address: Littauer Center G16R, 1805 Cambridge Street, Cambridge, Massachusetts 02138



## **Purpose of the study**

This is part of a strictly academic project, and our research is not affiliated with any candidate or political party. The survey includes questions about certain features of the campaign contribution laws and regulations. The purpose of our research project is to study the implications of those features. Your responses to this survey will be confidential – Your responses will not be shared with anyone under any circumstances. Our research project has no commercial or political objective and is in compliance with the rules regulating the use of contribution information. If you are interested in receiving information about the results of the studies we are conducting, just send an email to [rtruglia@fas.harvard.edu](mailto:rtruglia@fas.harvard.edu) with the subject line “Debriefing” and we will send you information about our work as soon as our studies are finished.

## **Terms of the lottery**

All the individuals who respond to the survey before January 31st 2013 (returned letters postmarked by that date) will be included in the lottery for the Amazon gift cards. Your odds of winning a prize will depend on the number of individuals who respond to the survey. For example, if - as expected - 1,500 individuals respond the survey, your chances of winning a prize will be of 1 in 150. Each individual can win a maximum of one (1) prize. If you are one of the winners, you will be notified by February 2013 by a letter sent to the same address where you received the survey. If you want us to use a different address, please state so in the space provided in the survey page.

**For more information about this project, please visit the project’s website**

[www.people.fas.harvard.edu/~rtruglia/campaign-survey.htm](http://www.people.fas.harvard.edu/~rtruglia/campaign-survey.htm)

**Thank you in advance for your cooperation!**

We are researchers from Harvard University who are carrying out a non-partisan study about campaign contributions in the United States. This is the short survey we mention in the accompanying letter. We kindly ask you to take two minutes of your time to fill out this short, confidential survey and send it back to us in the pre-paid and pre-addressed envelope enclosed with this letter. You do not need to affix a stamp to the envelope. You can simply drop the letter into any US Postal Service mailbox. Thank you for your cooperation.

**1- Please tell us your gender**

☐ Male ☐ Female

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**2- Your age (Please mark ONE option)**

☐ Under 20 ☐ 20-29 ☐ 30-39 ☐ Over 40

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**3- What is the maximum contribution that an individual can legally make to a campaign committee per election? (Please mark ONE option)**

☐ \$1,500 ☐ \$2,500 ☐ \$4,000 ☐ I don't know

---

**4- How accessible do you think that information on individuals' contributions to political campaigns is? If you do not know, please just give us your best guess. (Mark ONE option)**

☐ The law says that the identity of contributors to political campaigns, the amounts contributed and the identity of recipients are all **public** information, and that information can be easily obtained by anyone with Internet access

☐ The law says that the identity of contributors to political campaigns, the amounts contributed and the identity of recipients are all **confidential** information which cannot be accessed by third parties.

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**5- How sure are you about your answer to the previous question? (Mark ONE option)**

☐ Very sure ☐ Somewhat sure ☐ Unsure

**6- In your answers to the previous two questions, you told us what YOU believe. Now we want to know what you think that OTHERS believe. Think about people in your neighborhood. What do you think their best guess would be about how accessible the information on individual campaign contributions is? (Mark ONE option)**

☐ Most people would say that the identity of contributors to political campaigns, the amounts contributed and the identity of recipients are all **public** information.

☐ Most people would say that the identity of contributors to political campaigns, the amounts contributed and the identity of recipients are all **confidential** information.

---

**7- Would you say that... (Mark ONE option)**

☐ A **vast majority** of your neighbors believe that information on individual campaign contributions is **public**.

☐ A **majority** of your neighbors believe that information on individual campaign contributions is **public**.

☐ A **majority** of your neighbors believe that information on individual campaign contributions is **confidential**.

☐ A **vast majority** of your neighbors believe that information on individual campaign contributions is **confidential**.

---

**8- In your opinion, how much do you think a politically engaged individual with an average income should contribute to a presidential campaign per election cycle (every four years)?**

Please enter an amount:

\$

---

Respondent: John Doe (123 MAIN ST, SPRINGFIELD, VA 22150-1234)

**Thanks for your response!** We will contact you by mail if you win one of the lottery prizes. If needed, please write down an alternative address to receive any further correspondence about the prize:

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## D Further Results on the Scale Up of ITT Effects with the Post-Election Mail-In Survey

### E Scaled-Up Effects

As discussed previously, a substantial share of experimental subjects - most likely a majority - may not have read the letters we sent to them. Some individuals may have not opened the letters (the EPA estimates that almost half of the unsolicited mailing like ours is discarded without even being opened). Our estimates thus measure the intention to treat effects (ITT) of sending these letters. The magnitude of the ITT may be relevant in some contexts, such as fundraisers who want to design mailings to raise more contributions. However, the economic question motivating our analysis is the extent and magnitude of these social incentives, and the impact of the letters are only instruments to uncover these mechanisms. Thus, measuring the effect of reading the letter would give a more accurate intuition of the magnitude of the social mechanisms at play.

The treatment effect on the treated (i.e., on the individuals who read the letter) is a multiple of the intention to treat effect. The key parameter mediating between these two estimates is the reading rate,  $r$  (i.e., the proportion of the experimental subjects who read the letters). The intention to treat effects (ITT) can be scaled up into treatment effects on the treated (TOT) using the inverse of the reading rate:  $TOT = \frac{1}{r}ITT$ . For a given magnitude of this social incentive, an ITT effect of a 5% increase of the probability to contribute represents a moderate effect if all recipients read the letter, but it would represent a very large effect with a substantially lower reading rate (say, around 20%).

Our mailing was sent in the middle of the presidential campaign, when potential voters were receiving on a daily basis large amounts of unsolicited physical and electronic mailings soliciting campaign contributions and providing information about the candidates and the election. Even though we made efforts for our flyer to stand out, so were the candidates' campaign committees, and our letters had a high probability of being discarded unread. This scale-up factor between ITT and TOT effects played a key role in the power calculation analysis previous to our experiment. Intuitively, if we assumed a reading rate half as high as the one we selected, we would have needed to send twice as many letters for a given underlying effect size. The EPA estimates that about half of unsolicited mailings are discarded without being read, but given the context of the campaign we used more conservative estimates. Consultations with mass-marketing experts provided estimates of reading rates ranging from 10% to 25% in these specific conditions, and we used 15% for our power calculations, which explains why our field experiment involved such an unusually large number of subjects.

