

Partisan Interactions

Evidence from a Field Experiment in the United States^{*}

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Abstract

Since campaign contributions reveal the actor’s party leanings, they take place in a domain of social observation and are likely to be subject to social effects. We conducted a field experiment to identify some of these social effects. We sent letters to 92,000 contributors from all U.S. states during the 2012 presidential election campaign. We randomized subtle details in those letters to create non-deceptive experimental variation in the probability that the recipient’s contributions were observable to her neighbors, and in the recipient’s perception of the contributions of others. We use administrative data to measure the effects of these variations on the recipients’ subsequent contributions. We show that making an individual’s contributions more visible to her neighbors increases the contributions of supporters of the local majority party, and decreases those of supporters of the minority party. This evidence is consistent with a model of partisan signaling in which individuals treat supporters of their own party favorably and supporters of the opposite party unfavorably. Additionally, we show that individuals contribute more when they perceive higher average contributions from own-party supporters in their area, but do not react to contributions from opposite-party neighbors, which is consistent with social norm theories. Last, individuals contribute lower amounts when they perceive a higher share of own-party contributors, which can be interpreted as free-riding. Taken together, the evidence suggests that partisan interactions play an important role in shaping political participation.

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1 Introduction

Most forms of political participation (with notable exceptions, such as the act of voting) reveal the party or cause that an individual supports. Examples include making campaign contributions, attending rallies, making political comments on online social networks, or merely discussing politics with others. As a result, the effect of social interactions on those forms of participation can be markedly partisan. This paper provides unique revealed-preference on these partisan interactions.

Specifically, we discuss two distinct channels through which social interactions may affect political participation. First, the *conformity channel* posits that disclosing one’s party affiliation through political participation can result in better treatment by supporters of one’s party and in harsher treatment by supporters of the opposite party. For example, a Democrat living in an area with a majority of Republican supporters might refrain from making a contribution to her party’s candidate to avoid social sanctions from her contacts. Second, the *comparison channel* posits that an individual’s political behavior may depend on the observed political behavior of her peers. For example, if a Republican finds out that her like-minded friends are contributing very little, she may feel entitled to make a small contribution. In other words, while the conformity channel is the result of feeling observed by others, the comparison channel is the result of observing the behavior of others.

The identification of peer effects from observational data is a challenging task (Manski, 1993). First, it is hard to establish the direction of causality: e.g., whether Democrat contributors are more active when they live in a more Democratic area, or if more active Democrats simply are more likely to live in more Democratic areas.¹ Second, it is even more challenging to unpack the different underlying causal mechanisms from observational data, such as distinguishing the effects from observing others from the effects of being observed. We designed a field experiment to address these identification challenges, exploiting the unique institutional setting of U.S. campaign finance.

Federal law dictates that campaign committees must report the identity of individuals who contribute over \$200 to the Federal Election Commission (FEC) along with personal information. The FEC makes these contribution records not only publicly available but, more importantly, easily accessible online. The FEC website provides up-to-date disaggregated information about contributors, including full name and address, occupation, employers, contribution amount and date, and the party and candidate to which the contribution was

¹See for example the discussion in Perez-Truglia (2014), who uses an event-study analysis of residential mobility to disentangle the direction of causality in the context of partisan interactions. For a discussion of sources of spatial auto-correlation in campaign contributions, see for example Cho (2003), Gimpel, Lee and Kaminski (2006), McCarty et al. (2006), Bishop (2008) and Cho, Gimpel and Hui (2013).

made. Moreover, the FEC website offers an online tool that allows visitors to search for contributors on the basis of each of these characteristics (e.g., name, address).² The fact that an individual's contributions are observable by others and that an individual can observe (and thus be influenced by) the contributions of others makes campaign contributions in the United States an excellent context for studying partisan interactions.

In May 2012, we sent letters with individualized information related to campaign contributions to a sample of 91,998 individuals from all U.S. states who, according to the FEC records, had made a contribution to a presidential campaign between April 2011 and April 2012. The sample included individuals who, on average, had contributed about \$500 at that point. The letters sent to these subjects were identical in every aspect except subtle variations in the information displayed that were randomly assigned in a non-deceptive way to test the conformity and comparison channels. We then used the FEC records to measure how this exogenous variation in the letters affected the subject's subsequent contributions during the six months between the mailing delivery and the end of the 2012 presidential campaign.

The first treatment arm was designed to test the conformity channel by generating exogenous variation in the visibility of the recipient's own contribution. This type of letter provided information about the public nature of campaign contribution records and how to access the FEC's online search tool. We randomly assigned these 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 was identical in every aspect to the first except that it indicated that their household along with other households in the area had been randomly selected to receive a letter of this type. The second sub-treatment differs from the first in that other individuals in the area also received information about how to access the FEC records. Thus, the difference between these two sub-treatments can be interpreted as an increase in the recipient's perceived probability that her neighbors will observe her future contributions.

The second treatment arm was designed to test the comparison channel by generating exogenous variation in the recipient's perception of the contribution behavior by her peers. We sent a letter that listed the semi-anonymized names, the amounts contributed and the parties contributed to by nine contributors from the recipient's area of residence. We randomly selected those nine individuals from the thirty contributors nearest to the recipient's address. This selection was based on a series of parameters that we varied randomly to create non-deceptive exogenous variation along multiple dimensions of the list of contributors, such as the average amount contributed. As a result, this methodology creates exogenous variation in

²Appendix A.9 provides more details about the FEC's online search tool.

the recipient’s perception of the contributions of others. We then compare the contributions of recipients who were sent the same letter type but with different peer contributions.³

The results for the conformity channel suggest that the visibility of one’s contributions does indeed matter, and in a markedly partisan way. We find that, when contributions are made more visible to others, an individual’s contribution increases if a majority of her neighbors supports her party, but decreases if a majority supports the opposite party. These effects are not only statistically significant, but large in magnitude. Our preferred treatment-on-the-treated estimates suggest that in highly polarized areas, with 75% of neighbors supporting one party, our higher visibility treatment reduced the amount contributed by minority supporters by 41% (relative to the baseline amount) and increased the amount contributed by majority supporters by 15%.⁴ This evidence is consistent with partisan interactions where individuals treat supporters of their own party favorably and supporters of the opposite party unfavorably.

The results for the comparison channel suggest that an individual’s contributions are also significantly affected by her perceptions about the contribution behavior of others. Recipients contributed more when neighbors of the same party were shown to contribute higher average amounts. This effect is significant both statistically and economically. Our preferred treatment-on-the-treated estimate indicates that for each additional \$100 in the average amount contributed by own-party neighbors, the recipient’s own contribution increased by \$13.6. While there are other plausible interpretations, this evidence is consistent with the formation of a social norm about what constitutes a “fair” contribution amount. Individuals did not contribute significantly more, however, when neighbors from the opposite party were shown to contribute higher average amounts. This finding is consistent with theories of identity (Akerlof and Kranton, 2000), according to which individuals form social norms based on the behavior of peers with whom they identify (in this case, neighbors who support the same party). In addition to the information on the average amount contributed, we also examined whether individuals care about the distribution of contributors across parties. We find that individuals contributed less when there was a higher number of own-party relative to opposite-party contributors. This effect goes in the opposite direction than social norms, according to which an individual should feel more pressure to contribute when a higher number

³Even though a majority of our subjects knew that contribution records were public, we still expected them to react to the information in the List letter. There is ample evidence that, possibly due to irrational or rational inattention, individuals systematically fail to take into account publicly-available and easily-accessible information about important economic variables. See for instance Armantier et al. (2013) for inflation expectations, and Cruces, Perez-Truglia and Tetaz (2013) for perceptions of relative income.

⁴These estimates correct for the fact that many individuals may have discarded the mailpiece we sent to them. We conducted a post-election survey to obtain information for this correction. See Section 4.1.3 for details.

of her like-minded peers contribute. Instead, this evidence can be interpreted as free-riding behavior.

We perform some simple back of the envelope calculations to quantify the effects of the conformity and comparison channels on contribution patterns. First, we analyze the counterfactual scenario in which individuals cannot identify the contributions of others – i.e., without conformity effects. We find that conformity effects reduce the participation of supporters of the local minority and increase the participation of the majority, and thus induce higher geographic polarization of contributions along partisan lines. Second, we analyze a counterfactual scenario where individuals ignore the contribution behavior of others – i.e., without comparison effects. The counter-factual analysis suggests that, even though in theory this channel could have an effect on geographic polarization, in practice this effect is negligible.

While our evidence is based on the particular case of campaign contributions, similar partisan interactions are likely to take place with most forms of political participation, such as talking about politics, sharing political news, attending rallies, and even registering to vote.⁵

Our paper relates to a series of recent studies on political participation. Regarding voting turnout, the literature has long emphasized the importance of social pressure (Knack, 1992) and social norms (Riker and Ordeshook, 1968). In a seminal contribution, Gerber, Green and Larimer (2008) conducted a field experiment in which, close to election day, 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 found that these letters had a large positive effect on subsequent turnout, which they interpret as arising from some combination of social norms and social pressure.⁶ Unlike most other forms of political participation, though, the act of voting does not in itself reveal the party or cause that the individual supports. As a result, Gerber et al. (2008) and other related studies (Funk, 2010; DellaVigna, List, Malmendier and Rao, 2014) do not present any evidence about how individuals interact with peers from the same and the opposite party.⁷ To the best of our knowledge, our paper is the first to provide experimental evidence about partisan interactions.⁸

Our findings can be informative for the ongoing debate about the reasons behind individ-

⁵Furthermore, in the case of campaign contributions, we present some complementary evidence that indicates that the use of the FEC search tool arises naturally, without the need for our experimental interventions.

⁶A number of studies have extended the analysis in Gerber et al. (2008). For an overview of this literature, see Green and Gerber (2010).

⁷The letter sent by Gerber et al. (2008) did not disclose any information related to partisan affiliation.

⁸Additionally, we make a methodological contribution by developing an experimental design to disentangle the effects of being observed by others versus from the effects of observing the behavior of others.

ual contributions to political campaigns.⁹ This type of contributions represent a large portion of campaign funding in the United States: e.g., approximately 80% of the \$1.7 billion dollars raised in the 2012 presidential race consisted of individual contributions. In particular, there is a debate about whether individuals make contributions because of consumption and investment motives, which can have relevant implications for campaign finance (Ansolabehere et al., 2003).¹⁰ Our experimental evidence provides specific mechanisms underlying consumption motives for contributions: e.g., individuals may contribute to get favorable treatment from their family, friends and acquaintances (the conformity channel), or they may contribute to avoid the negative utility from self-image (the comparison channel).

Our findings on social incentives are also related to the literature on pro-social behavior. With respect to the conformity channel, there is evidence that social pressure is effective for inducing pro-social behavior. Individuals are also more likely to give money to a charity when they cannot avoid the solicitor (DellaVigna et al., 2012), and academics review journal articles faster when their review times are made public (Chetty et al., 2014).¹¹ In terms of political participation, individuals are more likely to vote when their participation in elections is observable to others (Gerber et al., 2008; DellaVigna et al., 2014). Related to the comparison channel, there is evidence that students are more likely to donate to a university when told that a higher share of other students donated in the past (Frey and Meier, 2004). There is also evidence that households' energy consumption changes when they are provided with information about the consumption of neighbors (Allcott, 2011). The fact that social pressure and social comparisons are also relevant factors for individuals' campaign contribution decisions suggests that making campaign contributions can be considered, to a significant extent, as another form of pro-social behavior.

The paper is organized as follows. Section 2 discusses the relevant hypotheses and the experimental design used to test them. Section 3 presents the data sources and the implementation of the field experiment. Sections 4 present the main results. Section 5 provides a counterfactual analysis of the conformity and comparison effects on geographic polarization. The final section concludes.

⁹To the best of our knowledge, few papers present experimental evidence on campaign contributions. A recent exception is Augenblick and Cunha (2015), who conducted an experiment in the United States with randomly assigned messages to contributors.

¹⁰For instance, Campante (2011) shows that taking into account the consumption motive plays an important role in explaining why the standard median-voter-based prediction that more inequality induces more redistribution has received little empirical support.

¹¹One traditional interpretation for these findings is that individuals use these forms of pro-social behavior to signal their altruism to others (Andreoni, 1989; Bénabou and Tirole, 2006, 2011; Andreoni and Bernheim, 2009; Ali and Lin, 2013). There is also evidence that social pressure plays an important role in eliciting giving within religious organizations (Bottan and Perez Truglia, 2015).

2 Hypotheses and Experimental Design

2.1 The Conformity Channel

2.1.1 Hypotheses

In their social interactions, individuals care about how they are perceived by others and thus can behave strategically to affect those social perceptions (Bénabou and Tirole, 2006, 2011). In this paper, we consider the possibility that peers care about the political party that the individual supports (or, more broadly, candidate, causes and ideology). The existence of partisan favoritism has been long recognized (e.g., Campbell et al., 1960), and is consistent with evidence that individuals report to be more sympathetic with supporters of their own political party (e.g., Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2015).¹²

Most social-signaling models deal with actions that are unambiguously perceived as good by all peers, in which case higher visibility is predicted to increase pro-social behavior. Under partisan favoritism, however, contributors interact with supporters of both parties and thus face a dual audience.¹³ As a result, the effects of more visible contributions will depend on the partisan composition of the peers with whom the individual interacts. If an individual interacts only with supporters of her party, an increase in the visibility of contributions would be expected to make contributing more attractive, due to the positive effects on interactions with like-minded peers. If, on the contrary, the individual interacts exclusively with supporters of the opposite party, then higher visibility would make contributions less attractive, due to the resulting negative effects in social interactions. More generally, the effect of higher visibility should increase with the share of the audience that sympathize with the individual's own party. Appendix F provides a simple signaling model that formalizes this intuition, which is based on the more general idea of conformity from Bernheim (1994).¹⁴

2.1.2 Experimental Design

The first treatment arm was designed to induce an exogenous variation in the visibility of the recipient's contributions, using the FEC search tool as a medium. To better understand

¹²See Sunstein (2015) for a review of related studies. Gentzkow and Shapiro (2011) find that the level of political segregation of direct social interactions with neighbors, coworkers and family members is higher than the segregation in online and offline media consumption. Furthermore, Gerber et al. (2013) show that some individuals do not vote because they do not trust the privacy of voting, which could perhaps suggest that they do not want to disclose their partisan affiliation.

¹³For other studies of dual-audience in political participation, see for example Gentzkow, Shapiro and Sinkinson (2014) on media outlets and Glaeser, Ponzeto and Shapiro (2005) on religious networks.

¹⁴This model shows, among other things, that the above prediction is qualitatively robust to the introduction of homophily (i.e., a higher likelihood of interacting with more like-minded neighbors). Of course, there are other plausible interpretations of these conformity effects besides partisan signaling.

this intervention, it is useful to describe the awareness about the disclosure policy. Figure 1.a shows the distribution of answers to a survey question about the knowledge of the disclosure policy.¹⁵ The results indicate that contributors were well aware of the FEC disclosure policy: 86% of respondents agreed with the statement that contribution records were a matter of public information, while the remaining 14% reported that they believed them to be confidential.¹⁶ This high awareness is not surprising, given that campaign committees are required to collect detailed information from individual contributors and, when they do, they must explain that this information is required by the FEC due to the disclosure policy. This awareness is consistent as well with the Internet browsing data discussed in Appendix E indicating that the FEC’s search tool and other websites based on its information are widely accessed. In turn, Figure 1.b indicates that contributors are not so certain as to whether their neighbors know about the public nature of contribution records: only 40% of respondents believed that the majority or the vast majority of their neighbors believed that contribution records were confidential.¹⁷ These results suggest that informing a contributor’s neighbors about how to access contribution records may have a potentially large impact on her perceived visibility of contributions.¹⁸

We designed a type of letter, which we labeled Website, for the purpose of providing information on how to use the FEC website to search for individual contributors (for a sample, see Appendix A.1). This flyer-like mailpiece consisted of a single sheet of paper that was folded and sealed to form an envelope (see sample in Appendix A.6). As with all the other types of letters used in this experiment, we identified the research purpose of the communication: “This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University.” The letters included the URL of the project’s website, which provided basic information on the project as well as contact information for the research team and for Harvard’s Institutional Review

¹⁵Details about the survey will follow in Section 3.3 below.

¹⁶These figures correspond to responses from subjects in our sample of contributors who did not receive any letter from us (besides the survey). 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). The wording in the survey was very general since we wanted to measure general awareness of the public nature of this information rather than test the subjects on the details of the regulation. Appendix D presents more details about the survey instrument, including a facsimile with the exact wording of this question and of response options.

¹⁷This may well reflect a belief among contributors that only a minority of their neighbors make contributions as well, and a belief that non-contributors are likely less aware of the public nature of contribution records than their contributor counterparts.

¹⁸Since the vast majority of contributors knew about the public nature of contribution records, sending information about the FEC disclosure policy to them should have only a limited effect on their perception of the visibility of their contributions. Furthermore, this visibility effect would be confounded with comparison effects, because informing contributors about the existence of the FEC search tool could prompt them to search for information about the contributions of others.

Board (see Appendix A.8 for the content of this site). The main purpose of the site was to provide interested subjects with contextual information about our study to clear up any doubts about its legitimacy by placing emphasis on its academic and non-partisan nature.¹⁹

This Website letter contained a list that included the name of the recipient and the five contributors nearest to the recipient’s location, along with the party and the amount given by each of those listed.²⁰ The recipient of the letter was always the second name on the list; the full name of the recipient was used (other contributors were identified only by first name and initial of last name) and highlighted on the list. This short list of contributors was included for two reasons: first, to draw the recipient’s attention to the content of the letter and, second, to reinforce the perception that contribution records are indeed publicly available by providing verifiable information (including the recipient’s own contributions to date). The second paragraph of the letter identified the FEC as the source of the information and explained that the name, address, and other details about contributions were readily accessible online. That paragraph also included a link to the FEC’s website search tool along with the statement that the website could be used “to see which candidates or political parties your neighbors, friends, family and co-workers are contributing to.”