However, the analysis of the results would benefit from an objective and precise measurement of this reading rate, and this was one of the goals of our post-election mail-in survey (see Section 3.4 for details).

## E.1 Estimates of the Reading Rate

The results from the mail-in survey provide different alternatives to estimate the reading rate  $r$ . Since the main information provided by our Website letters was the publicity of individual campaign contribution records, we could expect that an individual who read our letter should have a higher awareness about the public nature of these records. Figure 4 presents the distribution of beliefs about the publicity of contribution data for survey respondents from the Control and the Website treatment groups. The Website letter does not seem to have modified the perception of those for respondents who reported to be very sure or somewhat sure that the contribution information is confidential. The fact that about 10% of respondents report to be somewhat sure or very sure that contribution records are confidential may reflect true unawareness about the FEC disclosure policy, or alternatively some individuals may believe contributions are not public. For instance, they may believe that they are not public because SuperPACs can be used to make veiled contributions, or because of the limit of \$200 for the disclosure of individual contributions which implies that low total contributions are not public. It is also possible, although unlikely, that other recipients read our letter but forgot the information it contained when responding to our survey several months later.

However, we can expect a larger effect for those who reported to be unsure about the public nature of contribution records. Section 4.3 presents an estimate based on the effect of the information in our treatment, with an estimate of the implicit reading rate of  $r = 0.215$  (with a 90% confidence interval between 0.146 and 0.284), and a scale-up factor of 4.6. This estimate originates in the assumption that the effect of the Website letter was to reduce the proportion of those being unsure about their answers. Alternatively, we could assume that the impact of the Website letter was to decrease the proportion of those who did not know that contribution records were public: i.e., to shift individuals from any category to being somewhat sure or very sure about the publicity of contribution records. The share respondents who did not select any of these two categories was 25.6% in the Control group and 21.2% in the Website group. The difference between the two results in an implicit reading rate of  $r = 0.171$  and a scale up factor of 5.8. Thus, if anything, this alternative model leads to a higher scale-up factor. The estimate in the body of the paper and this alternative estimate are both within the range provided by the mass-mailing experts which we used for

our power calculations.

It should be noted that the mail-in survey's response rate, 21.2%, was relatively high, which may suggest that the reading rate for the original treatment letters was close to that figure or higher. However, the conditions of the survey mailing were very different than the conditions of the experimental mailing. The mail-in survey was sent in a closed envelope, whereas the experimental mailpiece consisted of a single sheet of paper that folded and sealed to make letter-sized mailpiece, and the latter design is more likely to be discarded unopened. Moreover, contributors probably received much more unsolicited physical and electronic mailing related to the election at the time we sent the experimental mailpieces, in the middle of the presidential campaigns, than when we sent the envelope with the mail-in survey, a month after the election. This also implies that the mail-in survey envelope was less likely to be discarded as junk mail than the mailpieces corresponding to the experiment. Finally, the high response rate to the survey was driven mostly by Democrat contributors, whose candidate had won about a month earlier and who had thus a more positive reaction to matters related to the election.

## E.2 Qualification of the Main Results

The discussion in the body of the paper indicated large scaled-up effects. We can, of course, qualify the results obtained with these simple estimates of the reading rate. A first concern is that the Website letter may have induced a lower willingness to participate in the mail-in survey, which could lead to an under-estimation of the reading rate. However, the mail-in survey response rate was 21.0% for subjects in the control group and 21.3% for recipients of the Website letter, and this 0.37 percentage points difference is not only very small but also statistically insignificant (p-value of 0.357).<sup>36</sup>

A further concern is that the mail-in survey sample is not representative of the subject pool, so that the estimated reading rate may not be representative of the reading rate in the subject pool. However, we can expect that individuals who were more likely to read our letter were also more likely to respond to our survey, for instance, those who have more free time. This type of bias implies an over-estimation of the reading rate and, in turn, an under-estimation of the scaled-up effects.

Finally, our estimate of the reading rate is based on the assumption that all of the recipients of the Website letter who were unsure about the publicity of individual contributions went on to report that this information is public in the mail-in survey. Some of these recipients, however, might not have been induced to state that these records are public even after

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<sup>36</sup>We are implicitly assuming that those who were sent a letter but did not read it had the same reaction to the mail-in survey as those who were not sent any letter.

reading the letters. This implies that we could be under-estimating the actual reading rate. In any case, even with a reading rate half as large (and a scale-up factor half the size) as in our estimates, the signaling effects would still imply large changes in contribution behavior.

## F Snapshots of the FEC Website's Search Tool

The FEC provides an easily accessible online database of individual campaign contributions. The database can be searched by first and/or last name:

### Transaction Query By Individual Contributor

Search Using the Following Criteria:

Individual Name (*Last, First*):

Get Listing

Clear Form

Advanced search can be done by other criteria, such as city, state, date range, and so forth:

### Advanced Transaction Query By Individual Contributor

Search From the Following Criteria:

Individual Name (*Last, First*):

City:

State (Select as many as appropriate):

ALL  
ALABAMA  
ALASKA  
AMERICAN SAMOA

Zipcode:

Employer/Occupation:

Amount Range:

From  To

**NOTE: Currently the 'Date Range' field must be used with at least one other constraint**

Date Range:

From  To  Format: MM/DD/YYYY

Type:

☐ Federal Contributions ☐ Non-Federal (Soft Money) ☒ All Contributions

Send Query

Clear Form



This is a sample of how the search results are displayed (they are the same for basic and advanced search). This sample is for one transaction - the search tool displays one record per transaction:

## Individual Contributions Arranged By Type, Giver, Then Recipient

### Contributions to Political Committees

**DOE, JOHN**

ELIOT, ME 03903

HOMEMAKER

[PAUL, RON](#)

[VIA RON PAUL 2012 PRESIDENTIAL CAMPAIGN COMMITTEE INC.](#)

12/16/2011

250.00

[12345678900](#)

The (fake) number [12345678900](#) has a link to the exact page of the Schedule A-P corresponding to the transaction. The following is a sample:

SCHEDULE A-P ITEMIZED RECEIPTS			Use separate schedule(s) for each category of the Detailed Summary Page	FOR LINE NUMBER: (check only one)	PAGE 9410 / 17901
				<input type="checkbox"/> 16 <input checked="" type="checkbox"/> 17a <input type="checkbox"/> 17b <input type="checkbox"/> 17c <input type="checkbox"/> 17d <input type="checkbox"/> 18 <input type="checkbox"/> 19a <input type="checkbox"/> 19b <input type="checkbox"/> 20a <input type="checkbox"/> 20b <input type="checkbox"/> 20c <input type="checkbox"/> 21	
Any information copied from such Reports and Statements may not be sold or used by any person for the purpose of soliciting contributions or for commercial purposes, other than using the name and address of any political committee to solicit contributions from such committee.					
NAME OF COMMITTEE (In Full) Ron Paul 2012 Presidential Campaign Committee Inc.					
<b>A. Full Name (Last, First, Middle Initial)</b> Doe, John				<b>Transaction ID : 0992651</b>	
Mailing Address 123 Fake Street				Date of Receipt M M / D D / Y Y Y Y Y Y 12 / 16 / 2011	
City Eliot		State ME	Zip Code 03903		
FEC ID number of contributing federal political committee.			C		
Name of Employer Homemaker		Occupation Homemaker		Amount of Each Receipt this Period 250.00	
Receipt For: 2012 <input checked="" type="checkbox"/> Primary <input type="checkbox"/> General <input type="checkbox"/> Other (specify) ▼		Election Cycle-to-Date ▼ 500.00			

## G Further Details on the Field Experiment’s Implementation

### G.1 Experimental Sample and Subject Pool

A total of 280,456 unique individuals were listed as having made a contribution to a presidential candidate in that time period in the FEC records. This sample was obtained from the FEC’s public records as of April 25, 2012, which includes contributions made until April 1 of that year. This sample of contributors, by definition, excludes individuals contributing \$200 or less over the course of the election cycle, as these individuals are not required to be reported to the FEC. While campaigns have increasingly relied on these donors (they represented 41.2% of all individual contributions in 2008 and 47.7% in 2012), the available evidence indicates that, besides the evident differences in income, those making small and large contributions are fairly similar.<sup>37</sup>

We discarded a substantial fraction of the original 280,456 contributors for data quality and other reasons. This selection resulted in a final subject pool of 191,832 individuals. The individuals were excluded from the experimental subject pool because they did not satisfy our requirements for data quality. We present here a list of the most important reasons and criteria. We cannot report what percentage of individuals were excluded for each reason because a majority of the excluded individuals did not satisfy multiple criteria. We geocoded all the addresses and excluded observations for which the address information was invalid and could not be corrected (e.g., missing street number). We also excluded individuals reporting addresses used by more than two unique individuals (which most likely corresponds to work addresses) and individuals who provided P.O. boxes as their home address. We matched the address information to the NCOA database to identify individuals or households that changed residence over the previous 18 months. We excluded all individuals who changed residence since the date when they made their first contribution during the election cycle, individuals who presented inconsistencies in the information reported for different contribution records (for example, reporting multiple addresses), individuals whose mean distance (as the crow flies) from the ten closest contributors is over three miles, individuals who had already made a total contribution over \$1,500, and all contributors living in Washington D.C., or outside the 50 States.

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<sup>37</sup>McAdams, J. and Green, J.C. (2002). “Fat Cats and Thin Kittens: Are People Who Make Large Campaign Contributions Different?”, CATO Institute Briefing Paper no. 76.

## G.2 Mailing Delivery

The date of delivery of each letter is an important factor to consider when determining exposure to our information treatment. We were able to track the delivery status of each letter through the USPS scanning system, which does not confirm delivery but tracks when and where each letter was last scanned. We generated a proxy for time of delivery equal to the most recent date when the letter was scanned if it was not forwarded or returned. For letters with incomplete tracking information, we imputed delivery information from other mail pieces in our batch delivered in the same 9-digit ZIP code. While the USPS tracking data is not a perfect indicator of delivery, it provides good approximation. Most of the reports correspond to the date when the letter was “out for delivery”, and it is safe to assume that those letters were delivered the same day or in the following few days. However, for about 18% of the letters the reported date corresponds to the time when they were last scanned in a processing facility, and in those cases the delivery may happen several days later. Therefore, our proxy is a conservative lower bound for the actual date of delivery. Again, proxy of delivery does not necessarily indicate letters were received or read, as the mailing did not include delivery confirmation service, and moreover, as noted previously, the letters may have been discarded by the recipient along with other unsolicited mail..

A more ideal experiment would include confirmation that the intended recipient actually read the letter sent. In place of this, we constructed an aggregate proxy for the distribution of the dates when the letters were read. A link to a website was included in each letter, with contact information for the research team and Harvard’s Institutional Review Board. The website records indicate the number and date of visits. It is likely that individuals visited the website on the same day that they read the letter, or at least within the next few days. The distribution of visits to the website over time thus provides a proxy for the time when the individuals read the letters.

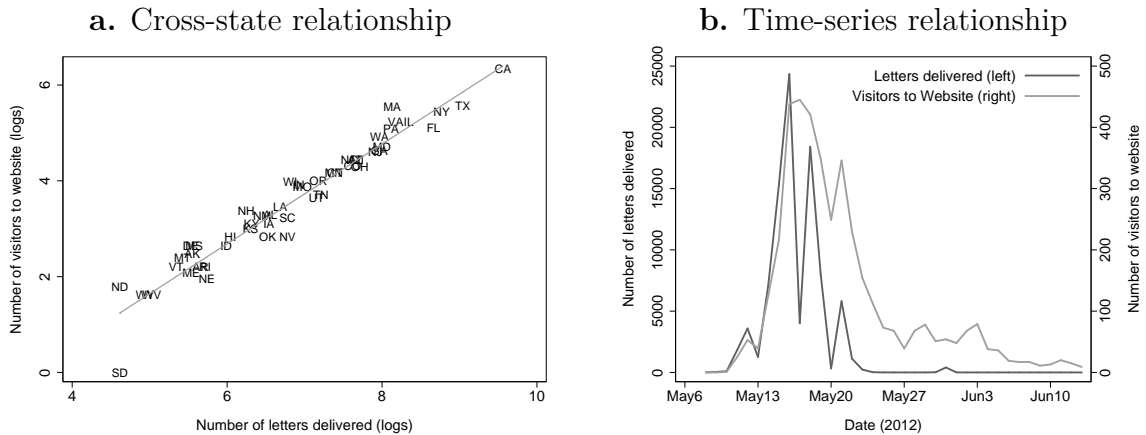
Figure E.1 compares our proxy of delivery date from the USPS tracking data with data on visits to the project’s website. Figure E.1.a indicates that the number of letters in each State was almost exactly proportional to the number of unique visitors to the website – the R-squared for the regression line in the Figure is 0.98. This strong correlation indicates that the proxies for letter delivery and letters read are consistent. Figure E.1.b shows the distribution of new visitors to the website over time and the USPS-based proxy for mail delivery. The two distributions are very similar, although visits to the website seem to have a three to four day lag with respect to the proxy for delivery date. This is consistent with the fact that individuals do not necessarily read the mail the same day they get it. Moreover, the difference in the right tail of the two distributions indicates that visits to the website sometimes occurred weeks after the letters were delivered. This probably corresponds to

individuals who accumulate mail over time, or to those who were absent from their homes for some time. However, the difference between the two distributions may simply correspond to the fact that our proxy for delivery is only a conservative lower bound estimate of the actual date of delivery.