We introduced an exogenous variation in the visibility of the recipient’s contributions by including two sub-treatments: Website-Self and Website-Neighbors. These two letter sub-types were identical in all aspects, except for a message prominently displayed in a box located right below the list of contributors stating:

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

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

This information was non-deceptive: we conducted the randomization such that those receiving the Website-Self were the only ones in their area to receive the letter, while there were multiple recipients of the Website-Neighbors letter within an area.²¹ Since other individuals

¹⁹Although the website did contain general information about the main research objective, to avoid contamination of the results, no information was provided on the precise hypotheses being tested or on 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 send an e-mail to a dedicated e-mail address with the indication that we would reply by sending a brochure only after the data collection process was completed.

²⁰The median pairwise distance between the recipients and their five closest neighbors who were contributors was 0.35 miles.

²¹Specifically, we divided the United States into disjointed geographical areas of similar population. 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 households in the area in our FEC database of contributors) was

in their area also received information about how to access the FEC records, recipients of Website-Neighbors letters should have considered it more likely that their neighbors use the FEC search tools to monitor their future contributions (relative to recipients of the Website-Self letters); every other aspect of the letters was identical, and thus any other potential effect of the letter in itself should cancel out in the comparison between the two groups.²² In other words, the difference between these two sub-treatments can be interpreted as an increase in the visibility of recipients’ contribution to their neighbors.²³

To estimate the effect of higher visibility, we proceeded as follows. Let Y_i be a measure of the recipient’s post-treatment contributions. The econometric specification is:

$$Y_i = \beta_0 \cdot HigherVisibility_i + \beta_1 \cdot HigherVisibility_i \cdot ShareOwnParty_i + \alpha \cdot ShareOwnParty_i + \delta X_i + \varepsilon_i \quad (1)$$

where *HigherVisibility* is a dummy variable that takes the value of 1 if the subject was assigned to the sub-treatment Website-Neighbors and the value of 0 if she was assigned to the sub-treatment Website-Self. *ShareOwnParty* denotes the proportion of the recipient’s neighbors who support the recipient’s party. The marginal effect of the higher-visibility treatment in a given area is given by: $\beta_0 + \beta_1 \cdot ShareOwnParty_i$. The conformity channel predicts that higher visibility will discourage participation in areas where a vast majority of neighbors support the opposite party (i.e., $\beta_0 < 0$), but will encourage participation in areas where the vast majority of neighbors support the same party (i.e., $\beta_0 + \beta_1 > 0$). Finally, X_i is a group of control variables such as the pre-treatment contributions made by the recipient. The inclusion of these variables improves the precision of the estimates slightly, but the results are robust even when they are excluded.

Finally, it must be noted that *ShareOwnParty* is a characteristic of our subjects’ areas of residence, and as such, it was not randomized as part of the experiment. Thus, we rely on the assumption that there are no other characteristics that mediate the effect of *HigherVisibility_i* and are correlated with *ShareOwnParty_i*: i.e., $E[HigherVisibility_i \cdot ShareOwnParty_i \cdot \varepsilon_i] = 0$. We provide a series of robustness checks related to this assumption in the empirical section

sent a letter of this type. In the areas assigned to the Website-Neighbors treatment, we randomly selected three households to be sent these letters, assigning more 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.

²²We could not anticipate all the effects that one type of letter could induce. This is why we do not base our analysis on comparisons between recipients of one type of letters and non-recipients, but rely instead on within-treatment-arm variation. This strategy nets out direct effects of the letters (the type of information displayed, for instance) or other indirect impacts such as Hawthorne effects (Levitt and List, 2011).

²³In the letters, we emphasized the fact that households were randomly selected to receive these letters in order to minimize the possibility that recipients make spurious inferences on the basis of having been selected for our mailing. Significantly, the letters did not specify whether the neighbors receiving the Website letter were contributors or not. In fact, because of the way we selected our sample, we only sent letters to contributors.

below.

One of these robustness checks consists of measuring whether the effects of an additional treatment arm, the Placebo letter, were heterogeneous with respect to $ShareOwnParty_i$. This Placebo letter had the same format as the Website letter but, instead of displaying a table with the contribution activity of neighbors, it presented 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,²⁴ and, most importantly, because contribution limits were not binding for virtually all of the individuals in our subject pool.

2.2 The Comparison Channel

2.2.1 Hypotheses

According to social norms theory, individuals are more motivated to engage in pro-social behavior when they perceive others to be doing so (Cialdini, 1984; Akerlof and Kranton, 2000). In the context of campaign finance, individuals may follow a social norm about the right amount to contribute to political campaigns. We would then expect an individual’s contribution to increase with her perception of the average amount contributed by others, which may be used to assess the contribution norm. Moreover, social norms theory poses that individuals care only about the behavior of others with whom they identify (Akerlof and Kranton, 2000). Applying this conjecture to the context of campaign finance implies that individuals may care about the average amount contributed by members of the same political party more than about the average amount contributed by members of the opposite party (indeed, she may not care at all about those contributions). Last, apart from the average amount contributed by others, social norms considerations may also depend on the number of individuals contributing to each party: i.e., a higher number of own-party contributors could also increase the individual’s own contribution rate.

Besides social norms, information about the contributions of others can affect an individual’s own contribution through other channels. While social norms predict that an individual should give more as others also contribute more, these additional channels may have effects in the opposite direction. First, if individuals contribute with the hope of affecting the electoral outcomes, their utility should depend on the marginal effect of their contribution on the campaign. Because of diminishing marginal returns, an individual’s marginal contribution would have a smaller effect on the campaign (on expectation) when others give more to her own party, thus making contributing less attractive. In other words, individuals may

²⁴See for example Figure D.1.a in the Appendix.

crowd-out each other’s contributions. Second, individuals may also make campaign contributions because they believe that these would buy them favors from (or at least access to) politicians. For instance, an experiment from Kalla and Broockman (2015) showed that U.S. legislators made themselves more available when receiving meeting requests from constituents when these requests stated that the prospective attendee had contributed to the legislator’s campaign. If contributors expect favors from politicians, an increase in others’ contributions could make their own less attractive, because they would “buy” fewer favors.

2.2.2 Experimental Design

We devised a treatment arm called the List letter that provided information about the behavior of other contributors in the recipient’s area. A sample letter is presented in Appendix A.3. The letter contained the same contextual information as the Website letter regarding the purpose of the communication (research), the project’s website, and contact information. The bulk of the List letter, though, consisted of information about presidential campaign contributions made by the recipient and by nine other individuals from the recipient’s area of residence from April 1, 2011 to April 1, 2012.²⁵ The information included, in table form, first name, last name initials, party and the amount contributed by each of those listed.²⁶ The recipient’s own contribution and name (highlighted) was at the top of the list in order to draw the recipient’s attention and to demonstrate the credibility of the study since the individual could confirm that the information was accurate. To facilitate the assimilation of the information, contributions were ordered from highest to lowest amounts, first for Democratic candidates and then for Republican candidates.

We used an algorithm that randomly selected the nine contributors to be included in the table in order to introduce experimental variation in the lists presented to the recipients. We first obtained the geo-location for all individuals listed in our baseline FEC database. Then, for each contributor i assigned a List letter, we identified the thirty closest contributors, L_i , which we defined to be the individual’s neighbors.²⁷ The nine neighbors included in the table

²⁵The median pairwise distance between the recipient and the nine neighboring contributors was 1.2 miles.

²⁶Since the main purpose of this treatment arm is to study how contributors act when they observe others rather than how they behave when they feel observed by others, we tried to prevent, to the extent possible, recipients from feeling that their contribution activity was more exposed to their neighbors due to our letter (e.g., by using last name initials and not including the URL of the FEC search tool). It should be noted, however, that the estimation of this treatment effect relies on within-arm variation (i.e., by comparing contributions for individuals who received the same letter type but with different information), and thus any visibility effect from the List letters should be netted-out by design.

²⁷ L_i is constructed on the basis of pairwise distances as the crow flies. These neighboring contributors were selected from all FEC records, not only from our selected subject pool. Only 0.08% of early contributors had simultaneously made contributions over \$200 to the Obama campaign and to at least one of the Republican presidential candidates. For the sake of simplicity, we did not take them into account to build L_i .

were selected from L_i first by ordering the list of thirty contributors in the area according to a composite index and then by picking 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, $Party(j)$, of the amount contributed by j during the preceding twelve months period, $Amount(j)$, and a set of constants, $\epsilon_i(j)$:

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

The parameters $\{\theta_i^D, \theta_i^A\}$ are the recipient-specific weights assigned to each of those dimensions. The list of the top nine neighbors is a function of those parameters, denoted by $g(L_i; \theta_i^D, \theta_i^A)$. The baseline list refers to the list of nine neighbors given by setting the two weights to zero, $g(L_i; 0, 0)$. The weight assigned 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 assigned to the contribution amount was randomly selected from three possible values: $\theta_i^A = -c_a, 0, c_a$, with $c_a > 0$. We calibrated the distribution of the parameter values so that the average characteristics of the lists were not biased relative to the baseline.²⁸ Note that the information provided was not deceptive insofar as the letters stated that the table included nine of the recipient's neighbors and, given our definition of neighbors, that claim always holds true.

This composite index induced exogenous variation in the contribution patterns shown in each List letter. Table 2 presents three possible lists of nine neighbors generated by different combinations of the parameter weights. The panel on the left presents the baseline list ($\theta_i^D = 0, \theta_i^A = 0$). The center panel presents the list obtained when the Democratic weight is assigned a negative value ($\theta_i^D < 0, \theta_i^A = 0$). When this operation is performed, two of the Democratic contributors that appear on the baseline list are replaced by two Republican contributors. In the panel on the right, the weight on the amount is assigned a positive value ($\theta_i^D = 0, \theta_i^A > 0$). Here, two of the Democrat contributors that appear on the baseline list are replaced by two other Democrats who contributed higher amounts, and one of the Republican contributors is replaced by one Republican who contributed a higher amount.

Our identification strategy does not rely on a comparison of post-treatment contributions by individuals who received the List letters and those who did not receive any letter. It relies, rather, on the comparison between individuals who received List letter. A simple example conveys intuition on the estimation of these effects. Imagine that we sent some contributors a table with an average contribution of \$500 while we sent others a table with an average

²⁸We calibrated as well the distribution of the parameter values to yield an orthogonal variation in key dimensions of the list, such as the average amounts contributed and the number of contributors to the Democratic party.

contribution of \$600. In that case, we could estimate a regression of the post-treatment contributions on a variable that takes the value of 0 for the recipients randomly assigned to the \$500-letter and the value of 100 for the recipients assigned to the \$600-letter. If the estimated coefficient on that variable is 0.1, it would imply that each additional dollar in average contributions shown in the letter caused the recipient to contribute an additional ten cents.

We can generalize the above framework for the case when we simultaneously randomize multiple dimensions of the information contained in the letter. Let $f^j(\cdot)$ represent any statistic j from a given list (e.g., the mean contribution to the recipient’s own party), and recall that Y_i denotes the recipient’s post-treatment contributions. The econometric specification is:

$$Y_i = \sum_{j=1}^J \beta_j \cdot \Delta f_i^j + \delta X_i + \varepsilon_i \quad (2)$$

where $\Delta f_i^j \equiv f^j(g(L_i; \theta_i^D, \theta_i^A)) - f(g(L_i; 0, 0))$ is the value of statistic j in the list shown to the individual compared to the value that would have resulted if she had received the baseline list (for instance, the mean contribution in the list sent minus the mean contribution in the baseline list). Since the variation in Δf_i^j is driven entirely by the random assignment of $\{\theta_i^D, \theta_i^A\}$, the coefficient on Δf_i^j can be interpreted as the causal effect of the f_i^j included in the list on the recipient’s post-treatment contributions. As a measure of how much exogenous variation was induced, the correlation between the mean amount contributed in the actual table sent to the recipient and the mean amount in the baseline table is about 0.75.²⁹

Our research design differs from the design of other related field experiments in that our informational treatments did not contain specific messages that would prime individuals to pay attention to a particular dimension of the information provided.³⁰ In other words, we did not include suggestive messages like “the average contribution in your neighborhood was \$500.” Instead, our letters simply provided itemized information about neighbors’ contribution records, and the recipients were free to do whatever they wanted with that information. If individuals actually changed their behavior in response to the information provided in

²⁹Note that simply selecting nine of the thirty closest neighbors at random, without a composite index, would also introduce exogenous variation in the information included in the table of contributors. A simple random selection process, though, would not have given us any control over how much exogenous variation was introduced for each statistic k , possibly resulting in insufficient variation for the regression analysis. On the other hand, we did not induce too much exogenous variation, since this could compromise the credibility of the information provided to the subject.

³⁰For instance, Allcott (2011) studies a field experiment in which subjects were mailed information with the energy consumption patterns of similar neighbors. Instead of providing itemized consumption levels for each neighbor, the letters provided information about aggregates of the recipient’s comparison group consumption (the mean and the 20th percentile of the group).

the letter, that would suggest that they were learning something useful from the itemized contribution records.

The randomization of the list of neighbors eliminates the concern for reverse causality, but this is not enough to solve the issue of omitted-variable bias. The orthogonality assumptions $E[\Delta f_i^j \cdot \varepsilon_i] = 0 \forall j = 1, \dots, J$ imply that there are no omitted dimensions of the information about the list (f_i^{J+1}) that can affect the recipient’s contributions while at the same time are correlated to the J characteristics included in the regression equation. For instance, recipients may care about both the mean contribution amount and the maximum contribution in the list, which are likely to be correlated. Including only the mean in the regression equation would yield a biased estimate, because its coefficient would pick up part of the effects of the maximum contribution. However, it should be noted that this type of bias would also arise if we had randomly assigned simpler messages such as “the average contribution in your neighborhood was \$500,” because the recipient could use this information to update her beliefs about both the mean and the maximum contribution. An advantage of our research design is that, as econometricians, we observe the same information as our subjects, and thus we can include in the analysis additional dimensions f_i^{J+1} which we believe might be driving the results.³¹

Last, note that if individuals care about the contribution behavior of others, the equilibrium distribution of contributions will depend on how individuals form their perceptions about the behavior of others. This question is particularly relevant to disclosure policies; disseminating objective information, for instance, could correct biases in the formation of beliefs. To explore this hypothesis, we randomized an additional feature of the List letter (List-Once vs. List-Update). Due to space constraints, these additional features of the experimental design, and the results they yielded, are presented in Appendix C.3.3.

3 Data Sources and Implementation of the Field Experiment

3.1 Subject Pool and Data Sources

Our subject pool was based on a subsample of the FEC contribution records, specifically some 280,456 individuals who had made over \$200 in contributions to a presidential campaign committee from April 1, 2011 to April 1, 2012, drawn from the online FEC records as of April 25, 2012. While the FEC’s records are remarkably comprehensive, there were some instances

³¹For instance, Appendix C.3 presents additional robustness checks of these results to the inclusion of other plausible characteristics of the lists.

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 adopted a conservative approach and limited the subject pool to those individuals for whom the highest quality information was available (e.g., quality of address information). We applied a number of additional arbitrary criteria, such as excluding contributors from Washington D.C. and those geographically isolated from other contributors (for more details, see Appendix B). After applying these criteria, our final subject pool consisted of 191,832 individuals.³² Appendix B provides descriptive statistics, and also shows that this subject pool was highly representative in observables of the universe of individuals who contributed during the 2012 presidential campaign.

Of the 191,832 contributors in the subject pool, 91,998 were randomly assigned to be sent a letter: 36,773 were sent a Website letter, 36,795 a List letter, and 18,430 a Placebo letter.³³ Within each treatment arm, we randomly assigned them to the sub-treatments described in the previous section (e.g., Website-Self and Website-Neighbors). We refer to the 99,834 individuals who were not assigned a letter as the No-Letter group. The random assignment was conducted at the household level and was stratified at the 3-digit ZIP code (ZIP-3) level.³⁴ Appendix B shows that the treatment groups are balanced in observable pre-treatment characteristics, as we would expect from the random assignment to treatments. Finally, since contributors to the Rand Paul primary (constituting 12% of the original sample) made virtually no contributions in the post-treatment period to the Romney presidential campaign, our baseline results exclude this group. The results are robust to inclusion of this group in the analysis, as shown in Appendix C.1.3.

3.2 Timing of the Experiment and Outcomes of Interest

The letters were sent on May 6, 2012, four days after the Republican National Committee had declared Mitt Romney the party’s presumptive nominee. The outcome variable was simplified by sending the letters once each party had a single presidential candidate, which meant we did not have to compare contributions from the same individual to different candidates.

³²The sample also excludes 1,002 individuals who were sent letters later deemed undeliverable or redirected by USPS. We took several measures to clean the address information in the FEC database, including geocoding, crosschecking an individual’s information across different records, and matching the data with the USPS National Change of Address database. Our mailing provider indicated that about 5% of letters are undeliverable even when address databases are carefully cleaned, so our efforts along these lines can be considered successful. The results are robust to alternative treatments of those observations.

³³We did not conduct a pilot, because otherwise we would have needed to wait for four years until the following presidential election to conduct the experiment.

³⁴That is, all household members were assigned to the same treatment group. About 96% of the households in the subject pool included only one contributor.

Unless stated otherwise, the outcomes of interest throughout our study are the individual campaign contributions made to the Obama or Romney committees from the estimated mail delivery until the official end of the election cycle, that is, December 31, 2012 (in practice, there were virtually no contributions shortly after election day, November 6).³⁵ We called these “post-treatment” contributions. The “pre-treatment” contributions, which were used in falsification tests, correspond to total contributions made between April 1, 2011, and the date when the letters were delivered.

In the pre-treatment period, 52% of individuals in the No-Letter group contributed to Obama and the remaining to Republican candidates.³⁶ The average amount contributed during the pre-treatment period was \$524. During the post-treatment period, 48.9% of our subjects made at least one contribution. For those who made contributions during the post-treatment period, the average amount contributed was \$587. When the dependent variable in the regressions presented below is amount of post-treatment contributions, we use an interval regression model to take into account the censored nature of this outcome.³⁷ As shown in Appendix C, the results are very similar using other regression models. For more details about the contribution patterns of subjects, see Appendix B.2.