### G.3 Feedback from Recipients

The project’s website included contact information for the research team and for Harvard’s IRB, which could be used by the experimental subjects to address their questions and potential concerns about our research. A minuscule share of individuals contacted us with concerns. We responded personally to every individual that contacted us under the supervision of Harvard’s IRB, following a detailed pre-specified protocol. Less than 0.1% of the subjects in the sample were deleted from our database (preventively or as per their request), and thus we do not use information about them for our study. A number of individuals contacted us for other reasons-some wanted to express their interest in our study. The project’s webpage also offered the option to subscribe to a mailing list to receive a non-technical brochure detailing the study’s main findings. In order to avoid contaminating the sample, the brochures were sent once the election cycle and the post-election survey were over.

Figure E.1: Relationship Between the Mailing Delivery Indicator and the Number of Visits to the Project’s Website



Notes: date of delivery provided by USPS. Number of visitors to the website includes unique visitors that reached the website directly (approximately 83% of the visits) or indirectly through a search engine (in virtually all cases after searching for “www.campaign-information.info” or “campaign-information.info”).

## H Summary Statistics for Informational Treatments

Table F.1: Informational Effects: Summary Statistics

	Mean	Standard Deviation	Minimum	Maximum
$\bar{c}_{own}$	6.33	4.88	0.00	25.00
$\bar{c}_{opp}$	5.92	5.42	0.00	25.00
$N_{own}$	5.31	2.22	0.00	9.00
$\sum c_{own}$	35.28	32.78	0.00	225.00
$\sum c_{own} - \sum c_{opp}$	11.07	48.97	-220.00	225.00
$ \sum c_{own} - \sum c_{opp} $	36.70	34.25	0.00	225.00

Notes:  $N = 36,795$ . Summary statistics corresponding to the sample of individuals assigned to the List treatment.  $\bar{c}_{own}$  ( $\bar{c}_{opp}$ ) corresponds to the average contribution among all own-party (opposite-party) contributors in the baseline list, expressed in hundreds of dollars.  $N_{own}$  is the number of own-party contributors in the baseline list.  $\sum \bar{c}_{own}$  ( $\sum \bar{c}_{opp}$ ) is defined as  $\bar{c}_{own}$  ( $\bar{c}_{opp}$ ) but refers to the sum of contributions instead of the average contribution (also expressed in hundreds of dollars).

# I A Signaling Model of Campaign Contributions

## I.1 The Model

The following is a simple model of campaign contributions that captures the party signaling described in Section 4. In this model, individuals, indexed by subscript  $i$ , can make a discrete contribution denoted by  $c_i \in \{-1, 0, 1\}$ .  $c_i = -1$  means that the individual contributes to the left-wing party,  $c_i = 1$  means that the individual contributes to the right-wing party and  $c_i = 0$  means that the individual does not contribute to any political party. The discrete nature of contributions is just a convenient simplification to facilitate the tractability of the model. The intuition of the model, however, extends to the case where individuals can make contributions of different amounts. Moreover, even though we are interested specifically in monetary contributions,  $c_i$  may also be interpreted as other forms of potentially-observable forms of political participation, such as attending a rally, displaying candidate's yard signs or simply speaking in favor of a candidate.

The individuals belong to reference groups. We will refer to other individuals in  $i$ 's reference group as  $i$ 's neighbors. In the empirical analysis, we rely on a geographic proxy for an individual's reference group, but these may represent something more general than just geographic vicinity. They may include family members, friends, acquaintances and co-workers, for instance. A given reference group is comprised by a continuum of agents who differ in a parameter  $\alpha_i$ , distributed in the support  $[\underline{\alpha}, \bar{\alpha}]$  according to the cumulative distribution function  $F_\alpha(\cdot)$ , with  $\underline{\alpha} < 0$  and  $\bar{\alpha} > 0$ . The parameter  $\alpha_i$  indicates the party supported and the strength of  $i$ 's political affiliation. Individuals with  $\alpha_i < 0$  sympathize with the left-wing party and those with  $\alpha_i > 0$  sympathize with the right-wing party. Thus,  $S_R = F_\alpha(0)$  and  $S_L = 1 - F_\alpha(0)$  are the shares of individuals supporting the left and right parties, respectively. Agent  $i$ 's utility from contributing to her favorite party is given by  $-K + |\alpha_i|$ , and her utility from contributing to the opposite party is  $-K - |\alpha_i|$ . The parameter  $K > 0$  represents the fixed cost of contributing, including both pecuniary and non-pecuniary costs. If only these costs were present, individuals with  $\alpha_i < -K$  would contribute to the left-wing party, individuals with  $\alpha_i > K$  would contribute to the right-wing party, and individuals with  $-K < \alpha_i < K$  would refrain from making any contribution.

There are also indirect costs and benefits from making contributions. Individuals interact with neighbors in their reference group. The utility an individual can expect from these interactions is a function of the coincidence or divergence in political affiliations with the neighbors. Political preferences are not directly observable by others, but contributions may be observed. Whether the individual contributed, and the party contributed to, is visible to  $i$ 's

neighbors with some probability  $v$ , and unobservable with probability  $1 - v$ .<sup>38</sup> Contributions are made prior to the interactions with neighbors.<sup>39</sup> When the contribution is observable, a neighbor can infer the individual's political preference - in a probabilistically sense - from the observed contribution (or lack thereof). Let  $P_i^j = P^j(c_i, c_{-i})$  be the perceived probability that  $i$  sympathizes with party  $j$  given  $i$ 's contribution,  $c_i$ , and the vector of everyone else's contributions in the same reference group,  $c_{-i}$ . The utility from the interaction with a neighbor of party  $j$  is  $\delta(P_i^j)$ . The function  $\delta(\cdot)$  is monotonically increasing, which means that neighbors treat individuals better when they believe that they support their own political party.

Denote  $P_i^R$  the perceived probability that individual  $i$  sympathizes with the right-wing party. When her contribution is observable to neighbors, the indirect utility for a right-wing individual is given by:

$$S_R \mu \delta(P_i^R) + S_L (1 - \mu) \delta(1 - P_i^R)$$

This is a weighted average of the expected utilities from interacting with right-wing and left-wing neighbors, where the weights are given by the parameter  $\mu$  in conjunction with the proportion of neighbors sympathizing with each party,  $S_R$  and  $S_L$ . Similarly, the indirect utility for a left-wing individual is given by:

$$S_R (1 - \mu) \delta(P_i^R) + S_L \mu \delta(1 - P_i^R)$$

The parameter  $\mu \in [\frac{1}{2}, 1]$  captures what we denominate political homophily, the tendency of individuals to associate with other sympathizers of the same political party. The parameter  $\mu$  can have one of the following two interpretations (or a combination of the two). First, it may represent differences in the likelihood of meeting a neighbor of each party. Second, it may represent party-based differences in how individuals value the interactions. The case where  $\mu = \frac{1}{2}$  is given by a situation where individuals are matched with neighbors regardless of their political preferences, while in the case where  $\mu > \frac{1}{2}$  each individual is relatively more likely to interact with neighbors supporting their own political party.<sup>40</sup> Alternatively,  $\mu = \frac{1}{2}$

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<sup>38</sup>An alternative interpretation of the probability parameter  $v$  is that the contribution information is always a matter of public record, but each individual is uncertain as to whether her neighbors know about these records or about their publicity, and, if they know about it, whether they would try to access this information.

<sup>39</sup>Note that the individual does not know whether her contribution will be observable to others when deciding about her contributions.

<sup>40</sup>However, the fact that individuals are more likely to bond with neighbors of the same political party should not be interpreted as an exogenous parameter, i.e.,  $\mu > \frac{1}{2}$ , but instead as part of the indirect costs embedded in  $\delta(\cdot)$ . That is, revealing oneself as a sympathizer of the opposite party (with respect to the neighbors) is disadvantageous because this results in fewer and/or poorer connections within the reference group.

could mean that individuals have the same valuation of interactions with neighbors from either party, while  $\mu > \frac{1}{2}$  could indicate that individuals value interactions with same-party neighbors relatively more.

We make two simplifying assumptions to make the model tractable. First, we assume  $\alpha_i$  is uniformly distributed. Second, we assume that  $\delta(\cdot)$  is linear. Without any loss of generality, we normalize the intercept of  $\delta(\cdot)$  to zero: i.e.,  $\delta(P) = \gamma \cdot P$ . In the signaling equilibrium there will be three groups defined by two thresholds:  $\alpha_L^* \in (\underline{\alpha}, 0)$  and  $\alpha_R^* \in (0, \bar{\alpha})$ .<sup>41</sup> Individuals with  $\alpha_i \leq \alpha_L^*$  will contribute to the left-wing party, individuals with  $\alpha_L^* < \alpha_i < \alpha_R^*$  will not contribute at all, and individuals with  $\alpha_i \geq \alpha_R^*$  will contribute to the right-wing party. Let  $\Omega_R$  ( $\Omega_L$ ) denote a right-wing (left-wing) individual's utility from interacting with neighbors when her own contribution is unobservable. The utility for a right-wing individual from contributing to her favorite party is:

$$-K + \alpha_i + v S_R \mu \gamma + (1 - v) \Omega_R$$

The utility for a left-wing individual from contributing to her favorite party is:

$$-K - \alpha_i + v (1 - S_R) \mu \gamma + (1 - v) \Omega_L$$

The utility of not contributing for a right-wing individual is:

$$v \left[ (S_R - 1 + \mu) \gamma \frac{\min \{\alpha_R^*, \bar{\alpha}\}}{\min \{\alpha_R^*, \bar{\alpha}\} - \max \{\alpha_L^*, \underline{\alpha}\}} + (1 - S_R) (1 - \mu) \gamma \right] + (1 - v) \Omega_R$$

The utility of not contributing for a left-wing individual is:

$$v \left[ (S_R - \mu) \gamma \frac{\min \{\alpha_R^*, \bar{\alpha}\}}{\min \{\alpha_R^*, \bar{\alpha}\} - \max \{\alpha_L^*, \underline{\alpha}\}} + (1 - S_R) \mu \gamma \right] + (1 - v) \Omega_L$$

By construction,  $\alpha_R^*$  is such that a right-wing individual with  $\alpha_i = \alpha_R^*$  is indifferent between contributing to the right-wing party and not contributing at all:

$$\alpha_R^* = v \gamma (S_R - 1 + \mu) \frac{\min \{\alpha_L^*, \bar{\alpha}\}}{\min \{\alpha_R^*, \bar{\alpha}\} - \max \{\alpha_L^*, \underline{\alpha}\}} + K \quad (2)$$

The analogous expression for a left-wing individuals is:

$$-\alpha_L^* = v \gamma (S_R - \mu) \frac{\min \{\alpha_R^*, \bar{\alpha}\}}{\min \{\alpha_R^*, \bar{\alpha}\} - \max \{\alpha_L^*, \underline{\alpha}\}} + K \quad (3)$$

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<sup>41</sup>Note that we implicitly assume an interior solution.



This system of two equations and two unknowns characterizes the signaling equilibrium.

Denote  $\alpha^* = \{\alpha_L^*, \alpha_R^*\}$  and let  $\Theta = \left\{ \alpha^* : \alpha_L^* \in \left( \underline{\alpha}, -\frac{K}{2} \right), \alpha_R^* \in \left( \frac{K}{2}, \bar{\alpha} \right) \right\}$ . We will focus on equilibria with  $\alpha^* \in \Theta$ . The first requirement in  $\Theta$  is that the solution is interior, i.e.,  $\underline{\alpha} < \alpha_L^* < \alpha_R^* < \bar{\alpha}$ .<sup>42</sup> The second requirement,  $\alpha_L^* < -\frac{K}{2} < 0 < \frac{K}{2} < \alpha_R^*$ , basically restricts the analysis to equilibria in which the mass of non-contributors to each party is above the threshold  $\frac{K}{2}$ . This condition is consistent with the fact that only a small share of individuals contribute to political campaigns. This condition guarantees that the equilibrium effects described below are of second order and therefore do not override the direct effects of changes in the relevant parameters.<sup>43</sup>

**Proposition 1.** *Given parameter values in a non-empty set  $\Pi$ , a signaling equilibrium exists, it is unique and it belongs to  $\Theta$ .*

Proofs of the propositions are provided at the end of this Appendix. While we cannot specify an explicit solution for the model, we can use the implicit function theorem to perform the key comparative statics. The following proposition presents a prediction about the effect of visibility on contributions relevant for the empirical analysis.