3.3 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 conducted a mail-in survey with a subsample of our subjects with two goals. The first goal was to provide some descriptive evidence to complement the experimental results. The second goal was to adjust and aid the interpretation of the magnitudes of the effects identified by our mailing experiment, which are attenuated by the fact that only a minority of our subjects may have actually read the letter that was sent to them (details to follow in Section 4.1.3).

The survey included five questions about knowledge of campaign finance law and a final subjective question about how much an individual should contribute to presidential campaigns. The envelope contained a letter, a survey questionnaire, and a prepaid business-reply envelope. Recipients were asked to fill out the survey and mail it back in the envelope pro-

³⁵See Appendix B for details on how we define the date of delivery for each individual according to USPS records. Our main results are robust if we define pre- and post- contributions according to the date when the letters were mailed rather than delivered.

³⁶For individuals in the No-Letter group, we defined the date dividing pre- and post-treatment contributions as the median date when other letters were delivered in their 5-digit ZIP code.

³⁷Note that if a Republican did not make a pre-treatment contribution to Romney, she may appear as not having made a post-treatment contribution to Romney if that contribution is below \$200 (that is, the FEC threshold). We use the interval regression model instead of a simple censored regression model to take that information into account.

vided.³⁸ We sent the envelopes on December 6, 2012, one month after the date of the 2012 presidential election, because we did not want any of the information contained in the letter to contaminate the effects of the letters sent in the experiment. The intended recipients, 44,380 in total, were a random sample of individuals from the No-Letter and Website groups. We received 9,414 responses, which implies a response rate of 21.21% (this response rate was statistically indistinguishable between subjects from the No-Letter and the Website letter treatment groups). Appendix D contains the survey instrument and provides further details on this survey and its response rate.

4 Experimental Results

4.1 Results: The Conformity Channel

4.1.1 Conformity Channel: Main Results and Robustness Checks

The evidence about the conformity channel is based on the sample of subjects who were sent letters of the Website type. The estimation is given by equation (1), in which *Higher Visibility* is a dummy that takes the value of 1 if the subject (the recipient of the letter) was assigned to the Website-Neighbors letter and 0 if the subject was assigned to the Website-Self letter. *Share Own-Party* in turn is the proportion of the recipient’s neighbors who support the recipient’s party, defined as the share of contributors to the subjects’s party over the three previous presidential campaigns who reside in the recipient’s ZIP-3 area.³⁹

Figure 2 depicts the effect of *Higher Visibility* on the probability of making a contribution in the post-treatment period for different values of *Share Own-Party*, in the spirit of a partial regression plot. Each dot corresponds to one decile of the distribution of *Share Own-Party*, with its position in the horizontal axis corresponding to the mean value of *Share Own-Party* in that decile. Thus, the horizontal dispersion of the dots illustrates the support of *Share Own-Party*. For each dot, the position in the vertical axis corresponds to the average effect of the *Higher Visibility* intervention on the probability of making post-treatment contributions within the corresponding decile of *Share Own-Party* (see the notes to the Figure for estimation details). The results are consistent with the prediction of the conformity channel: when the majority of a subject’s neighbors support her party, higher visibility increases the probability that she will make a contribution; when supporters are evenly split between the two parties, higher visibility has no effect on the likelihood of contributing; and when most of a subject’s

³⁸As an incentive for participation, we included prizes awarded by lottery to individuals who mailed in the completed survey before January 31, 2013 (for details, see Appendix D).

³⁹The results are robust to using alternative measures of *Share Own-Party*.

neighbors support the opposite party, higher visibility decreases the probability of a subject making a contribution. Moreover, this partial regression plot suggests that the effect of *Higher Visibility* increases roughly linearly with *Share Own-Party*, which validates the linear specification used in the rest of the analysis.

Table 1 presents the baseline results in regression form, along with a number of robustness checks. The results in column (1) present the effects of the higher visibility treatment on the amount contributed during the post-treatment period. The negative coefficient on *Higher Visibility* indicates that the treatment reduces participation in areas where everyone supports the party that the recipient opposes, while the significant positive coefficient on the interaction between *Higher Visibility* and *Share Own-Party* indicates that the effect of the higher visibility treatment is more positive (or less negative) when *Share Own-Party* is higher. These coefficients can be used to estimate the effects of the higher visibility treatment in areas with different values of *Share Own-Party*. For instance, a representative example of a highly polarized area is given by a ZIP-3 where 75% of contributors support the majority party and thus 25% of contributors support the minority party. Our results indicate that, in such areas, higher visibility would reduce by about \$53 the amount contributed by supporters of the minority party and increase by about \$19 the amount contributed by supporters of the majority party. These effects are not only statistically, but also economically, significant. The effects of \$53 and \$19 constitute respectively 9.0% and 3.2% of the average amount contributed by subjects who made further contributions during the post-treatment period (\$587). These estimates may severely underestimate the true effects of visibility, for example, due to the possibility that only a minority of subjects read the fliers that were sent to them. To provide a more accurate assessment of the magnitude of the effects, Section 4.1.3 below discusses and addresses some of these issues.

Column (7) in Table 1 reproduces the results from column (1) but with the probability of making at least one post-treatment contribution, rather than the amount contributed, as the dependent variable. The sign and statistical significance of the coefficients are consistent with the results from column (1). There are, however, some differences in terms of magnitude. In areas where 75% of neighbors support the majority party, the higher visibility treatment reduces the probability of contributing by 1.86 percentage points among supporters of the minority party and increases the probability of contributing by 1.47 percentage points among supporters of the majority party. These effects of 1.86 and 1.47 percentage points represent respectively 3.3% and 2.6% of the baseline contribution rate of 55.7%. It would appear that the effects on the decision to contribute, although still significant, were smaller in magnitude than the effects on the amount contributed (i.e., 9.0% and 3.2% of the average amount contributed, as shown above).

A simple and straightforward way to check the randomness of treatment assignment is to compute the “effects” of our experiment on pre-treatment, rather than post-treatment, contributions. Column (8) in Table 1 presents the results from this falsification test. As expected, the higher visibility treatment had no “effect” on pre-treatment contributions: the estimates of the coefficients on *Higher Visibility* and on its interaction with *Share Own-Party* are very close to zero, not statistically significant and precisely estimated.

One potential concern with our findings is that the heterogeneous effects of higher visibility by partisan alignment may reflect heterogeneity in other area characteristics that are correlated with partisan alignment. Columns (2) and (3) from Table 1 present some additional robustness checks regarding this potential concern. Column (2) in Table 1 presents the results of a regression model similar to the one presented in column (1), but including the interaction between *Higher Visibility* and the share of individuals of the same race as the recipient’s in her ZIP-3 (using two categories, white and non-white), instead of the share of individuals from the same party. The results suggest that there is no significant heterogeneity in the effect of *Higher Visibility* with respect to *Share Own-Race*: the coefficient on the interaction (-32.99, SE 45.76) is close to zero, not statistically significant, and more precisely estimated than (and statistically significantly different from) the interaction with *Share Own-Party* reported in column (1).⁴⁰

The share of Democrats (or Republicans) in a given ZIP-3 is significantly correlated to a number of characteristics of the area’s population such as average income, race and education. If our analysis focused on the interaction between *Higher Visibility* and *Share Democrat*, then there would be potential for omitted variable bias from these other ZIP-3 characteristics. However, our analysis is based on the alignment of the subject with respect to the political composition of her area. This difference is subtle but important, because the same ZIP-3 characteristics that are correlated to *Share Democrat* (or *Share Republican*) are only weakly correlated with the alignment variable, *Share Own-Party*, leaving less room for potential omitted variable biases. For instance, let *Share Low-Income* be the share of low-income households in the recipient’s area. The correlation between *Share Democrat* and *Share Low-Income* is 0.275 (p-value<0.01), whereas the correlation between *Share Own-Party* and *Share Low-Income* is only 0.033 (p-value<0.01). The regression presented in column (3) modifies the baseline model shown in column (1) by including an interaction between *Higher Visibility* and *Share Low-Income* instead of the interaction with *Share Own-Party*. As expected, the coefficient on the interaction with *Share Low-Income* is close to zero and not statistically

⁴⁰From the FEC data, we could also use our proxy for gender to construct a variable reflecting the share of individuals of the same gender in the same ZIP-3. However, there is very little variation in this variable across ZIP-3s, and it is thus impossible to identify the coefficient on this interaction.

significant.⁴¹ Furthermore, we find similar results when we include interactions with other characteristics of the recipient’s area of residence, such as the share of college graduates and married individuals (results reported in Appendix C).

As an additional falsification test, we measure whether the effect of the Placebo letter, a treatment not expected to have heterogeneous effects, was in fact heterogeneous with respect to *Share Own-Party*. If found, this would suggest that the corresponding heterogeneity of the *Higher Visibility* treatment could be mediated by a confounding factor. As described above, the Placebo letter has the same contextual information and format as the Website letter, but it provided regulatory information about contribution limits that was largely irrelevant for our subjects. In column (4) of Table 1, we present the results from a specification where we replace the *Higher Visibility* with the *Placebo* treatment indicator. As expected, there is no significant heterogeneity with respect to *Share Own-Party* in the response to the *Placebo* letter: the coefficient on the interaction between *Placebo* and *Share Own-Party* (-8.99, SE 46.75) is close to zero, not statistically significant, precisely estimated, and also statistically different from the corresponding coefficient from column (1) on the interaction between *Higher Visibility* and *Share Own-Party* (143.50, SE 60.30).

We can also explore the timing of the effects of higher visibility. Columns (5) and (6) from Table 1 present estimates of the effect of higher visibility on the post-treatment amount contributed before and after September 1, 2012, respectively. This date divides the post-treatment period roughly into two halves of approximately four months and with similar average contribution amounts. If the salience of the intervention remained constant over the post-treatment period, we would expect equal effects for the two sub-periods. If, instead of finding constant effect, we found stronger effects in the first sub-period compared to the second, this would suggest that the higher visibility intervention was gradually forgotten by the recipients, or became less salient. Finally, finding no effects for the first sub-period and significant effects in the second would suggest that the effects are actually spurious – there would be no obvious explanation for such pattern. The results from columns (5) and (6) suggests that the effects of the higher-visibility treatment faded, at least gradually, over time. For instance, the coefficient on the interaction between *Higher Visibility* and *Share Own-Party* decreases by 73% (i.e., from 149.04 to 40.84) from the first to the second half of the post-treatment period, and this difference is statistically significant (p-value<0.10). This finding also implies that the magnitude of the effects of visibility reported in this paper would be substantially larger if we focused our analysis on the short-term reactions.

⁴¹The estimate of this coefficient is less precise than the estimate of the coefficient on the interaction of *Higher Visibility* and *Share Own-Party*. This is due to the fact that the share of low-income households varies less across ZIP-3s than the share of supporters of the recipient’s party.

Finally, Appendix C presents a number of additional robustness and specification checks, such as showing that the results are very similar when clustering the standard errors and when using Tobit or Poisson regressions instead of the Interval regressions.

4.1.2 Conformity Channel: Potential Confounding Factors

While the impact of our higher visibility treatment is consistent with the presence of conformity effects, there are some alternative interpretations of our results worthy of discussion.

The first is that individuals use campaign contributions to send signals about characteristics that are not of a partisan nature – for instance, to signal wealth, generosity or level of civic engagement. There are, however, more efficient ways to signal those traits, such as buying an expensive car to signal wealth, or making non-anonymous charitable contributions to signal altruism. However, even though non-partisan signaling may exist, it could not explain our findings. For instance, non-partisan signaling cannot explain the heterogeneity in the effect of higher visibility with respect to *Share Own-Party*. In particular, if individuals were signaling income or altruism then higher visibility should always result in higher contributions. Thus, non-partisan signaling could not explain our finding of a negative effect of higher visibility on contributions for supporters of the local minority party.

A second alternative explanation for the effects of higher visibility could be leading-by-example. According to this conjecture, individuals give more if they feel observed by neighbors because they believe that others will follow their lead and, in turn, contribute more themselves. This is unlikely to be driving our results for at least two reasons. First, based on our evidence on comparison effects, an individual’s contribution is expected to have a small effect on the contributions of others.⁴² Second, it is not obvious that leading-by-example could explain the heterogeneity with respect to *Share Own-Party*. When the share of own-party neighbors is zero, then there would be no one to follow the lead and thus the effect of higher visibility should be zero. In this case, leading-by-example would not be able to explain our finding of a negative effect of higher visibility on contributions for supporters of the local minority party.

⁴²A simple numerical example can illustrate this point. Let us start with a number $N - 1$ of own-party contributors in a given area. If a recipient in this area increased her own contribution by \$100, the average contribution for that party in the area would go up by $100/N$. If the recipient thinks that the letters were sent to all of the other $N - 1$ own-party contributors in her area (a conservative assumption), the expected effect on total contributions by neighbors would be: $0.0295 * 100 * (N - 1)/N$, where 0.0295 is the coefficient on \bar{c}_{own} from column (1) of Table 3. With a price-elasticity of giving of 1, this mechanism would predict TOT effects of around 1.5%-3%, which is an order of magnitude lower than our results.

4.1.3 Conformity Channel: Assessing the Magnitude of the Effects

In this subsection, we discuss the possibility that our previous results significantly underestimate the effects of higher visibility and provide some plausible alternatives with which to estimate the true magnitude of the effects.

The first consideration is that our estimates capture the effect of visibility following a relatively minor intervention: a letter on campaign contributions that provides a limited number of neighbors with information on how to access the FEC’s search tool. However, due to the existence of the FEC search tool, individuals may potentially feel observed by a much greater number of peers. Second, exactly who could potentially observe one’s contributions is, in all likelihood, a very important aspect of the conformity channel. We estimate the effect of an increase in visibility among neighbors, but individuals could arguably care more about their social interactions – and, hence, visibility – among friends, relatives, coworkers and employers, who may largely live in other neighborhoods.⁴³ Third, most recipients of our mailing appeared in the FEC search tool before receiving our letter, which means that there was already some publicly available information about which party the individual supported.⁴⁴ The higher visibility treatment affected the marginal contribution even for this sample.⁴⁵

Last but not least, the results provide estimates of the effect of having been mailed a letter with certain information, which we denominate the Intention to Treat (ITT) effect. To assess the importance of higher visibility would require estimating the effect of reading the letter, which we denominate the treatment on the treated (TOT) effect. The ITT effects can be scaled up to TOT effects using the inverse of the reading rate r (i.e., the proportion of recipients who actually read the letters we sent): $TOT = \frac{1}{r}ITT$. A substantial share of experimental subjects – probably a majority – may not have read the letters we sent them: our mailing was sent in the middle of the presidential campaign, when potential voters, especially those who had made contributions before, were being flooded by large amounts of unsolicited physical and electronic mailings soliciting campaign contributions and providing information about the candidates and the election. While we attempted to make our mailpiece stand out,

⁴³In an ideal experiment, we would design an intervention based on an increase in visibility among an individual’s reference group constructed from her social networks. Since such data is not available, we relied instead on a geographic proxy, a common feature in the literature on social interactions (for a discussion in the context of consumption signaling, see Perez-Truglia, 2013).

⁴⁴There is a lag between the itemized contribution records, which we used to form our subject pool, and the appearance of those contributions in the FEC’s online search tool database. It is possible, then, that a recipient of one of our letters did not appear on the search tool database until some time (approximately one month) after receiving our mailing.

⁴⁵An additional contribution would still have an effect on the perceptions of neighbors insofar as it would make party affiliation even more salient in the search results and signal higher commitment to a party. The effects of higher visibility should be much stronger, however, among individuals for which an additional contribution would take them above the \$200 disclosure threshold.

so did the candidates' campaign committees. In particular, for cost reasons, our mailpiece consisted of a folded flyer, which tend to stand out less than letters in regular envelopes.

For our statistical power calculations, we consulted mass-marketing experts who provided us with estimates for our mailpiece's expected reading rate. These estimates ranged from 10% to 25%, which explains why we used such a large sample for conducting the experiment. Relatedly, the Environmental Protection Agency (EPA) indicates that about 50% of unsolicited mail is discarded before being opened, which provides a conservative upper bound on the reading rate. Visitors to our project's website account for 5% of the letters we sent, which in turn provides a conservative lower bound for the reading rate. 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. This broad range was one of the motivations to conduct the post-election mail-in survey. One of its main goals was to provide a more precise and objective estimate of the reading rate.

Since the primary piece of information in our Website letter was the public nature of contribution records, we would expect an individual who had read our letter to have a greater awareness of the public nature of contribution records. Figure 3 compares the distribution of beliefs about the public nature of contribution records between survey respondents who were selected not to receive any letter (the No-Letter group) and recipients of our Website letters. As expected, respondents who had received a Website letter were significantly less likely to report being unsure about the public nature of contribution records. More precisely, the share of respondents who were unsure about the public nature of contribution declined from 19.2 percentage points in the No-Letter group to 15.8 percentage points in the Website group. Assuming that a subject who had read the letter would always report certainty about the disclosure policy, that difference of 21.5% implies a reading rate of $r = 0.215$ (with a 90% confidence interval of 0.146-0.284). Reassuringly, this estimate of the reading rate is within the range of estimates provided by our mass-mailing experts.⁴⁶

We can use the estimated reading rate to scale-up the magnitude of the effects of higher visibility. Our results indicated that, in an area where 75% of the population supports one party, the higher visibility treatment induced a drop in the amount contributed post-treatment of 9.0% of the mean amount for recipients supporting the local minority party, and an increase of 3.2% for recipients supporting the local majority. These effects, while significant, are not exceedingly large. The scale-up factor of 4.6 (i.e., $\frac{1}{0.215}$) implied by the reading rate, however, indicates that the TOT effects were substantially larger: -41% (i.e.,

⁴⁶The response rate to the mail-in survey, 21.2%, suggests that its reading rate was probably much higher than that of the original treatment letters. This difference can be explained by the very different conditions under which the mail-in survey was sent – i.e., after the presidential election (see Appendix D.2 for a detailed discussion).

-9.0% \times 4.6) and 15% (i.e., 3.2% \times 4.6), respectively.⁴⁷ Even with a reading rate twice as high, the effects of higher visibility would still be very large. Appendix D.2 presents a more formal discussion of the implicit assumptions in this exercise, and it discusses some qualification, robustness checks and alternative estimators.