**Proposition 2.** *In any signaling equilibrium from  $\Theta$ , an increase in visibility ( $v$ ) induces a change in the number of contributors to the majority party that is greater than the change in the number of contributors to the minority party.*

If there are more neighbors identified with an individual's party, she will have greater incentives to signal her political preference by making a contribution to that party. In terms of the empirical application presented in this paper, the proposition implies that an exogenous variation in  $v$  should result in a very specific form of heterogeneous effects: the effect of changes in visibility on contributions should be increasing in the share of neighbors supporting the same party as the contributor.

**Proposition 3.** *In any signaling equilibrium from  $\Theta$ , an increase in visibility ( $v$ ) induces a change in the number of contributors to party  $j$  that is positive if  $S_j > 1 - \mu$ , null if  $S_j = 1 - \mu$ , and negative if  $S_j < 1 - \mu$ .*

We should expect changes in  $v$  to have effects of opposite signs on contributions for individuals in two different groups: we should expect a negative effect for those with  $S_j <$

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<sup>42</sup>It would be straightforward to extend the Propositions to the alternative scenario, although the notation would be significantly more complicated. Intuitively, we would need to reproduce the whole analysis for each corner solution.

<sup>43</sup>Even though the propositions focus on the more plausible equilibria in  $\Theta$  (given the fraction of contributors in the actual population), it is straightforward to extend the comparative statics for  $\alpha^* \notin \Theta$  based on the proofs provided here.

$1 - \mu$ , and a positive effect for individuals with  $S_j > 1 - \mu$ . For example, if  $\mu = \frac{1}{2}$ , which denotes a pattern of interactions with neighbors independent of their political preferences, we should expect an exogenous increase in  $v$  to increase contributions for individuals who belong to the majority party in the area, and a reduction in contributions for those identified with the minority party. As a result, an increase in  $v$  in a given reference group should result in one of two scenarios. If  $S_j > \mu$  (so that  $S_j > 1 - \mu$  and  $1 - S_j < 1 - \mu$ ), greater visibility will increase contributions to the majority party but reduce those to the minority party. Alternatively, if  $1 - \mu < S_j < \mu$  (so that  $S_j > 1 - \mu$  and  $1 - S_j > 1 - \mu$ ), then an increase in visibility will increase contributions to both parties, but (because of Proposition 2) the increase will be greater for the majority party.<sup>44</sup>

Finally, the results also provide a more intuitive interpretation for the condition  $\alpha_L^* < -\frac{K}{2} < 0 < \frac{K}{2} < \alpha_R^*$ . Changes in visibility have both direct and equilibrium effects on contributions. The direct effect is that, holding constant all other agent's contribution patterns, greater visibility makes contributions to a given party either more or less attractive, depending on whether  $S_j$  is lower or higher than  $1 - \mu$ . For example, if  $\mu = \frac{1}{2}$  then an increase in visibility makes contributions more attractive for the sympathizers of the majority party and less attractive for the sympathizers of the minority party. The equilibrium effect, in turn, results from the fact that other individuals should also react to the change in  $v$ , thereby altering the political composition of the pool of non-contributors. For example, if as a result of a change in  $v$  contributions by individuals identified with the majority party increase, the signal of making no contributions would become more closely associated to being sympathetic to the minority party, thereby changing the value of not making a contribution. When the share of non-contributors is large enough, i.e.  $\alpha_L^* < -\frac{K}{2} < 0 < \frac{K}{2} < \alpha_R^*$ , these equilibrium effects are of second order, so the net effect is dominated by the direct effects. However, if the share of non-contributors is very low, then the equilibrium effects may override the direct effects and change the sign of the overall impact of the change in visibility. We only discuss the comparative statics under the more realistic condition where only a small share of individuals make campaign contributions, although it is straightforward to derive predictions under alternative scenarios.

Last, if we define geographic polarization as the difference in contributions to the two parties within a given reference group, the following Corollary is implied by Proposition 2:

**Corollary 1.** *In any signaling equilibrium from  $\Theta$ , an increase in visibility of contributions ( $v$ ) will increase geographic polarization.*

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<sup>44</sup>If we allowed the scenario with  $\mu < 1/2$  then a fourth possibility would arise: if  $S_j < 1 - \mu$  and  $1 - S_j < 1 - \mu$ , an increase in visibility decreases contributions to both parties in the reference group, although the fall would be milder for the individuals of the majority party.

## I.2 Proof of Propositions

### I.2.1 Proof of Proposition 1

From Equation 3 we obtain:

$$\alpha_L^{\star 2} + \alpha_L^{\star} [K - \alpha_R^{\star}] - (v\gamma (S_R - \mu) + K) \alpha_R^{\star} = 0$$

Using the quadratic formula:

$$\alpha_L^{\star} = \frac{\alpha_R^{\star}}{2} - \frac{K}{2} - \sqrt{\left(\frac{\alpha_R^{\star}}{2} - \frac{K}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R^{\star}}$$

We only use the left root because the right root cannot have simultaneously  $\alpha_L^{\star} < -\frac{K}{2}$  and  $\alpha_R^{\star} > 0$ . Note that we also need  $(v\gamma (S_R - \mu) + K) > 0$ , which implies that  $\alpha_L^{\star} < 0$ . We can replace in Equation 2:

$$\alpha_R^{\star} = v\gamma (S_R - 1 + \mu) \frac{-\frac{K}{2} + \frac{\alpha_R^{\star}}{2} - \sqrt{\left(\frac{K}{2} - \frac{\alpha_R^{\star}}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R^{\star}}}{\alpha_R^{\star} - \left[-\frac{K}{2} + \frac{\alpha_R^{\star}}{2} - \sqrt{\left(\frac{K}{2} - \frac{\alpha_R^{\star}}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R^{\star}}\right]} + K$$

and then define:

$$f_R(\alpha_R) = v\gamma (S_R - 1 + \mu) \frac{-\frac{K}{2} + \frac{\alpha_R}{2} - \sqrt{\left(\frac{K}{2} - \frac{\alpha_R}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R}}{\alpha_R - \left[-\frac{K}{2} + \frac{\alpha_R}{2} - \sqrt{\left(\frac{K}{2} - \frac{\alpha_R}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R}\right]} + K$$