4.2 Results: The Comparison Channel

The evidence about the comparison channel is based on the sample of subjects who were sent letters of the List type. The estimation is given by equation (2), which consists of regressing the post-treatment contributions on the characteristics of the table of contributors included in the letter: the average amount contributed to the recipient’s own party (\bar{c}_{own}) and to the opposite party (\bar{c}_{opp}), as well as the number of contributors to the recipient’s party on the list (N_{own}). As explained in Section 2.2.2, the coefficients on these variables are identified using only the exogenous variation created by our random assignment procedure.

Figure 4 presents the basic evidence in graphical form, using the same type of partial regression plots used in the previous section.⁴⁸ Figure 4.a corresponds to the effect of \bar{c}_{own} , holding constant \bar{c}_{opp} and N_{own} . The evidence suggests that recipients contribute more the higher the mean of contributions from supporters of their own party. Figure 4.b corresponds to the effect of N_{own} , holding constant \bar{c}_{own} and \bar{c}_{opp} . This evidence suggests that individuals contribute less when the list we mailed had a larger number of contributors to their own party. Furthermore, these results suggest that using a linear specification is a good approximation for the relationship between contributions and both \bar{c}_{own} and N_{own} .

Table 3 presents further regression results. The specification in column (1) includes as independent variables the average amount contributed to the recipient’s own party (\bar{c}_{own}) and the average amount contributed to her opposite party (\bar{c}_{opp}). These independent variables were defined in hundreds of dollars. 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 \$2.95 (p-value<0.05). While we cannot discard other interpretations, this evidence is consistent with the models of social norms, which predicts that individuals contribute more if they perceive that similar individuals (i.e., geographically close individuals supporting the same party) contributed higher average amounts. In contrast, the coefficient

⁴⁷This estimate corresponds to the split-sample instrumental variable estimation (Angrist and Krueger, 1992). Following Dee and Evans (2003), we can compute standard errors using the Delta method: the TOT effects of -41% and 15% have standard errors of 19.9% and 10.6%, respectively. These standard errors estimates 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.

⁴⁸This figure use the quintiles of the distributions of \bar{c}_{own} and N_{own} , with the coefficients for the middle categories normalized to zero.

on the contributions of neighbors who support the opposite party indicates that an increase of \$100 in \bar{c}_{opp} has a non-significant effect on the subject’s contribution (\$0.06, p-value>0.10). Moreover, the difference between the two coefficients is statistically different from zero at standard levels (p-value=0.096). This finding is also consistent with identity theories (Akerlof and Kranton, 2000), according to which an individual does not follow the behavior of peers with whom that individual does not identify.

The economic significance of the social norms effects can be qualified. Like conformity effects, social norms effects are intention to treat estimates, since we do not know which recipients actually read the letter. According to the calculations in Section 4.1.3, the treatment on the treated effect may be 4.6 times greater than 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 \$13.57 (i.e., 2.95×4.6).⁴⁹

To provide complementary evidence on the economic significance of social norms, we included a question in the post-election survey intended to quantify the respondent’s perception of the contribution norm. This question asked how much an individual earning an average income *should* contribute to a presidential campaign (question 8 in the survey’s questionnaire, presented in Appendix A.7). By matching the responses to this question to the FEC records, we can measure the relationship between the perceived social norm and the actual contributions made by respondents during the 2012 presidential campaign. Figure 5 depicts this relationship for responses from individuals in the No-Letter group. As expected, there is a significant positive relationship between the perceived contribution norm and actual contributions: a \$100 increase in the perceived norm is associated with an extra \$11.21 (p-value<0.01) in contributions. Of course, this observational evidence is subject to a number of identification challenges. With that caveat in mind, this auxiliary result suggests that the effects of social norms in a non-experimental setting are of the same order of magnitude than those suggested by the experimental evidence.

While social norms is our preferred explanation of our experimental findings, there are alternative interpretations. Contributors may, for example, react to the average contributions of others as a function of some “optimal” contribution amount given by a certain goal (e.g., making a pivotal contribution, buying a future favor from a politician, etc.).

Table 3 presents additional results on the comparison channel. Column (2) presents

⁴⁹It should be noted that, in fact, this is just the effect of the information provided, $\bar{c}_{own}^{provided}$, on the recipient’s contribution, c . A more relevant structural parameter is the effect of the perceived social norm, \bar{c}_{own}^{norm} , on c . The latter effect is indirect, since individuals do not necessarily adjust their posterior beliefs about the social norm solely in response to the information provided. Our estimated effect, $dc/d\bar{c}_{own}^{provided}$, results from the multiplication of two effects: $dc/d\bar{c}_{own}^{provided} = dc/d\bar{c}_{own}^{norm} \times d\bar{c}_{own}^{norm}/d\bar{c}_{own}^{provided}$, where $d\bar{c}_{own}^{norm}/d\bar{c}_{own}^{provided}$ represents the learning rate. Since this learning rate is expected to be somewhere between 0 and 1, the structural parameter of interest, $dc/d\bar{c}_{own}^{norm}$, is a multiple of the effects reported here ($dc/d\bar{c}_{own}^{provided}$).

the results from a specification that includes as an independent variable the number of individuals on the list who contributed to the recipient’s party (N_{own}), in addition to the average amounts contributed to the recipient’s own and to the opposite party, \bar{c}_{own} and \bar{c}_{opp} . Social norms theory predicts that a higher value of N_{own} should increase the recipient’s contribution because individuals feel pressured to behave like the majority. The negative and statistically significant coefficient on N_{own} in column (2) suggests that, to the contrary, the effect operates in the opposite direction, which could be interpreted as a form of free-riding. For each additional individual supporting the same party on the list, the recipient reduced the amount of her contribution by \$5.44 (p-value<0.10). The magnitude of this effect is equivalent to the effect of decreasing the mean contribution of own-party neighbors by \$170.⁵⁰

As with conformity effects, a first robustness test is to check the randomness of the treatment assignment by estimating the “effects” of our experiment on pre-treatment contributions. The specification in column (6) of Table 3 presents the results of this falsification test. As expected, all of the coefficients are close to zero and not statistically significant. This evidence is consistent with the assumption that the effects are identified by the experimental assignment (see Appendix C.3 for additional robustness checks).

We can quantify the effects of the information about contribution patterns on the extensive margin of contributions as well. The specification in column (5) from Table 3 is the same as in column (2), with the only difference that the dependent variable is the probability of making at least one post-treatment contribution. The relevant coefficients from column (5) are small in magnitude and not statistically significant. This result suggests that the comparison channel is significant for the intensive margin but insignificant for the extensive margin of contributions.

Columns (3) and (4) in Table 3 consider the effects on the amount contributed during the two post-treatment sub-periods: before and after September 1, 2012. The estimates suggest that \bar{c}_{own} had a lasting effect: the coefficient is similar for the two sub-periods (1.75 and 2.48), and their difference is not statistically significant. This could be evidence that our letter had a lasting effect on the contribution norm. The effect of N_{own} , however, only lasted for the first half of the post-treatment period: the coefficient on N_{own} is statistically significant during the first sub-period (-6.19, p-value<0.01) but close to zero and not statistically significant during the second half of the post-treatment period (-0.47, p-value>0.10), and the difference between the two is statistically significant. One potential explanation for this finding is that, as the

⁵⁰The effects of the number of contributors supporting the same party (N_{own}) and of the average amount contributed to that party by neighbors on the list (\bar{c}_{own}) are similar in magnitude, in the sense that the impact of a one standard deviation increase in N_{own} (corresponding to the non-experimental variation in this variable) is similar in magnitude to the impact of a one standard deviation decrease in \bar{c}_{own} .

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.

Because of space constraints, the additional evidence is presented in the Appendix. Appendix C presents a number of additional robustness checks, such as showing that the results are similar clustering the standard errors and using the Tobit or Poisson instead of the Interval regression model, as well as additional results.

5 Counterfactual Analysis of the Conformity and Comparison Effects on Geographic Polarization

5.1 Implications of the Conformity Channel

The findings in the previous section indicate that the conformity channel did not affect contributions on average, but that it can affect the distribution of contributions across geographic areas. More precisely, the higher visibility treatment increased the contributions of supporters of the local majority party, and decreased those of supporters of the minority party. This implies that if in a given geographic area 60% of individuals self-identify as Republican and 40% as Democrats, the conformity channel can induce contributions that are even more uneven than 60%-40%. In this section, we present some back of the envelope calculations of the counterfactual distribution of contributions in a scenario with no conformity effects, based on the experimental estimates.

Let P_j^k be the probability that an individual from ZIP-3 area $j \in \{1, 2, \dots, J\}$ makes a contribution to a presidential candidate of party $k \in \{DEM, REP\}$, and let \tilde{P}_j^k be the corresponding probability in the counterfactual scenario with no conformity effects. Let $v \in [0, 1]$ be the probability that a randomly selected neighbor from the same ZIP-3 area observes another neighbor's campaign contributions. We can parametrize the conformity effects in a simple way:

$$P_j^k = \tilde{P}_j^k \left(1 + \gamma_1 \cdot v \cdot \left(\frac{\tilde{P}_j^k}{\sum_k \tilde{P}_j^k} - \gamma_0 \right) \right), \quad \forall i, j, k \quad (3)$$

where the parameter $\gamma_1 > 0$ represents the intensity of conformity effects, and the parameter $\gamma_0 \in [0, 1]$ represents their degree of symmetry.⁵¹ The symmetric case, $\gamma_0 = \frac{1}{2}$, corresponds

⁵¹More precisely, we should be using $\tilde{P}_{-i,j}^k$, the probability of contributions excluding the i 's own, instead of \tilde{P}_j^k . This approximation has virtually no effect since there are hundreds of thousands of individuals in

to the patterns we observe in the data: when neighbors are evenly split between the two parties, the conformity channel does not affect contributions; but in areas with more uneven distributions of political preferences, the conformity channel increases contributions to the majority party and decreases contributions to the minority party.

Let P and \tilde{P} denote the vectors with all P_j^k 's and \tilde{P}_j^k 's. Given $\{P, \gamma_0, \gamma_1 \cdot v\}$, we obtain the counterfactual probabilities \tilde{P} by numerically solving the system of nonlinear equations given by (3). Thus, this counterfactual analysis simply requires estimates for $\{P, \gamma_0, \gamma_1 \cdot v\}$ – note that we only need the product $\gamma_1 \cdot v$ and not each of these parameters separately.

We use the vector of observed shares of contributors in each ZIP-3 during the 2012 presidential election as estimate of P , and we estimate the values of γ_0 and $\gamma_1 \cdot v$ using the experimental findings. Our results on the conformity channel amount to the effects of an intervention which increased visibility by some unknown degree Δv . The value of γ_0 corresponds to the value of *Share Own-Party* for which the implied effect of an increase in visibility would be zero. The results from column (7) from Table 1 thus imply an estimate of $\gamma_0 = 0.53$ (i.e., $-3.53 + 6.67 \cdot 0.53 = 0$). In turn, the parameter γ_1 measures the difference in the effect of higher visibility on the probability of contributing between the extreme cases of areas with all own-party neighbors and with all opposite-party neighbors. This parameter can be approximated by the coefficient on the interaction between *Higher Visibility* and *Share Own-Party*, scaled-up by the implied effect of *Higher Visibility* on the perceived visibility, Δv .⁵² Relying again on the results from column (7) from Table 1, we obtain $\gamma_1 = \frac{6.67}{56 \cdot 0.215 \cdot \Delta v} = \frac{0.55}{\Delta v}$. In turn, this estimate implies $\gamma_1 \cdot v = 0.55 \cdot \frac{v}{\Delta v}$. Thus, the only remaining unknown to complete the counterfactual analysis is $\frac{\Delta v}{v}$, the proportional increase in visibility which resulted from our higher visibility treatment. We do not have a direct measure of $\frac{\Delta v}{v}$ nor of either of its components, so we present some baseline results based on the case of $\frac{\Delta v}{v} = 1$ (i.e., our intervention doubled the visibility of contributions among neighbors). We then discuss these results under alternative assumptions.⁵³

Figure 6.a presents the actual and counterfactual distributions of contributors as a func-

each ZIP-3 area. We are also implicitly assuming that the parameters are bounded such that the resulting probabilities are between zero and one.

⁵²Note that our experimental estimates correspond to the probability of making a post-treatment contribution conditional on having contributed during the first half of the election (the pre-treatment period). Instead, this counterfactual analysis is based on the unconditional probability of making a contribution. This exercise is then relying on the assumption that, in proportional terms, the magnitude of the conformity effects is similar between these conditional and unconditional probabilities.

⁵³The visibility could have increased due to several factors – for instance, because of an increase in neighbors' awareness of the FEC online search tool, but also because of a higher salience of this information. We are also making a series of additional implicit assumptions: e.g., we do not take into account the potential equilibrium effects that can arise from a signaling model, and we are not dealing explicitly with individuals who contribute to both parties.

tion of the share of contributors to the Democratic party. The solid bars represent the histogram of the actual shares of Democrat contributors across ZIP-3s. The dispersion of this variable is intrinsically related to the degree of geographic clustering of contributors (i.e., the extent to which active Democrats are located near other active Democrats), which is also known as geographic polarization.⁵⁴ The main driver of this geographic polarization of contributors is the sorting of individuals into areas with a higher share of like-minded peers. Additionally, once individuals are sorted into geographical areas, the conformity effects can exacerbate polarization by increasing the participation of supporters of the local majority and reducing the participation of the local minority.

The hollow bars from Figure 6.a correspond to the distribution of contributors in the counterfactual scenario with no conformity effects, assuming $\frac{\Delta v}{v} = 1$. The mean of the distribution is roughly the same (0.55) in the factual and counterfactual scenarios. However, the dispersion of the actual distribution is significantly larger than that of the counterfactual scenario with no conformity effects: the standard deviation of the distribution increases by 20% (i.e., from 0.142 to 0.170) due to the conformity effects. In other words, the conformity channel exacerbates the geographic polarization by 20%. Coincidentally, this is in the same order of magnitude of the effects of the conformity channel estimated with observational data: based on an event study of geographically mobile contributors, Perez-Truglia (2014) finds that conformity effects increase geographic polarization of contributors by 27%.

These baseline results depend on the assumption that $\frac{\Delta v}{v} = 1$. In fact, the counterfactual change in polarization is close to inversely proportional to the value of $\frac{\Delta v}{v}$. For instance, if we assume a value of $\frac{\Delta v}{v}$ half as large, the effect on polarization is about twice as high (40%), and under the assumption that $\frac{\Delta v}{v}$ is twice as large, the effect on polarization is about half as high (10%).

5.2 Implications of the Comparison Channel

In this section we provide a similar counterfactual analysis for the comparison channel. Bearing in mind that our experimental results indicate significant comparison effects on the intensive margin but not on the extensive margin, we focus on the share of Democrats' contribution amounts rather than on the share of Democrat contributors analyzed in the previous section.

Let $C_{i,j}^k$ be the contribution from individual i in ZIP-3 area $j \in \{1, 2, \dots, J\}$ to a presidential candidate of party $k \in \{DEM, REP\}$, and let $\tilde{C}_{i,j}^k$ be i 's corresponding contribution in the counterfactual scenario with no comparison effects. Let N_j^k be the number of contributors from ZIP-3 area j to the presidential candidate of party k – based on our experimental results,

⁵⁴For a formal discussion of this relationship, see Perez-Truglia (2014). For more information on polarization across space and other dimensions, see Gentzkow, Shapiro and Taddy (2015).

we assume that the number of contributors remains unchanged with or without comparison effects. We can parametrize the comparison effects by means of the following equation:

$$C_{i,j}^k = \tilde{C}_{i,j}^k + \delta_1 \frac{\sum_i \tilde{C}_{i,j}^k}{N_j^k} + \delta_2 \frac{N_j^k}{\sum_k N_j^k}, \quad \forall i, j, k \quad (4)$$

where we assume the parameter δ_1 to be positive, since our results indicated that individuals contribute higher amounts when other contributors from the same party and ZIP-3 contribute higher amounts. The parameter δ_2 , in turn, is assumed to be negative, since our experimental results indicated that individuals contribute lower amounts if there is a higher share of own-party contributors in the same ZIP-3.⁵⁵ Let \overline{C}_j^k and $\overline{\tilde{C}}_j^k$ denote average contributions in the actual and counterfactual scenarios.⁵⁶ We can average equation (4) and solve for $\overline{\tilde{C}}_j^k$:

$$\overline{\tilde{C}}_j^k = \frac{1}{1 + \delta_1} \overline{C}_j^k - \frac{\delta_2}{1 + \delta_1} \frac{N_j^k}{\sum_k N_j^k}, \quad \forall j, k \quad (5)$$

Let C , N and \tilde{C} denote the vectors with the average contributions and number of contributors in all ZIP-3s. We can obtain the counterfactual distribution of contributions \tilde{C} by replacing estimates of $\{C, N, \delta_1, \delta_2\}$ into equation (5). Analogously to the previous section, C and N are estimated by means of the average contribution amounts and the number of contributors in each ZIP-3 during the 2012 presidential election. We can also obtain δ_1 and δ_2 from our experimental results. Based on the results presented in column (2) of Table 3, we use $\delta_1 = \frac{3.20}{100} \cdot \frac{1}{0.215} = 0.149$ and $\delta_2 = \frac{-5.44 \cdot 9}{0.215} = -2.811$.

Note that, unlike the case of the conformity channel, which was expected to increase geographic polarization in contributors, the expected effect of the comparison channel on polarization is ambiguous.⁵⁷ Figure 6.b depicts the distribution of the actual share of Democratic contributions and the counterfactual distribution with no conformity effects. The results suggest that the comparison effects induce a slightly smaller share of Democratic contributions: the actual mean share of Democratic contributions is slightly lower (49.2%)

⁵⁵In principle, our results cannot determine whether individuals care about the contribution patterns in some reference group, such as their neighbors, or whether they simply care about broader (e.g., nationwide) patterns. Since individuals tend to interact more with (and learn more from) those who are closer to them, they are more likely to attribute a greater weight on information about those surrounding them.

⁵⁶As in the previous section, we should base our analysis on $\overline{\tilde{C}}_{-i,j}^k$, the average contribution excluding i 's own, but we make a simplifying assumption and use $\overline{\tilde{C}}_{i,j}^k$ instead. The difference this assumption makes is negligible given the high number of contributors in each area.

⁵⁷The effect from the first term from the right hand side of (5), mediated by δ_1 , has the same impact on contributions from both parties and thus it cannot affect the partisan geographic polarization of contributions. The second term, mediated by the parameter δ_2 , can have a differential impact on individuals identified with the minority and the majority parties, and can thus affect polarization. Its overall effect, however, is ambiguous, because it depends on the joint distribution of the number of contributors and of average contributions for each party.

than in the counterfactual scenario with no comparison effects (51.8%). This effect can be attributed to a stronger free-riding effect for Democrats, given the higher average share of Democratic contributors in the United States. This exercise also indicates that the standard deviation of the share of Democrat contributions increases by just 1.1% in the counterfactual scenario with no comparison effects compared to the actual figure (from 0.275 to 0.278). This result suggests that, unlike the conformity channel, the comparison channel has at most a negligible effect on the geographic polarization of contributions.

6 Conclusions

We presented novel evidence about the importance of partisan interactions for political participation. We found that feeling observed by neighbors significantly increases the contributions of individuals supporting the local majority party, but decreases contributions by supporters of the minority party. We found as well that an individual's contribution is affected by her perception of the contribution behavior of others, possibly due to factors like social norms and free-riding. While our study examines the particular case of campaign contributions, we believe that, except in forms of participation that do not reveal partisanship (e.g., voting), similar partisan interactions take place with other forms of political participation, such as talking about politics, sharing political news, attending rallies, and maybe even registering to vote.

We conclude by discussing some implications of our findings for the disclosure of contribution records. With the advent of the Internet and the proliferation of online services provided by both the public and private sectors, the issues of information disclosure and privacy have become salient topics in the public debate. Nonetheless, there is still limited evidence about those issues and their effects. The requirement that all political contributions be filed with a regulatory agency such as the FEC is key to preventing corruption and to enforcing other campaign regulations. The purpose of making detailed contribution records easily accessible online to the general public, as in the current regulation, is less clear, however. For example, voter turnout records are publicly available, but they are easily accessible online only for a few states.

The primary justification of an open disclosure policy is that voters can use public records to learn about candidates (Gilbert, 2013).⁵⁸ Our evidence suggests that individuals may be using the public records with unintended goals, such as exerting social pressure on neighbors,

⁵⁸In the words of the Supreme Court: "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" (*Buckley v. Valeo*, 1976).

friends, relatives and employees. A boss could, for instance, deny a promotion to an employee who contributed to the opposite party or neighbors could ostracize a household that contributed to the minority party. These conformity effects can be deemed as undesirable for the policymakers because of the chilling effect on the participation by local minorities and, possibly, for the welfare costs created by the stigmatization. Interestingly, these unintended effects were mentioned by the Supreme Court even decades before the contribution records were easily accessible online.⁵⁹ This view of stigmatization is also consistent with some anecdotal evidence, such as the use of FEC records to harass supporters of ballot proposition 8 in California in 2008, and the alleged use of these records by the Internal Revenue Service to target supporters of the Tea party (e.g., Briffault, 2010; La Raja, 2014). Additionally, in Appendix E we exploit some Internet browsing data to provide some suggestive evidence about the possibility of misuses of the FEC search tool and related websites.

Some simple modifications to the current policy could reduce the unintended uses of the FEC records without compromising some of the other goals of the regulation. If the goal of the FEC’s disclosure policy is to allow voters to learn about candidates and to let journalists monitor corruption, specific identifying information about who makes each contribution – information like name, address, employer – should be largely irrelevant, possibly with the exception of large contributors.⁶⁰ Thus, the FEC could restrict the amount of identifying information about contributors that they make easily accessible online for contributions below a certain threshold (e.g., \$2,000).⁶¹ Alternatively, the FEC could impose a small pecuniary or non-pecuniary cost for accessing information about the identity of contributor below a certain threshold. This change in policy would not be a restriction for reporters searching for information on the identity of contributors, but it would presumably discourage individuals using the search tool with the goal of exerting social pressure on friends, relatives and neighbors.⁶²

⁵⁹“Contributors of relatively small amounts are likely to be especially sensitive to recording or disclosure of their political preferences. These strict requirements may well discourage participation by some citizens in the political process, a result that Congress hardly could have intended” (*Buckley v. Valeo*, 1976).

⁶⁰In other words, it is hard to conceive how knowing the names of each individual contributing \$300 could provide any valuable information about the candidate.

⁶¹For instance, they could report initials instead of full names and ZIP codes instead of full addresses.

⁶²For example, the FEC could require individuals to provide their social security numbers when requesting identifiable information about a small contributor, and then let that contributor find out who had requested information about him or her. Indeed, this system seemed to be very successful in Norway, where tax records are easily accessible online. After making the searches non-anonymous, the number of searches plummeted by 88%, presumably because individuals were discouraged from snooping on the incomes of their social contacts (Perez-Truglia, 2015).

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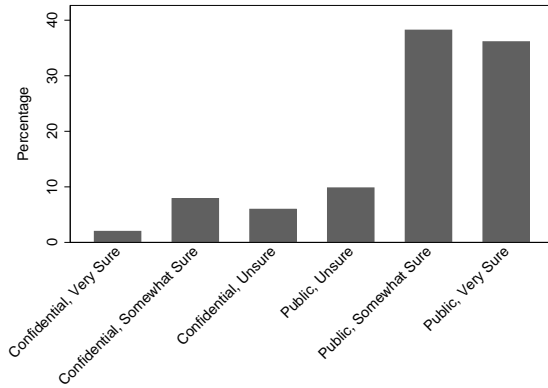
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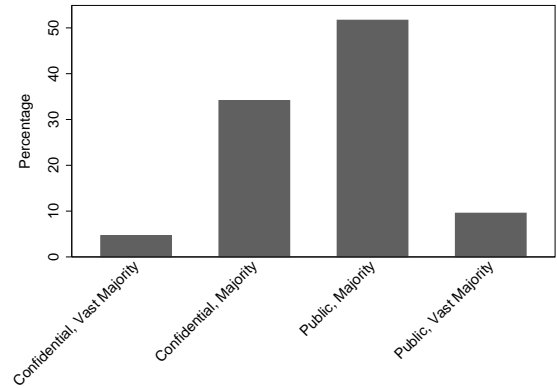
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Figure 1: Contributors' Perception of the Confidentiality of Contributions (Post-Election Mail-In Survey)

a. Perception of whether contribution records are confidential/public:

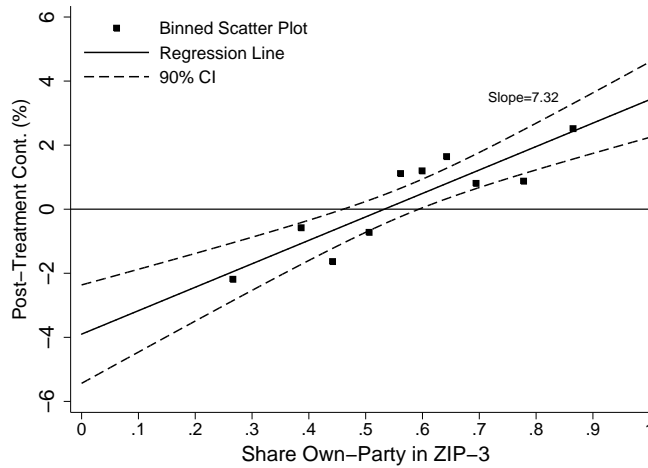


b. Perception of the proportion of neighbors who believe that contribution records are confidential/public:



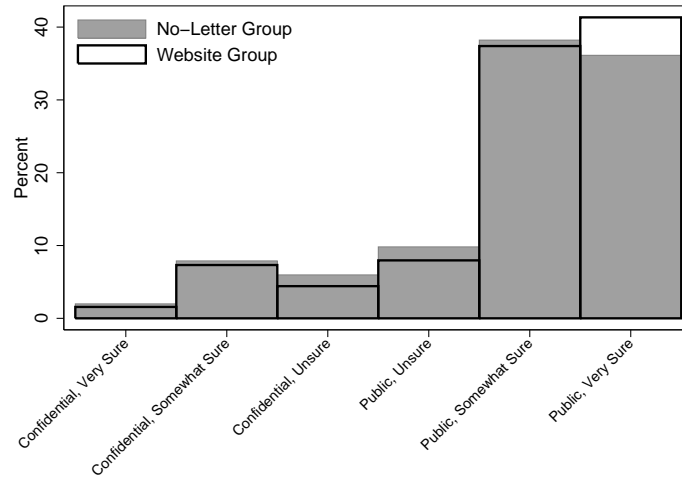
Notes: N=3,068 (a) and 3,018 (b). Responses to our post-election mail-in survey by subjects in the No-Letter group. Panel (a) combines answers to questions 4 and 5 from the questionnaire. Panel (b) is based on question 7 from the questionnaire. For a copy of the questionnaire, see Appendix D.

Figure 2: Effect of Higher Visibility on the Probability that the Recipient Makes a Post-Treatment Contribution, by Partisan Composition of the Recipient's Area



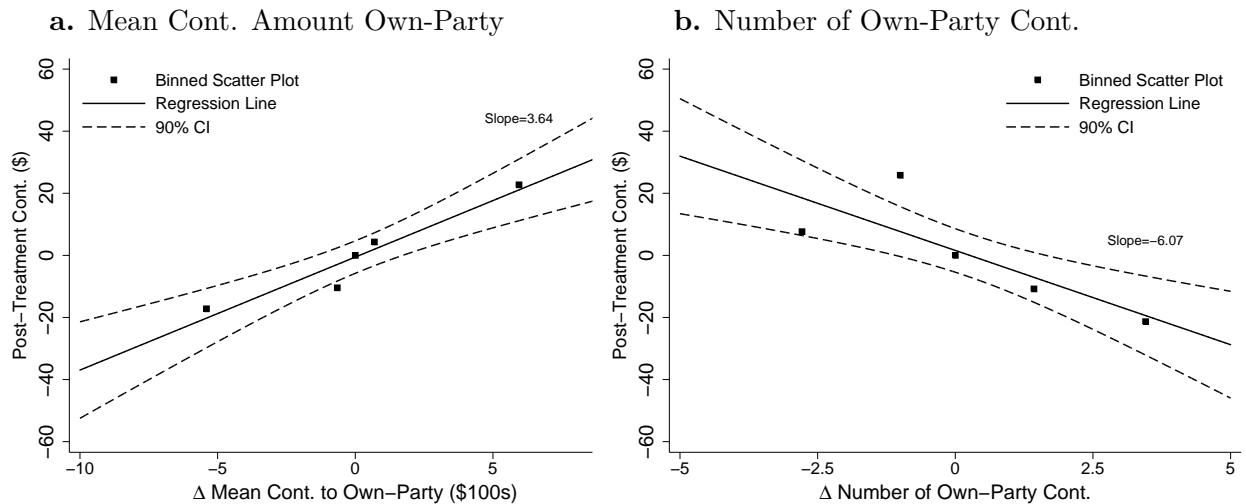
Notes: $N = 32,070$. Observations from subjects assigned to the Website letter. The dots in the Figure correspond to a binned scatterplot representation of the partial regression plot. Each dot corresponds to one decile of the distribution of *Share Own-Party*, with its position in the horizontal axis corresponding to the mean value of *Share Own-Party* in that decile. For each dot, the position in the vertical axis corresponds to the average effect of the *Higher Visibility* intervention on the probability of making post-treatment contributions within the corresponding decile of *Share Own-Party*. The regression line corresponds to the linear relationship between the two variables, with confidence intervals based on heteroskedasticity-robust standard errors. The binned scatterplot was estimated from a regression of an indicator of whether the individual made a post-treatment contribution on a dummy for whether the subject was assigned the Website-Neighbors sub-treatment, dummies for the deciles of *Share Own-Party*, the interaction between the two latter sets of dummies, and a set of individual-level controls (for more details about the regression, see the notes to Table 1).

Figure 3: Effect of the Website Letter on the Belief that Contribution Records are Confidential/Public (Post-Election Mail-In Survey)



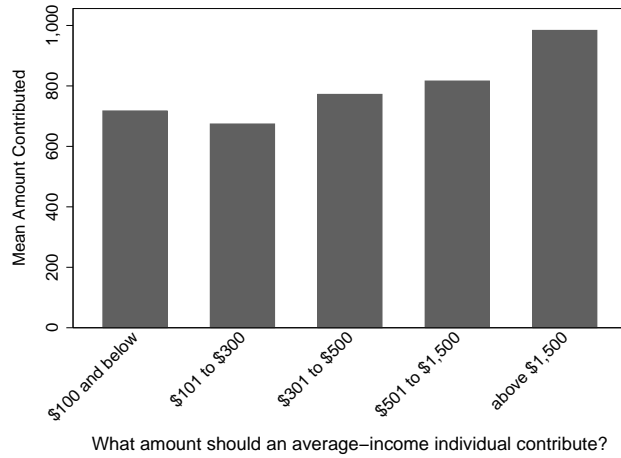
Notes: $N = 9,414$. Histograms of responses to the post-election mail-in survey. No-Letter corresponds to respondents who did not receive any letter during the experimental stage, while Website group corresponds to respondents who received a Website-Self or a Website-Neighbors letter. This measure of perception of the public nature of contribution records combines the answer to a first question about disclosure policy and the answer to a second question about the respondent's confidence in that first answer (questions 4 and 5 from the questionnaire in Appendix A.7, respectively).

Figure 4: Effect of Information from List-Letter on Post-Treatment Contribution Amount



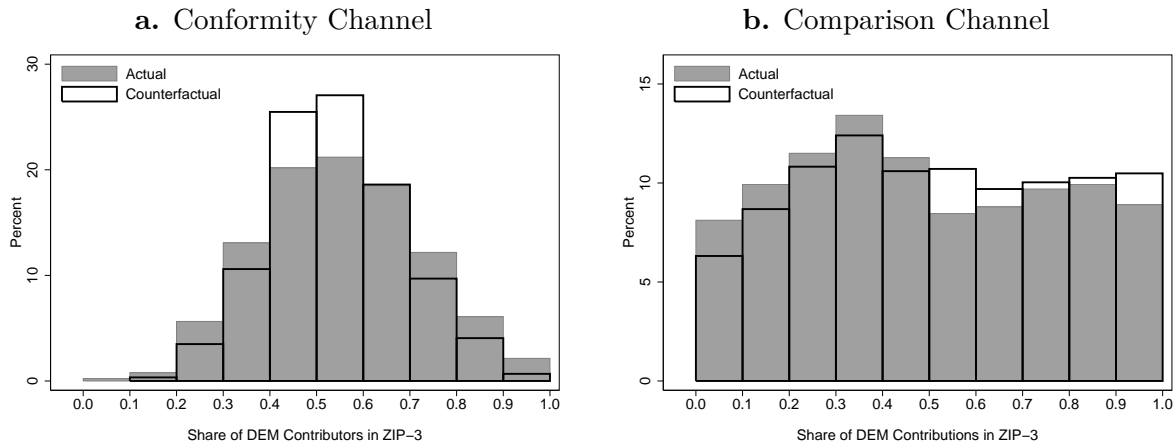
Notes: $N = 36,795$. Observations from subjects assigned to the List letter. The dependent variable in both panels is the amount contributed during the post-treatment period. The dots in the two figures correspond to binned scatterplot representations of the partial regression plots. Each dot corresponds to one quintile of the distribution of horizontal axis variable, with its position in the horizontal axis corresponding to the mean value of the variable in that quintile. For each dot, the position in the vertical axis corresponds to the average effect of the intervention on the post-treatment contribution amount within the corresponding quintile of the horizontal axis variable. The regression line corresponds to the linear relationship between the two variables, with confidence intervals based on heteroskedasticity-robust standard errors. The binned scatterplot was estimated from an Interval Regression of the post-treatment amounts contributed on a set of dummies corresponding to the quintiles of the variables on the x-axis – more specifically, corresponding to the difference between the value of this variable computed with the list sent to the recipient and the corresponding value computed in the baseline list. The regressions also include the usual set of individual-level controls (for more details about the regression, see the notes to Table 1). Additionally, the regression from panel (a) controls for \bar{c}_{opp} and N_{own} , while the regression from panel (b) controls for \bar{c}_{own} and \bar{c}_{opp} .

Figure 5: Relationship Between Self-Reported Contribution Norm and Actual Amount Contributed (Post-Election Mail-In Survey)



Notes: $N = 3,018$. The figure is based on a combination of responses to the post-election mail-in survey from subjects in the No-Letter group and data on those respondents' contributions during the entire 2012 presidential campaign cycle (from FEC records). The horizontal axis represents the quintiles of the distribution of responses to the survey question about how much individuals "should" contribute to a presidential campaign (question 8 from the questionnaire in Appendix A.7). The vertical axis represents the average amount contributed by respondents during the presidential election cycle.

Figure 6: Counterfactual Analysis of the Comparison and Conformity Effects on Geographic Polarization



Notes: $N = 31,996$ (panel a) and $N = 32,070$ (panel b). In panel (a), the solid bars correspond to the actual distribution of the share of Democratic contributors across ZIP-3 areas during the 2012 presidential campaign cycle, and the hollow bars correspond to the counterfactual distribution in a scenario with no conformity effects. In panel (b), the solid bars correspond to the actual distribution of the share of Democratic contributions across ZIP-3 areas during the 2012 presidential campaign cycle, and the hollow bars correspond to the counterfactual distribution in a scenario with no comparison effects. The details of the computation of both counterfactual distributions are presented in Section 5.