We thus need to prove that a fixed point of  $f_R(\alpha_R)$  exists and is unique in the domain  $\alpha_R \in \left(\frac{K}{2}, \bar{\alpha}\right)$ . Define  $g_R(\alpha_R) = f_R(\alpha_R) - \alpha_R$ . First we need to prove that  $g'_R(\alpha_R) > 0$ . Given that, we would only need to find conditions such that  $g_R\left(\frac{K}{2}\right) > 0$  and  $g_R(\bar{\alpha}) < 0$  to prove existence and uniqueness. Starting with  $g'_R(\alpha_R)$ :

$$g'_R(\alpha_R) = \frac{\left[-\frac{K}{2} + \frac{\alpha_R}{2} - \sqrt{\left(\frac{K}{2} - \frac{\alpha_R}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R}\right]}{\left(\alpha_R - \left[-\frac{K}{2} + \frac{\alpha_R}{2} - \sqrt{\left(\frac{K}{2} - \frac{\alpha_R}{2}\right)^2 + (v\gamma (S_R - \mu) + K) \alpha_R}\right]\right)^2 - 1}$$

To prove that  $g'_R(\alpha_R) < 0$ , it is sufficient that:

$$\frac{\alpha_R}{2} - \frac{K}{2} < \sqrt{\left(\frac{\alpha_R}{2} - \frac{K}{2}\right)^2 + (v\gamma(S_R - \mu) + K)\alpha_R}$$

If  $\frac{\alpha_R}{2} - \frac{K}{2} < 0$ , this condition is automatically satisfied. If  $\frac{\alpha_R}{2} - \frac{K}{2} > 0$ , we must have  $(v\gamma(S_R - \mu) + K) > 0$ , which we already had to assume. We must then find conditions such that  $g_R\left(\frac{K}{2}\right) > 0$  and  $g_R(\bar{\alpha}) < 0$ , where:

$$g_R\left(\frac{K}{2}\right) = v\gamma(S_R - 1 + \mu) \frac{-\frac{1}{4}K - \sqrt{\frac{9}{16}K^2 + v\gamma(S_R - \mu)\frac{K}{2}}}{\frac{3}{4}K + \sqrt{\frac{9}{16}K^2 + v\gamma(S_R - \mu)\frac{K}{2}}} + \frac{K}{2}$$

$$g_R(\bar{\alpha}) = v\gamma(S_R - 1 + \mu) \frac{-\frac{K}{2} + \frac{\bar{\alpha}}{2} - \sqrt{\left(\frac{K}{2} - \frac{\bar{\alpha}}{2}\right)^2 + (v\gamma(S_R - \mu) + K)\bar{\alpha}}}{\alpha_R - \left[-\frac{K}{2} + \frac{\bar{\alpha}}{2} - \sqrt{\left(\frac{K}{2} - \frac{\bar{\alpha}}{2}\right)^2 + (v\gamma(S_R - \mu) + K)\bar{\alpha}}\right]} + K - \bar{\alpha}$$

We now need to reproduce the entire analysis for  $\alpha_L$ : i.e., we need to prove that a fixed point of  $f_L(\alpha_L)$  exists and is unique in the domain  $\alpha_L \in \left(\underline{\alpha}, -\frac{K}{2}\right)$ . From Equation 2 we obtain:

$$\alpha_R^* = \frac{K}{2} + \frac{\alpha_L^*}{2} + \sqrt{\left(\frac{K}{2} + \frac{\alpha_L^*}{2}\right)^2 - (K - v\gamma(S_R - 1 + \mu))\alpha_L^*}$$

In this expression, we need to assume that  $(K - v\gamma(S_R - 1 + \mu)) > 0$ . From the following:

$$f_L(\alpha_L) = -v\gamma(S_R - \mu) \frac{\frac{K}{2} + \frac{\alpha_L^*}{2} + \sqrt{\left(\frac{K}{2} + \frac{\alpha_L^*}{2}\right)^2 - (K - v\gamma(S_R - 1 + \mu))\alpha_L^*}}{\left[\frac{K}{2} + \frac{\alpha_L^*}{2} + \sqrt{\left(\frac{K}{2} + \frac{\alpha_L^*}{2}\right)^2 - (K - v\gamma(S_R - 1 + \mu))\alpha_L^*}\right] - \alpha_L^*} - K$$

we can proceed in a similar manner than for  $f_R(\alpha_R)$ , since  $(K - v\gamma(S_R - 1 + \mu)) > 0$ ,  $g'_L(\alpha_L) < 0$ . To sum up, if the parameter values belong to the following set then an equilibrium exists, it is unique and it belongs to  $\Theta$ :

$$\Pi = \left\{ \{K, \mu, \underline{\alpha}, \bar{\alpha}, v, \gamma\} : g_R\left(\frac{K}{2}\right) > 0, g_R(\bar{\alpha}) < 0, g_R(\underline{\alpha}) > 0, g_L\left(-\frac{K}{2}\right) < 0, \right. \\ \left. K > \max\{-v\gamma(S_R - \mu), v\gamma(S_R - 1 + \mu)\} \right\}$$

Finally, it is trivial to prove that  $\Pi$  is non-empty by means of an example.

### I.2.2 Proof of Proposition 2

Denote  $C_R = \frac{\bar{\alpha} - \alpha_R^*}{\bar{\alpha} - \underline{\alpha}}$  as the mass of individuals contributing to the right-wing party and  $C_L = \frac{\alpha_L^* - \underline{\alpha}}{\bar{\alpha} - \underline{\alpha}}$  as the mass of individuals contributing to the left-wing party. It follows that:

$$\frac{dC_R}{dv} - \frac{dC_L}{dv} = \frac{1}{\bar{\alpha} - \underline{\alpha}} \left[ -\frac{d\alpha_R^*}{dv} - \frac{d\alpha_L^*}{dv} \right]$$