Table 1: Experimental Evidence on the Conformity Channel

	Post-Treatment Contributions						Pre-Treatment	
	(1) \$	(2) \$	(3) \$	(4) \$	(5) \$	(6) \$	(7) P(\$>0)	(8) \$
Higher Visibility (Website-Neighbors - Website-Self)	-89.07** (37.22)	12.72 (26.74)	-6.99 (61.55)		-84.38*** (27.72)	-35.40 (31.03)	-3.53** (1.61)	-3.32 (21.77)
Interaction with:								
Share own-party in ZIP-3	143.50** (60.30)				149.04*** (44.81)	40.84 (50.03)	6.67** (2.71)	3.37 (34.16)
Share own-race in ZIP-3		-32.99 (45.76)						
Share low-income in ZIP-3			4.65 (129.63)					
Placebo (Placebo - No-Letter)				8.58 (29.00)				
Interaction with:								
Share own-party in ZIP-3				-8.99 (46.75)				
Regression Method	Interval	Interval	Interval	Interval	Interval	Interval	OLS	OLS
Sub-Period					≤ Sep-1	> Sep-1		
Mean Outcome	\$329	\$329	\$329	\$329	\$163	\$166	56%	\$522
Observations	32,070	32,070	32,070	103,367	32,070	32,070	32,070	32,070

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. Observations from subjects assigned to Website letters. The dependent variable in columns (1) through (6) is the amount contributed during the post-treatment period. The dependent variable in column (7) takes the value of 100 if the individual made at least one post-treatment contribution and 0 otherwise. The dependent variable in column (8) is the dollar amount contributed in the pre-treatment period. *Higher Visibility* is a dummy on whether the subject received a Website-Neighbors rather than a Website-Self letter. *Share Own-Party* stands for the share of own-party contributors to presidential campaigns in the ZIP-3 during the three previous presidential election cycles. *Share Own-Race* refers to the share of individuals of the same race as the recipient in the same ZIP-3, with two groups for race (white and non-white). *Share Low-Income* refers to the share of income-earning adults with an annual income below \$30,000 (U.S. Census, 2010). All the regressions except the one in column (8) include as controls the levels of all variables that are interacted with *Higher Visibility*, the time it took for delivery of the mailpiece, and a set of variables with pre-treatment contributions to each candidate. The outcome variables in columns (4) and (5) correspond to two disjointed moments during the post-treatment period: before and after September 1, 2012. *Mean Outcome* corresponds to the average of the outcome variable.

Table 2: Identification of Comparison Channel: Sample Treatment Lists Generated with Different Parameter Values

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

Notes: This is an example of how the algorithm generates different lists of nine neighbors from a given sample of the recipient's thirty closest contributing neighbors. See Section 4.2 for a detailed description of the algorithm.

Table 3: Evidence on the Comparison Channel

	Post-Treatment Contributions					Pre-Treatment
	(1)	(2)	(3)	(4)	(5)	(6)
	\$	\$	\$	\$	P(\$>0)	\$
\bar{c}_{own}	2.95** (1.47)	3.20** (1.47)	1.75 (1.22)	2.48** (1.17)	0.05 (0.06)	0.90 (0.91)
\bar{c}_{opp}	0.06 (0.92)	-0.40 (0.96)	-0.01 (0.74)	-0.41 (0.78)	-0.05 (0.04)	0.06 (0.58)
N_{own}		-5.44* (2.86)	-6.19*** (2.24)	-0.47 (2.30)	-0.16 (0.13)	1.59 (1.75)
Regression Method	Interval	Interval	Interval	Interval	OLS	OLS
Sub-Period			\leq Sep-1	$>$ Sep-1		
Mean Outcome	\$338	\$338	\$168	\$171	56%	\$527

Notes: $N = 31,996$. * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. Observations from subjects assigned to the List letter. The dependent variable in columns (1) and (2) is the amount contributed during the post-treatment period. The dependent variable in column (3) takes the value of 100 if the individual made at least one post-treatment contribution and 0 otherwise. The dependent variable in column (4) is the dollar amount contributed during the pre-treatment period. All the independent variables except N_{own} are expressed in hundreds of dollars (i.e., the estimates correspond to effects of \$100 changes in the independent variables). \bar{c}_{own} (conversely, \bar{c}_{opp}) corresponds to the average contribution of all the individuals in the letter's table who contributed to the recipient's own (conversely, other). N_{own} is the number of individuals in the table who contributed to the recipient's party. 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. The outcome variables in columns (3) and (4) correspond to two disjoint sets of the post-treatment period: before and after September 1, 2012. See Table B.4 for descriptive statistics for all these independent variables. All the regressions except for the one in column (6) include the usual control variables: the time it took for delivery of the mailpiece and a set of variables with pre-treatment contributions to each candidate. *Mean Outcome* corresponds to the average of the outcome variable.

Online Appendix: Not For Publication

A Additional Material: Treatment Letters, Mail-In Survey, Project Website and the FEC Online Search Tool

Further Details on the Treatment Letters

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 A.8, 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 send an email to a dedicated 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 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. The implications for our estimates are discussed in Section 4.1.3 in the body of the paper.

A.1 Sample Letter: Website-Self

Boston, April 25th 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
DOE, JOHN	\$375 – DEM	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

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

A.2 Sample Letter: Website-Neighbors

Boston, April 25th 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
DOE, JANE	\$375 – DEM	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

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

A.3 Sample Letter: List-Once

Boston, April 25th 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

A.4 Sample Letter: List-Update

Boston, April 25th 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		<i>Future contributions to be reported:</i> 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

A.5 Sample Letter: Placebo

Boston, April 25th 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

A.6 Outside of the Mailing

Outside of the mailing - front

**DEAR JOHN:
IMPORTANT
INFORMATION ABOUT
POLITICAL CAMPAIGN
CONTRIBUTIONS**

Harvard University
108 Littauer Center
Cambridge, MA 02138

Nonprofit Org.
US Postage
PAID
Permit No. 492
04104



002*01*****3-DIGIT 040

TO: JOHN DOE
123 MAIN ST
SPRINGFIELD, VA 22150-1234

Outside of the mailing - back

**DEAR JOHN:
IMPORTANT INFORMATION
ABOUT POLITICAL
CAMPAIGN CONTRIBUTIONS**

A.7 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

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 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 to 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

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

2- Your age (Please mark ONE option)

Under 20 20-29 30-39 Over 40

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.

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:

A.8 Text Displayed on the Project’s 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)

A.9 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*):

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):
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

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

SCHEDULE A-P ITEMIZED RECEIPTS	Use separate schedule(s) for each category of the Detailed Summary Page	FOR LINE NUMBER: (check only one) <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	PAGE 9410 / 17901
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.			
A. Full Name (Last, First, Middle Initial) Doe, John Mailing Address 123 Fake Street City Eliot State ME Zip Code 03903		Transaction ID : 0992651 Date of Receipt <div style="border: 1px solid black; padding: 2px; display: flex; justify-content: space-around;"> M M / D D / Y Y Y Y Y Y </div> <div style="border: 1px solid black; padding: 2px; display: flex; justify-content: space-around;"> 12 / 16 / 2011 </div>	
FEC ID number of contributing federal political committee. C		Amount of Each Receipt this Period <div style="border: 1px solid black; padding: 2px; display: flex; justify-content: flex-end;"> 250.00 </div>	
Name of Employer Homemaker Occupation Homemaker Receipt For: 2012 <input checked="" type="checkbox"/> Primary <input type="checkbox"/> General <input type="checkbox"/> Other (specify) ▼		Election Cycle-to-Date ▼ <div style="border: 1px solid black; padding: 2px; display: flex; justify-content: flex-end;"> 500.00 </div>	

B Further Details on the Subject Pool and on the Implementation of the Field Experiment

B.1 Subject Pool and Timing of the Mailing

As detailed in Section 3, a total of 280,456 unique individuals were listed in the FEC records as having made a contribution to a presidential candidate between April 1, 2011 and April 1, 2012. 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 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.¹

We discarded a substantial fraction of the original 280,456 contributors for data quality and other reasons, resulting in a final subject pool of 191,832 individuals. We present here a list of the most important reasons and criteria. We do not report what percentage of individuals were excluded for each reason because a majority of the excluded individuals were excluded for multiple reasons. We 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, and we excluded all individuals who changed residence since the date when they made their first contribution during the election cycle. We excluded individuals who reported contributions in multiple addresses. We excluded individuals whose mean distance (as the crow flies) from the ten closest contributors was over three miles. We also excluded individuals who had already made a total contribution over \$1,500, all contributors living outside the 50 U.S. States, and all contributors in Washington D.C.. Finally, we also discarded individuals who had simultaneously made contributions over \$200 to the Obama campaign and to at least one of the Republican presidential candidates – only 0.08% of the original sample belonged in this category.

We mailed the letters depicted in the previous paragraph on May 6, 2012. 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

¹McAdams, 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.

system, which does not confirm delivery but provides an estimate of when the letter was out for delivery (i.e., it 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 is a good approximation that provides a conservative lower bound for the actual date of delivery. Again, this proxy of delivery does not necessarily indicate that the letters were received or read, as the mailing did not include delivery confirmation service. We also constructed a proxy for the time when individuals may have read the letters. The letters included a link to a website with contact information for the research team and details about the research project. 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 B.1 compares our proxy of delivery date from the USPS tracking data with data on visits to the project’s website. Figure B.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 B.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 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. 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. All in all, the evidence is consistent with our proxy for delivery being a conservative lower bound estimate of the actual date of delivery.

B.2 Descriptive Statistics, Subjects’ Contribution Patterns and Balance Test for Randomization

Table B.1 presents summary statistics of individual characteristics from our experimental sample of early 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 to the 2012 presidential election to the extent that 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 is partly due to the fact that Republicans, who contributed higher amounts, are over-represented in the experimental sample. 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.

Table B.2, in turn, 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 not statistically significant.

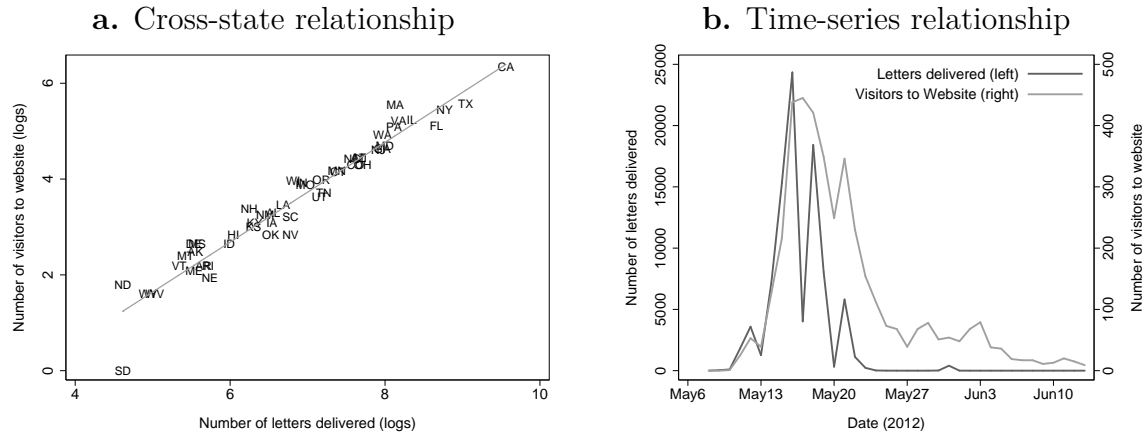
Finally, Table B.3 details the pre and post-treatment contribution patterns for the No Letter group. The top panel presents detailed statistics for the pre-treatment period, during which 52% contributed to Obama and the remaining to Republican candidates. On average, individuals contributed about \$524 during the pre-treatment period. Republican contributed substantially larger amounts than their Democratic counterparts, which was expected given that the Republican candidates were taking part in a primary while President Obama was only in the general election. The bottom half of Table B.3 presents similar statistics for post-treatment contributions. During the post-treatment period, 48.9% of our subjects made at least one contribution. For those who made contributions during the post-treatment period, the average amount contributed was \$587. There are significant differences in post-treatment contributions across parties: e.g., the probability of making a post-treatment contribution was 75.9% for supporters of Obama, but only 38.6% for Romney supporters and 11.9% for supporters of other Republican candidates. Because of these differences in baseline rates, it is not straightforward to compare the magnitude of the effects across party lines.

B.3 Descriptive Statistics about the Table of Contributors Shown in the List Letter

As described in the body of the paper and illustrated by a facsimile in the previous section, the List letter type included a list of 9 neighbors along with the amount contributed and party contributed

to. Section 2.2.2 describes the methodology used to create random non-deceptive variation in this list of itemized contribution records. Table B.4 presents summary statistics about the contribution records shown in that table, such as the average amount contributed by own- and opposite-party neighbors, as well as the number of neighbors contributing to the party of the recipient. We computed those statistics using the lists of contributors that would have been produced if we had set all of the weighting parameters to zero. That is, the variation in the contribution behavior corresponds to the baseline lists. As a result, Table B.4 illustrates the “natural” variation in contribution records and does not include the “induced” variation due to the randomization of the weighting parameters.

Figure B.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”).

Table B.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	U.S. Average
Percent Democrat	60.24 (48.94)	64.55 (47.84)	51.40
Mean amount contributed (\$)	850.91 (903.04)	559.39 (846.59)	
Percent male	56.22 (48.79)	55.15 (48.82)	49.14
Percent white	79.10 (21.51)	78.77 (22.20)	62.99
Percent black	12.21 (14.57)	12.01 (14.52)	12.07
Population density, ZIP-5	6468.53 (16141.28)	6360.17 (16136.04)	3907.85
Mean income (\$), ZIP-5	108782.02 (119017.11)	98097.34 (113653.43)	55241.02
Observations	167,433	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 to the criteria described in Section 3. 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 B.2: Balance of Observable Individual Characteristics across Treatment Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control	Placebo	Website Self	Website Neighbors	List Once	List Update	Diff. Test
Percent Democratic	60.13 (0.17)	60.35 (0.39)	61.05 (0.39)	60.30 (0.39)	60.37 (0.39)	59.70 (0.39)	0.22
Mean amount contributed (\$)	523.53 (2.15)	519.33 (4.84)	512.15 (4.85)	514.29 (5.04)	527.49 (5.11)	526.72 (5.15)	0.14
Percent male	56.10 (0.17)	56.48 (0.40)	55.55 (0.40)	56.62 (0.40)	56.23 (0.40)	56.85 (0.40)	0.21
Percent white	79.07 (0.08)	78.93 (0.18)	79.26 (0.18)	79.14 (0.18)	79.04 (0.18)	79.22 (0.18)	0.81
Percent black	12.16 (0.05)	12.43 (0.12)	12.17 (0.12)	12.23 (0.12)	12.37 (0.12)	12.13 (0.12)	0.16
Percent hispanic	3.96 (0.05)	3.96 (0.11)	3.87 (0.11)	3.94 (0.11)	3.66 (0.11)	3.88 (0.11)	0.25
Observations	87,283	16,084	15,993	16,077	15,979	16,017	

Notes: Averages for different pre-treatment individual-level characteristics for treatment groups. Standard deviations 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 B.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 B.3: Pre-Treatment and Post-Treatment Contribution Patterns, No-Letter Group

	All	Pre-treatment contribution to		
		Obama	Romney	Other
<i>Pre-Treatment Period</i>				
Amount Contributed (\$)	523.53 (634.92)	372.79 (367.40)	649.45 (673.24)	807.80 (931.22)
<i>Post-Treatment Period</i>				
Percent Contributed at Least Once	55.72 (49.67)	75.93 (42.75)	38.61 (48.69)	17.74 (38.20)
Amount Contributed (\$), if Positive	586.57 (681.29)	568.16 (644.95)	600.29 (716.40)	756.35 (927.19)
Observations	87,283	52,316	12,971	21,996

Notes: Average contribution behavior with standard deviations in parenthesis. The pre-treatment period spans from April 1, 2011 to the date the letter was received, and the post-treatment period spans from the date the letter was received to December 31, 2012. The other Republican candidates are: Bachman, Cain, Gingrich, Huntsman, Pawlenty, Perry and Santorum. Data from FEC public records.

Table B.4: Informational Effects: Summary Statistics

	Mean	Sd	Min	Max
\bar{c}_{own}	6.17	4.86	0.00	25.00
\bar{c}_{opp}	6.27	5.63	0.00	25.00
N_{own}	5.37	2.23	0.00	9.00
$\sum c_{own} - \sum c_{opp}$	9.25	50.19	-220.00	225.00
$ \sum c_{own} - \sum c_{opp} $	37.34	34.79	0.00	225.00

Notes: $N = 36,795$. Summary statistics corresponding to the sample of individuals assigned to the List treatment. The descriptive statistics for these variables correspond to the counterfactual baseline letter (i.e., when all the weighting parameters used to generate the list of 9 neighbors are set to zero). All these 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 the difference between total contributions to the own- and opposite-party. $|\sum c_{own} - \sum c_{opp}|$ is the absolute value of $\sum c_{own} - \sum c_{opp}$.

C Additional Results and Robustness Checks

C.1 General Robustness and Specification Checks

C.1.1 Robustness of Results to Alternative Regression Models and Standard Errors

This section presents a series of robustness and specification checks of the baseline results for the conformity and the comparison channels.

Table C.1 presents the main results for the conformity and comparison channels with the baseline specification (heteroskedasticity-robust standard errors with no clustering, column 1), and with clustering at different levels. The top panel reports the results for the conformity channel, and the bottom half reports the results for the comparison channel. Column (2) presents alternative results with clustering at the ZIP-5/party level (i.e., grouped by individuals from the same party and area of residence defined by the ZIP-5). In column (3), the standard errors are clustered at the ZIP-5 level, in column (4) at the ZIP-3 level, and in column (5) at the state level. The standard errors are very similar under all the alternatives, and the small differences do not imply changes in significance with respect to standard levels. The only noticeable difference is the clustering at the state level (column), which reduces standard errors and results in even more statistically significant coefficients.

Table C.2, in turn, presents the results for regression specification checks. The top panel reports the results for the conformity channel, and the bottom half reports the results for the comparison channel. Column (1) reports the baseline results (discussed in the body of the paper) which were computed by means of an interval regression model. Column (2) reports the results from the Tobit model. The point estimates and standard errors are virtually identical, for both the comparison and the conformity channels. In turn, we report in column (3) the estimates based on a Poisson regression model (the results – not reported – are virtually identical with a Negative Binomial model). In terms of statistical significance, the results are robust between columns (1) and (3). Quantitatively, we can compare the magnitude of these effects with the interval regression model from column (1). We start with the coefficient on Higher Visibility for the conformity channel. Those who made contributions during the post-treatment period gave on average \$587. The coefficient of -\$89.069 from column (1) corresponds to a -15.17% effect on the mean baseline contribution. Column (3) presents the simple coefficients from a Poisson regression. These coefficients can be interpreted as semi-elasticities: i.e., the coefficient of -0.177 implies that, in a place where everyone supports the opposite-party, the higher visibility intervention would decrease the expected amount contributed by 19.4% (i.e., $\exp(0.177) - 1$). This is close to (and statistically indistinguishable from) the 15.17% implied by column (1). The results are similar for the coefficient on the interaction between *Higher Visibility* and *Share Own-Party*.

The bottom panel in Table C.2 presents the corresponding regression specification checks for the comparison channel. The Poisson estimates imply that a \$100 increase in \bar{c}_{own} raises contributions by 0.6%. The interval regression estimates, in turn, imply that a \$100 increase in \bar{c}_{own} raises contributions by 0.55% (i.e., 3.2/587) of the mean baseline rate. These two effects are also very close and statistically indistinguishable. The pattern of comparison of results is similar for the coefficient on N_{own} . In summary, if anything, the effect sizes implied by the Poisson model are slightly larger than the effect sizes implied by the interval regression model.

The last two columns of Table C.2 report results for the falsification test using the pre-treatment contributions. Column (5) corresponds to the specification reported in the body of the paper, which uses an OLS model with the amount contributed pre-treatment as the dependent variable. There is no point in showing the results for the interval regression model or the Tobit model because they would result in exactly the same estimates, since by construction all subjects in the sample had contributed non-zero amounts in the pre-treatment period. We can also verify if the falsification test is robust if we compute proportional effects instead. Column (6) presents the results when we use the logarithm of pre-treatment contributions as the dependent variable. The results are qualitatively similar: all the coefficients are close to zero, precisely estimated and not statistically significant. The results are also quantitatively similar. For example, the coefficient on *Higher Visibility* for the conformity effects in the linear model implies an effect of -0.5% (i.e., -3.316/587), while the logarithmic model implies an effect of 1.1%. Both coefficients are not statistically significant, and also statistically indistinguishable from each other. Most importantly, both estimates are very small compared to the magnitude of the effects on the post-treatment contributions (in the order of 15%-19%).

C.1.2 Effects on Non-Presidential Contributions

We can also assess whether our treatments affected contributions to other federal campaigns besides the presidential campaign (e.g., Senatorial races). For the conformity channel, we would expect that a Democrat contributor in a highly Republican area should feel pressure to contribute less to all Democratic candidates, not only Obama. The top panel of column (4) in Table C.2 reports the results of a regression with the same specification as in column (1), but with the post-treatment amount contributed to non-presidential committees as the dependent variable (this information was also obtained from the FEC public records).ⁱⁱ This additional outcome has the disadvantage that it has less variation in our subject pool: e.g., only 14.4% of our subjects contributed to non-presidential campaigns during the post-treatment period, whereas 49% contributed to presidential

ⁱⁱFor the sake of simplicity, this measure excludes contributions to non-presidential committees or candidates from a party different from the one to which the presidential contributions were made (such cross-party variations in contributions are a very small minority: less than 1% of non-presidential contributions are made to another party). The results are robust to the inclusion of all non-presidential contributions in this variable.

campaigns.ⁱⁱⁱ The estimates from columns (1) and (4) (top panel) are very similar, suggesting that the effect of the higher visibility treatment on non-presidential contributions may well be similar to its effect on presidential contributions. However, due to the anticipated lack of precision, the estimates from column (4) are not statistically significant at conventional levels.^{iv}

The bottom panel of column (4) in Table C.2 reports the results of a regression with the same specification as in column (1) for the main specification of the comparison effects. The coefficients on \bar{c}_{own} , \bar{c}_{opp} and N_{own} have the same sign in the bottom panel of column (4) and in column (1), suggesting that the information about presidential contributions may have affected non-presidential contributions in the same direction than it affected presidential contributions. However, due to lack of variation in the dependent variable, the coefficients from column (4) are much less precisely estimated and as a result they are not statistically significant at conventional levels.

C.1.3 Robustness to the Inclusion of Contributors to the Rand Paul Campaign

As discussed in the main body of the paper, subjects contributing to Rand Paul in the pre-treatment period made virtually no contributions to the Republican nominee that emerged from the primaries (i.e., Romney) in the post-treatment period. In the No-Letter group, only 1.56 percent of Rand Paul supporters contributed to Romney. This average contribution rate is an order of magnitude lower than the rest of the subjects (56 percent). The linear regressions used for the analysis of our main results do not allow the magnitude of the effects to vary with the baseline contribution rate, and this can be problematic for dealing with these very large differences in baseline contribution rates. Additionally, this pattern suggests that Rand Paul supporters may not identify themselves with the Republican party, which is a basic assumption for our conformity and comparison channel hypotheses. For all these reasons, we removed Rand Paul supporters from the database used for our main analysis and present our main results excluding this group.

Table C.3 presents the results of our main baseline results including and excluding these observations. The main results for both the conformity and the comparison channels remain unchanged under these alternatives samples: all the coefficients have the same sign and the same level of statistical significance with and without Rand Paul supporters (columns (1) and (2), and (3) and (4)). Of course, the post-treatment contribution behavior of Rand Paul supporters implies an attenuation in the effects, but while some of the coefficients are lower in absolute value when we include this group in the regressions, the differences are relatively small and the coefficients are

ⁱⁱⁱDue to FEC reporting requirements, the proportions here, as in the rest of the paper, represent only individuals contributing more than the \$200 threshold.

^{iv}Another minor concern regarding post-treatment contributions is that a small number of subjects made presidential campaign contributions to both parties. In our results, we imputed them as contributors to the party to which they donated in the pre-treatment period. While we could present robustness checks regarding this assumption, these cases represent only 0.04% of our subjects: their number is so small that the results remain virtually identical if we drop these observations or if we make alternative assumptions.

statistically indistinguishable between the two sets of regressions. Finally, as expected from the random assignment process, the balance check for pre-treatment contributions is also qualitatively not affected by the inclusion or exclusion of Rand Paul supporters (columns (5) and (6)).

C.2 Conformity Channel: Heterogeneity with Respect to Other ZIP-3 Characteristics

In the robustness checks of the results for the conformity channel in Section 4.1, we tested whether the effect of higher visibility varied with the share of individuals of the recipient’s own race, and with the share of low income households in the same ZIP-3 (Table 1). Here we present additional robustness checks to gauge whether the effects of higher visibility vary as a function of other important characteristics of the recipient’s ZIP-3 population composition: race, education, age and marital status, as well as the area’s population density and the average tax paid according to the 1040 IRS forms.

Results are reported in Table C.4, where we interact *Higher Visibility* with these other characteristics of the recipient’s area of residence. The first column repeats the baseline specification with *Share Own-Party* as in column (1) in Table 1. In column (2) we present the coefficient of the interaction of *Higher Visibility* with the share of African Americans in the recipient’s area of residence. This coefficient is very small (1.644) and not statistically significant at standard levels (SE of 77.164). We obtain the same qualitative result in columns (3) through (6) from Table C.4, in which we interact *Higher Visibility* with the share of college graduates, the share of individuals under 25 years old, the share of married individuals, the area’s population density and the average tax rate. In none of these specifications the coefficient of *Higher Visibility* nor that of its interaction with these area’s characteristics are statistically significant at standard levels, and they are also economically small (close to zero).

Since all these variables have different distributions, the best way to compare the magnitude of the coefficients is by studying the effect of a one standard deviation increase in each ZIP-3 characteristic. The coefficients imply that a one standard deviation would increase the effect of *Higher Visibility* by \$24.39** (SE 10.62) for *Share Own-Party*, \$0.19 (SE 9.49) for *Share Black*, \$3.02 (SE 10.10) for *Share College*, \$1.09 (SE 11.23) for *Share <25yo*, \$0.25 (SE 9.99) for *Share Married*, -\$5.79 (SE 9.50) for *Population Density* and -\$3.95 (SE 12.37) for *Mean Tax Rate*. The pairwise differences of these coefficients with that of the interaction of *Higher Visibility* and *Share Own-Party* are either statistically significant or marginally non-significant. In other words, there is no evidence of heterogeneity in the impact of the higher visibility treatment with respect to other ZIP-3 characteristics. Finally, our main results remain virtually unchanged if we estimate the specification in column (1) (i.e., with the interaction of *Higher Visibility* and *Share Own-Party*)

and include additional controls for the area characteristics and their interactions with *Higher Visibility* (results not reported).

C.3 Comparison Channel: Additional Results

C.3.1 Results with Alternative Statistics

In Section 4.2 we reported that a higher number of own-party contributors (N_{own}) discourages contributions. In this section, we start by exploring whether the effect from N_{own} may be due to contributors updating their beliefs about the probability of making a pivotal contribution.

Some individuals may contribute because of the perception that, with some probability, their marginal contribution will change the election outcome (Ansolabehere et al., 2003). This is similar to the probability of being a pivotal voter in a model of voter turnout (e.g., Dhillon and Peralya, 2002).^v One common argument against this theory, similar to the argument about a marginal vote, is that 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, so that the probability of making a pivotal contribution is extremely small. However, individuals may still systematically over-estimate this probability. One prediction from this line of reasoning is that individuals should be more likely to contribute when an election is close. Since the distribution of contributions between the two parties may be a signal of how close an election will be, a prediction from this theory is that contributors should care about the contributions of others. For example, if we assume that a close campaign signals a close election, then a contributor should be more eager to make a contribution when she observes that the “contribution race” is more even. Note that, however, the List letter included contribution records from the individual’s area of residence, so that this could affect the perception of making a pivotal contribution only to the extent that individuals extrapolate how the campaign is going from the local to the national level.

The regression results are presented in Table C.5. Column (1) reproduces the results from the baseline specification in Section 4.2. Column (2) replaces N_{own} by the difference in total contributions between both parties, $\sum c_{own} - \sum c_{opp}$.^{vi} As in column (1), the significant negative coefficient suggests that recipients make less generous contributions when they are shown that their own party is doing better than the opposite party. To disentangle the pivotal contribution motive, the specification in column (3) includes as an additional independent variable the absolute value of the difference between the total amounts contributed to the recipient’s own and opposite-party, $|\sum c_{own} - \sum c_{opp}|$, which is a measure of how uneven the campaign is. The coefficient on

^vDhillon, A. and Peralta, S. (2002), “Economic Theories of Voter Turnout,” *The Economic Journal*, Vol. 112 (480), pp. F332-F352.

^{vi}Note that, in this specification, increasing \bar{c}_{own} while holding N_{own} constant has an effect through both \bar{c}_{own} and $\sum c_{own}$.

this variable has the expected sign: individuals are less motivated to make a contribution in more uneven races. However, this coefficient is smaller than the coefficient on $\sum c_{own} - \sum c_{opp}$, and it is not statistically significant.^{vii} Furthermore, the coefficient on $\sum c_{own} - \sum c_{opp}$ remains similar to the corresponding coefficient from column (2). Finally, for completeness, column (4) in Table C.5 presents the falsification test with pre-treatment contributions as the dependent variable. As in all other results for this type of falsification test, none of the included variables have a significant effect on pre-treatment contributions.

C.3.2 Robustness Check: Controlling for Area Fixed Effects

One concern is whether the estimated comparison effects are due solely to the experimental assignment, or whether they may be contaminated by cross-sectional differences in the neighbors of the recipients.

In the body of the paper, we discuss a key falsification test: we estimate the same regression with pre-treatment contributions instead of the post-treatment contributions as the dependent variable (Table 3, column 6). In this subsection, we present an additional robustness check, consisting of estimating our baseline regression with ZIP-5 fixed effects. Intuitively, two individuals in the same ZIP-5 are more likely to have a very similar list of 30 neighboring contributors that could potentially end up in the letter we mailed. Our experimental assignment procedure provides these two individuals from the same ZIP-5 with different subsets of contributors from the same area. If the identification is due to the experimental assignment, then controlling for these fixed effect should not affect the results in a meaningful or substantial way. If, however, the identification was contaminated by non-experimental differences across space, then the effects should disappear or change substantially with the inclusion of the area fixed effects.

The results from this exercise are presented in Table C.6. One technical challenge is that, to the best of our knowledge, there is no fixed-effects estimator for an Interval Regression model. Instead, we use the Tobit model as a baseline, which provides very similar results to the Interval Regression model (as shown in Appendix C.1) and for which there is a fixed-effects estimator (Honoré, 1992).^{viii} Column (1) presents the baseline results from the Tobit model without ZIP-5 fixed effects, while column (2) presents results including the fixed effects. We cannot reject the null hypothesis that the coefficients in column (1) and (2) are equal. If anything, the coefficients are larger in absolute value with the addition of the fixed effects, contrary to what we would expect from non-experimental “contamination” of our results. However, we should not over-interpret

^{vii}It is also possible that the investment motive would have been more relevant if, instead of generating variation in how close the campaign was, we had created variation directly on how close the election was expected to be (such as using information from prediction markets).

^{viii}Honoré, B.E. (1992), “Trimmed Lad and Least Squares Estimation of Truncated and Censored Regression Models with Fixed Effects,” *Econometrica*, Vol. 60 (3), pp. 533–565.

this difference, because the semi-parametric estimator from Honoré (1992) results in less precisely estimated coefficients.

We can provide an additional robustness check using the Poisson model, for which there is also a fixed-effects estimator. Column (3) presents the baseline Poisson results without ZIP-5 fixed effects. As discussed in Appendix C.1, the sign, statistical significance and magnitude of the effects are similar between the Poisson and Tobit estimates. Column (4) presents results of a similar model including the ZIP-5 fixed effects. The coefficients are almost identical in columns (3) and (4), suggesting again that controlling for fixed-effects does not make a tangible difference in our results. In sum, the evidence from this robustness check confirms the hypothesis that the identification is coming from the experimental variation.

C.3.3 Results on the Effects of Disseminating Unbiased Information about Contribution Records

In this section, we study whether the dissemination of objective information about the contribution behavior of others may affect contributions, possibly due to systematic biases in the perceptions about the contribution behavior of others.

We measure the effect of providing contributors with unbiased information about contribution records of other individuals in their area (i.e., the table of contributions contained in the List letter). If individuals had a systematically biased perception of the contribution behavior of others, then the unbiased information should have a significant effect on their contributions. For instance, suppose that individuals, on average, under-estimate the average amount contributed by other supporters of their same party.^{ix} Since the individual's contribution is increasing in this belief (see Section 4.2), then the distribution of unbiased information should have a positive effect on the contributions of the recipients.

However, the comparison of contributions between the List and No-Letter subjects may be contaminated by other pieces of information contained in the List letter that are unrelated to the information about contribution behavior of others. 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. To test this hypothesis, we use the Placebo letter, which has the same format than the List letter but, instead of providing a table about the contributions of neighbors,

^{ix}There are several reasons why individuals may fail to form an accurate perception of the distribution of contributions. Tversky, A and Kahneman, D. (1973), "Availability: A heuristic for judging frequency and probability," *Cognitive Psychology*, Vol. 5 (1), pp. 207-233.

it displayed standard regulatory information about contribution limits. We did not expect this information to have an effect on contributions, because contribution limits were not binding for virtually all of the individuals in the No-Letter group. The Placebo letter could still have some effects if, for example, it works as a reminder that the individual must make a contribution, because of the feeling that the recipient is part of an academic study, etc. By comparing the effects of the List and Placebo letters, we can provide some suggestive evidence about the effect of the List letter that can be attributed exclusively to the table with information about contributions of others, rather than to these other confounding factors.

There is, however, a second limitation with this comparison. We explicitly took measures to ensure that recipients of the List letter would not feel more observed by their neighbors: e.g., semi-anonymizing the records, not including information about the search tool of the FEC website, choosing neighbors from a broader geographical area. Despite our efforts, however, it is possible that part of the effect of the List letter, compared to the Placebo letter, came from the recipient feeling more observed by neighbors. In principle, this should not be a problem because, as seen in the results about conformity effects, the average effect of increased visibility is practically zero. However, since we did not know this fact before running the experiment, we randomized another aspect of the List-letter to test whether it significantly increased the perceived visibility of the recipient's contribution. 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 could 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 individuals felt that their names could be recognized by neighbors they knew, they should feel more monitored in terms of their future contributions from the List-Update letter, because the neighbors would get an update about the subsequent contributions.

Table C.7 presents a series of comparisons between the post-treatment amounts contributed in these different treatment groups. Column (1) shows that, compared to the No-Letter group, sending an individual a List letter increases the amount contributed post-treatment by about \$19.45. This effect is statistically significant at the 1% level, and also economically significant. Recall that, among those who contributed at least once during the post-treatment period, the mean amount contributed was about \$587. This means that the effect of the List letter accounted for 3.3% of this baseline contribution. Column (4) shows that the effect of the List letter on the probability of making a post-treatment contribution was about 0.478 percentage points (p-value<0.10). Once again, the effect on the extensive margin was somewhat lower than on the amount contributed: this effect of the List letter amounted to just about 0.85% of the baseline rate of 55.72% (i.e. the mean probability of post-treatment contribution in the No-Letter group). As a

falsification test, column (5) estimates the “effect” of the List letter on pre-treatment contributions. As expected, the coefficient is very close to zero and not statistically significant.

Columns (2) and (3) explore whether the effects of the List letter were indeed due to the information about the contribution patterns of others. Instead of comparing the List and No-Letter groups, column (2) offers a comparison between the List and Placebo groups. The coefficient is similar in magnitude (\$15.82) to the corresponding coefficient from column (1) (\$19.45), and it is also statistically significant. Furthermore, we cannot reject the null hypothesis that these two coefficients are equal. Another way of looking at this evidence is that, compared to the No-Letter group, the Placebo did not affect the contribution behavior of recipients. Last, column (3) compares post-treatment contributions between the List-Update and List-Once groups. The difference between these two is very close to zero and not statistically significant. This result suggests that the effect of the List letter cannot be attributed to increased visibility of the recipient’s contribution.