We need to prove that  $S_R > \frac{1}{2}$  implies that  $\frac{dC_R}{dv} - \frac{dC_L}{dv} > 0$ . To establish this, we need to obtain expressions for  $\frac{d\alpha_R^*}{dv}$  and  $\frac{d\alpha_L^*}{dv}$ . We will calculate those derivatives using the implicit function theorem. We start by defining:

$$F(v, \alpha_R^*, \alpha_L^*) = \begin{bmatrix} \alpha_R^* - v\gamma(S_R - 1 + \mu) \frac{\alpha_L^*}{\alpha_R^* - \alpha_L^*} - K \\ -\alpha_L^* - v\gamma(S_R - \mu) \frac{\alpha_R^*}{\alpha_R^* - \alpha_L^*} - K \end{bmatrix}$$

$$H = \begin{bmatrix} \frac{dF_1}{d\alpha_R^*} & \frac{dF_1}{d\alpha_L^*} \\ \frac{dF_2}{d\alpha_R^*} & \frac{dF_2}{d\alpha_L^*} \end{bmatrix} = \begin{bmatrix} 1 + v\gamma(S_R - 1 + \mu) \frac{\alpha_L^*}{(\alpha_R^* - \alpha_L^*)^2} & -v\gamma(S_R - 1 + \mu) \frac{\alpha_R^*}{(\alpha_R^* - \alpha_L^*)^2} \\ v\gamma(S_R - \mu) \frac{\alpha_L^*}{(\alpha_R^* - \alpha_L^*)^2} & -1 - v\gamma(S_R - \mu) \frac{\alpha_R^*}{(\alpha_R^* - \alpha_L^*)^2} \end{bmatrix}$$

$$M_{\alpha_R^*}^v = \begin{bmatrix} \frac{dF_1}{dv} & \frac{dF_1}{d\alpha_L^*} \\ \frac{dF_2}{dv} & \frac{dF_2}{d\alpha_L^*} \end{bmatrix} = \begin{bmatrix} -\gamma(S_R - 1 + \mu) \frac{\alpha_L^*}{\alpha_R^* - \alpha_L^*} & -v\gamma(S_R - 1 + \mu) \frac{\alpha_R^*}{(\alpha_R^* - \alpha_L^*)^2} \\ -\gamma(S_R - \mu) \frac{\alpha_R^*}{\alpha_R^* - \alpha_L^*} & -1 - v\gamma(S_R - \mu) \frac{\alpha_R^*}{(\alpha_R^* - \alpha_L^*)^2} \end{bmatrix}$$

$$M_{\alpha_L^*}^v = \begin{bmatrix} \frac{dF_1}{d\alpha_L^*} & \frac{dF_1}{dv} \\ \frac{dF_2}{d\alpha_L^*} & \frac{dF_2}{dv} \end{bmatrix} = \begin{bmatrix} 1 + v\gamma(S_R - 1 + \mu) \frac{\alpha_L^*}{(\alpha_R^* - \alpha_L^*)^2} & -\gamma(S_R - 1 + \mu) \frac{\alpha_L^*}{\alpha_R^* - \alpha_L^*} \\ v\gamma(S_R - \mu) \frac{\alpha_L^*}{(\alpha_R^* - \alpha_L^*)^2} & -\gamma(S_R - \mu) \frac{\alpha_R^*}{\alpha_R^* - \alpha_L^*} \end{bmatrix}$$

By the implicit function theorem, we know that:

$$\frac{d\alpha_R^*}{dv} = -\frac{\det(M_{\alpha_R^*}^v)}{\det(H)} = \gamma(S_R - (1 - \mu)) \frac{\alpha_L^* + \frac{K}{2}}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)}$$

Then, for  $\frac{d\alpha_L^*}{dv}$ :

$$\frac{d\alpha_L^*}{dv} = -\frac{\det(M_{\alpha_L^*}^v)}{\det(H)} = \gamma(S_L - (1 - \mu)) \frac{\alpha_R^* - \frac{K}{2}}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)}$$

Finally, we can replace back in  $\frac{dC_R}{dv} - \frac{dC_L}{dv}$ :

$$\frac{dC_R}{dv} - \frac{dC_L}{dv} = \frac{1}{\bar{\alpha} - \underline{\alpha}} \gamma \left[ \frac{\left[ -\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right) \right] \left(S_R - \frac{1}{2}\right) - \left(\mu - \frac{1}{2}\right) [\alpha_L^* + \alpha_R^*]}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)} \right]$$

Combining  $F_1(\cdot) = 0$  and  $F_2(\cdot) = 0$ , we know that:

$$\alpha_R^* + \alpha_L^* = v\gamma \left(S_R - \frac{1}{2} + \left(\mu - \frac{1}{2}\right)\right) \frac{\alpha_L^*}{\alpha_R^* - \alpha_L^*} - v\gamma \left(S_R - \frac{1}{2} + \left(\frac{1}{2} - \mu\right)\right) \frac{\alpha_R^*}{\alpha_R^* - \alpha_L^*}$$

Plugging this expression in the previous equation, we obtain:

$$\frac{dC_R}{dv} - \frac{dC_L}{dv} = \frac{1}{\bar{\alpha} - \underline{\alpha}} \gamma \left[ \frac{\left[ -\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right) + \left(\mu - \frac{1}{2}\right) v\gamma \right] \left(S_R - \frac{1}{2}\right) + v\gamma \left(\mu - \frac{1}{2}\right)^2}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)} \right]$$

If  $\mu \geq \frac{1}{2}$ , then  $S_R > \frac{1}{2}$  implies  $\frac{dC_R}{dv} - \frac{dC_L}{dv} > 0$ , which is exactly what we needed to prove.

### I.2.3 Proof of Proposition 3

Recall the values of  $\frac{dC_R}{dv}$  and  $\frac{dC_L}{dv}$  from Proof of Proposition 2:

$$\begin{aligned} \frac{dC_R}{dv} &= -\frac{1}{\bar{\alpha} - \underline{\alpha}} \frac{d\alpha_R^*}{dv} = \gamma \frac{S_R - (1 - \mu)}{\bar{\alpha} - \underline{\alpha}} \frac{-\left(\alpha_L^* + \frac{K}{2}\right)}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)} \\ \frac{dC_L}{dv} &= \frac{1}{\bar{\alpha} - \underline{\alpha}} \frac{d\alpha_L^*}{dv} = \gamma \frac{S_L - (1 - \mu)}{\bar{\alpha} - \underline{\alpha}} \frac{\alpha_R^* - \frac{K}{2}}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)} \end{aligned}$$

Since  $\alpha^* \in \Theta$ , we have  $-\left(\alpha_L^* + \frac{K}{2}\right) > 0$ ,  $\alpha_R^* - \frac{K}{2} > 0$  and  $-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right) > 0$ . It is straightforward to verify that the sign of  $\frac{dC_j}{dv}$  is positive if  $S_j > 1 - \mu$ , null if  $S_j = 1 - \mu$ , and negative if  $S_j < 1 - \mu$ .