In sum, the evidence from this appendix suggests that the information about contribution records from the List letter may have corrected some systematic biases in the recipients’ perception about the contribution behavior of others. More precisely, the positive effect on contributions may imply that individuals systematically under-estimate the mean amount contributed by own-party neighbors, that they over-estimate the number of own-party contributors relative to the opposite-party contributors, or a combination of both.^x

^xIn unreported results we also find that the effect of the List letter was more positive in areas with higher average contributions by own-party neighbors and in areas where the total contributions to the own-party are surpassed by the total contributions to the opposite-party.

Table C.1: Robustness to Different Alternatives of Standard Error Clustering

	Dep Var.: Amount Contributed Post-Treatment				
	(1)	(2)	(3)	(4)	(5)
<i>Conformity Channel:</i>					
Higher Visibility (Website-Neighbors - Website-Self)	-89.069** (37.222)	-89.069** (38.295)	-89.069** (38.147)	-89.069** (40.077)	-89.069*** (30.262)
Interaction with:					
Share Own-Party in ZIP-3	143.499** (60.303)	143.499** (62.493)	143.499** (62.099)	143.499** (65.406)	143.499*** (50.163)
<i>Comparison Channel:</i>					
\bar{c}_{own}	3.200** (1.473)	3.200** (1.459)	3.200** (1.446)	3.200** (1.444)	3.200** (1.387)
\bar{c}_{opp}	-0.399 (0.956)	-0.399 (0.955)	-0.399 (0.964)	-0.399 (1.003)	-0.399 (1.171)
N_{own}	-5.440* (2.862)	-5.440* (2.899)	-5.440* (2.886)	-5.440* (2.830)	-5.440** (2.706)
SE Clustered By:	None (Robust)	ZIP-5/Party	ZIP-5	ZIP-3	State

Notes: $N = 32,070$ (top panel, conformity channel) and $N = 31,996$ (bottom panel, comparison channel). * significant at the 10% level, ** at the 5% level, *** at the 1% level. Column (1) presents results with heteroskedasticity-robust standard errors in parenthesis. Column (2) presents alternative results with clustering at the ZIP-5/party level (i.e., grouped by individuals from the same party and area of residence defined by the ZIP-5). In column (3), the standard errors are grouped by ZIP-5 areas, in column (4) by ZIP-3, and in column (5), by state. Control variables and details of regression specifications as in Tables 1 and 3 in the body of the paper.

Table C.2: Robustness Checks: Alternative Regression Models and Specifications

	Post-Treatment Contributions				Pre-Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
	\$	\$	\$	\$	\$	log(\$)
<i>Conformity Channel:</i>						
Higher Visibility (Website-Neighbors - Website-Self)	-89.069** (37.222)	-89.025** (37.225)	-0.177** (0.074)	-52.887 (263.288)	-3.316 (21.766)	0.011 (0.036)
Interaction with:						
Share Own-Party in ZIP-3	143.499** (60.303)	143.453** (60.308)	0.279** (0.117)	93.527 (430.402)	3.365 (34.163)	-0.019 (0.060)
<i>Comparison Channel:</i>						
\bar{c}_{own}	3.200** (1.473)	3.197** (1.473)	0.006** (0.003)	9.499 (11.916)	0.896 (0.910)	0.000 (0.001)
\bar{c}_{opp}	-0.399 (0.956)	-0.401 (0.956)	-0.000 (0.002)	-5.361 (6.957)	0.061 (0.580)	0.000 (0.001)
N_{own}	-5.440* (2.862)	-5.447* (2.862)	-0.011** (0.005)	-19.645 (19.852)	1.586 (1.750)	0.003 (0.003)
Regression Model	Interval	Tobit	Poisson	Interval	OLS	OLS
Contribution Type	Pres.	Pres.	Pres.	Non-Pres.	Pres.	Pres.

Notes: $N = 32,070$ (top panel, conformity channel) and $N = 31,996$ (bottom panel, comparison channel). * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. Control variables and details of regression specifications as in Tables 1 and 3 in the body of the paper. The contribution type *Pres.* corresponds to presidential committees (i.e., Obama and Romney), while *Non-Pres.* corresponds to non-presidential committees.

Table C.3: Robustness to the Inclusion of Contributors to the Rand Paul Campaign

	Post-Treatment Contributions				Pre-Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
	\$	\$	P(\$)>0	P(\$)>0	\$	\$
<i>Conformity Channel:</i>						
Higher Visibility (Website-Neighbors - Website-Self)	-89.07** (37.22)	-77.17** (36.72)	-3.53** (1.61)	-2.69** (1.33)	-3.32 (21.77)	-11.02 (19.90)
Interaction with:						
Share Own-Party in ZIP-3	143.50** (60.30)	128.82** (59.69)	6.67** (2.71)	5.40** (2.34)	3.37 (34.16)	16.00 (31.97)
Observations	32,070	36,773	32,070	36,773	32,070	36,773
<i>Comparison Channel:</i>						
\bar{c}_{own}	3.20** (1.47)	2.88** (1.46)	0.05 (0.06)	0.04 (0.05)	0.90 (0.91)	0.74 (0.82)
\bar{c}_{opp}	-0.40 (0.96)	-0.61 (0.95)	-0.05 (0.04)	-0.05 (0.04)	0.06 (0.58)	0.11 (0.57)
N_{own}	-5.44* (2.86)	-6.15** (2.85)	-0.16 (0.13)	-0.17 (0.11)	1.59 (1.75)	2.04 (1.64)
Observations	31,996	36,795	31,996	36,795	31,996	36,795
Incl. Rand Paul Regression Model	No Interval	Yes Interval	No OLS	Yes OLS	No OLS	Yes OLS

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. Control variables and details of regression specifications as in Tables 1 and 3 in the body of the paper.

Table C.4: Heterogeneity of Higher Visibility with Other ZIP-3 Characteristics

	Dep Var.: Amount Contributed Post-Treatment						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Higher Visibility (Website-Neighbors - Website-Self)	-89.069** (37.222)	-5.150 (14.272)	-22.344 (57.972)	-15.224 (106.017)	-6.428 (53.681)	-2.111 (11.257)	2.770 (23.734)
Interaction with:							
ZIP-3 Characteristic	143.499** (60.303)	1.644 (77.164)	30.240 (98.965)	21.717 (222.212)	2.864 (111.666)	-41.328 (71.426)	-30.351 (88.715)
ZIP-3 Characteristic	Share Own-Party	Share Black	Share College	Share <25yo	Share Married	Pop. Density	Mean Tax Rate
Mean ZIP-3 Characteristic	0.57	0.13	0.58	0.48	0.48	0.07	0.26
Std. ZIP-3 Characteristic	0.17	0.12	0.10	0.05	0.09	0.14	0.13
Observations	32,070	32,070	32,070	32,070	32,070	32,070	32,070

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. Observations from subjects assigned to Website letters. The dependent variable is the amount contributed during the post-treatment period. *Higher Visibility* is a dummy on whether the subject received a Website-Neighbors rather than a Website-Self letter. *Share Own-Party* stands for the share of own-party contributors to presidential campaigns in the ZIP-3 during the three previous presidential election cycles. The source for the following ZIP-3 characteristics is the American Community Survey 2012: *Share Black* refers to the share of African Americans in the recipient's ZIP-3, *Share College* is the share of college graduates in the same area, *Share <25yo* is the share of the population below 25 years of age in the area, *Share Married* is the share of married individuals in the area, and *Pop. Density* represents the population density. The *Mean Tax Rate* is the average tax rate paid in 1040 IRS forms (source: IRS Statistics of Income 2012). All the regressions include as controls the levels of all variables that are interacted with *Higher Visibility*, the time it took for delivery of the mailpiece, and a set of variables with pre-treatment contributions to each candidate. The contribution type *Pres.* corresponds to presidential committees (i.e., Obama and Romney), while *Non-Pres.* corresponds to non-presidential committees. *Mean Outcome* corresponds to the average of the outcome variable over the entire sample. Data on contributions from the FEC public records (see Table B.3 for descriptive statistics)..

Table C.5: Comparison Channel: Additional Specifications and Robustness Checks

	Post-Treatment Contributions			Pre-Treatment
	(1)	(2)	(3)	(4)
\bar{c}_{own}	3.20** (1.47)	4.44*** (1.58)	4.96*** (1.69)	1.04 (1.00)
\bar{c}_{opp}	-0.40 (0.96)	-1.18 (1.09)	-1.12 (1.09)	0.13 (0.65)
N_{own}	-5.44* (2.86)			
$\sum c_{own} - \sum c_{opp}$		-0.39** (0.17)	-0.41** (0.18)	0.05 (0.11)
$ \sum c_{own} - \sum c_{opp} $			-0.16 (0.18)	-0.08 (0.12)
Regression Method	Interval	Interval	Interval	OLS
Mean Outcome	\$338	\$338	\$338	\$527

Notes: $N = 31,996$. * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. Observations from subjects assigned to the List letter. All the independent variables except N_{own} are expressed in hundreds of dollars (i.e., the estimates correspond to effects of \$100 changes in the independent variables). \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 the difference between total contributions to the own- and opposite-party. $|\sum c_{own} - \sum c_{opp}|$ is the absolute value of $\sum c_{own} - \sum c_{opp}$. See Table B.4 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. *Mean Outcome* corresponds to the average of the outcome variable over the entire sample. All regressions include as control variables the time elapsed until mailing delivery and a set of variables with the pre-treatment contributions to each candidate. Data on contributions from the FEC public records (see Table B.3 for descriptive statistics).

Table C.6: Comparison Channel: Robustness to Controlling for Area Fixed Effects

	Dep Var.: Amt. Cont. Post-Treatment			
	(1)	(2)	(3)	(4)
\bar{c}_{own}	3.197** (1.473)	4.836** (2.168)	0.006** (0.003)	0.006** (0.003)
\bar{c}_{opp}	-0.401 (0.956)	0.419 (1.462)	-0.000 (0.002)	0.000 (0.002)
N_{own}	-5.447* (2.862)	-7.497* (4.346)	-0.011** (0.005)	-0.010* (0.006)
Regression Model	Tobit	Tobit	Poisson	Poisson
ZIP-5 Fixed Effects?	No	Yes	No	Yes

Notes: $N = 31,996$. * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis, except for column (2) which uses bootstrapped standard errors. Independent variables as in the previous Table.

Table C.7: Estimating the Effects of Disseminating Unbiased Information about Contribution Records

	Post-Treatment Contributions				Pre-Treatment
	(1) \$	(2) \$	(3) \$	(4) P(\$>0)	(5) \$
List vs. No-Letter	19.450*** (6.097)			0.478* (0.273)	3.264 (4.631)
List vs. Placebo		15.823* (9.138)			
List-Update vs. List-Once			-1.251 (10.834)		
Observations	119,279	48,080	31,996	119,279	119,279
Regression	Interval	Interval	Interval	OLS	OLS
Mean Outcome	\$330	\$330	\$330	56%	\$524

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedasticity-robust standard errors in parenthesis. The dependent variable in columns (1) through (3) is the amount contributed post-treatment. The dependent variable in column (4) takes the value 100 if the individual made at least one contribution post-treatment and 0 otherwise. The dependent variable in column (5) is the dollar amount contributed in the pre-treatment period. All regressions except column (5) include the usual set of individual-level control variables: time since mail delivery and a set of variables on pre-treatment contributions to each candidate.

D The Post-Election Mail-In Survey and the Scale Up of the Conformity Effects

D.1 More Details and Descriptive Statistics About the 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, 44,380 in total, were a random sample of individuals from the No-Letter group (one third) and from the Website treatment group (two thirds).^{xi} The survey instrument and its envelope are shown 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. During the three months after we sent the surveys, we received 9,414 responses. This implies a response rate of 21.21%. 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. However, the mail-in survey response rate was 21.0% for subjects in the No-Letter group and 21.3% for recipients of the Website letter (difference not statistically significant – p-value of 0.357).

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). Such small effect of the lottery prize does not provide useful variation for the analysis. The fact that contributors did not react significantly to this economic incentive is probably due to the fact that the average recipient is relatively well-off and thus less sensitive to pecuniary incentives. 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

^{xi}As discussed in the paper, contributors to the Rand Paul primary made virtually no contributions in the post-treatment period to the Romney presidential campaign. We thus excluded this group from the sample for the mail-in survey.

of the prize.

Last, Figure D.1 shows the distribution of responses for two questions that were not included in the main body of the paper. Figure D.1.a shows the perceptions about the regulation of contribution limits, while Figure D.1.b shows the perceptions about contribution norms.

D.2 Further Details on the Scale-Up of the Conformity Channel Effects

D.2.1 The Identification Assumption and the Exclusion Restriction in the Estimation of the Conformity Channel

A straightforward way to think about the identification assumption for our conformity channel experiment is to consider the case of a binary treatment: “reading a letter a Website-Neighbors instead of a Website-Self letter” (R_i). We are interested in identifying the casual effect of this treatment. However, our experiment implies that we can only observe the effects of a related but different treatment, “being sent a Website-Neighbors instead of a Website-Self letter” (S_i). There are three possibilities regarding the relationship between R_i , S_i and contributions:

First, there may be an homogeneous effect of R_i on contributions, and an homogeneous effect of S_i on R_i . In this case, we do not need to make any assumptions other than the usual exclusion restriction for identifying the causal effect of S_i . We can compute the TOT effect by means of a Wald estimate, where the numerator is the ITT estimated in our experimental analysis, and the denominator is the effect of S_i on R_i (i.e., the reading rate). Since we do not observe the latter effect, we construct a proxy based on the post-election mail-in survey data.

Second, there may be an homogeneous effect of R_i on contributions, but heterogeneous effects of S_i on R_i . In this case, the denominator of the Wald estimate should be the average effect of S_i on R_i for the entire sample of subjects (i.e., not only respondents to the mail-in survey). To use the results from the survey, we need to assume that the average propensity to read the letter in the sample of survey respondents reflects the average for the entire sample of experimental subjects. There are several plausible instances in which this assumption can be violated. The most likely scenario is that survey respondents have less limited attention (or a lower opportunity cost of time), so that the reading rate is higher among survey respondents than among non-survey respondents. However, this source of bias would lead to an over-estimation of the reading rate and, in turn, an under-estimation of the scaled-up effects, as discussed in the previous sections.

A third case would have heterogeneous effects of R_i on contributions, and heterogeneous effects of S_i on R_i . In this case, we need to make all of the assumptions from Imbens and Angrist (1994).^{xii}

^{xii}Imbens, G.W. and Angrist, J.D. (1994), “Identification and Estimation of Local Average Treatment Effects,” *Econometrica*, Vol. 62 (2), pp. 467-475.

It should be noted that in our context we only have, by construction, compliers and never-takers, since we do not expect non-recipients to read the letter we sent to those selected for the treatment. As a result, the monotonicity assumption is satisfied automatically. We can also safely assume that sending someone a letter has no effect on the recipient if she does not actually read the letter. Under these conditions, the Wald estimate corresponds to the Local Average Treatment Effect of R_i – i.e., the effect on those who read the letter because we sent it to them, but would not have read it if we had not sent it. As in the previous case, to use the mail-in survey as a basis for the Wald estimate, we must rely on the assumption that the average propensity to read the letter in the sample of survey respondents is representative of the average reading rate for the entire sample of experimental subjects.

D.2.2 Qualifications of the Scale Up of Effects

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 No-Letter group and 21.3% for recipients of the Website letter, and this 0.37 percentage points difference is not only very small but also not statistically significant (p-value of 0.357).

A second concern is that the mail-in survey sample may not be representative of the subject pool, and thus the estimated reading rate may not correspond to that of the overall sample. However, individuals who were more likely to read our letter were probably also more likely to respond to our survey. For instance, more pro-social subjects, or those who had more free time, may have been more likely to have read our Website letter and to have responded to our mail-in survey. This type of bias implies an over-estimation of the reading rate and, in turn, an under-estimation of the scaled-up effects.

A third concern is that 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 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 conformity effects would still imply very large changes in contribution behavior.

Last, it should be noted that the mail-in survey’s response rate, 21.2%, was relatively high, which suggests that the reading rate for the original treatment letters may have been even higher. In fact, this response rate was more than twice what we expected and used for our power cal-

culations. However, the conditions of the survey mailing were very different than the conditions of the experimental mailing. First, 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. Second, contributors 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. On the contrary, we sent the mail-in survey a month after the election, when subjects were not receiving any correspondence related to the campaign. This also implies that the mail-in survey envelope was substantially less likely to be discarded unread than the mailpieces corresponding to the experiment.

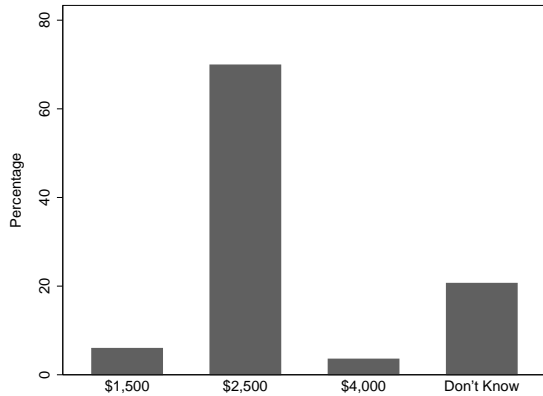
D.2.3 Alternative Estimate of the Scale Up of Effects

Section 4.1.3 presents results on the effect of the information in our treatment, with an estimate of the implicit reading rate of our letters 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 Website letter reduced the proportion of those being unsure about their answers about the publicity of contributions in our post-election mail-in survey. Alternatively, we could assume that the Website letter decreased the proportion of those who did not know that contribution records were public: i.e., it shifted individuals from any response category to being somewhat sure or very sure about the publicity of contribution records. The share of respondents who did not select any of these two categories was 25.6% in the No-Letter 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. This alternative estimate thus leads to an even 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.

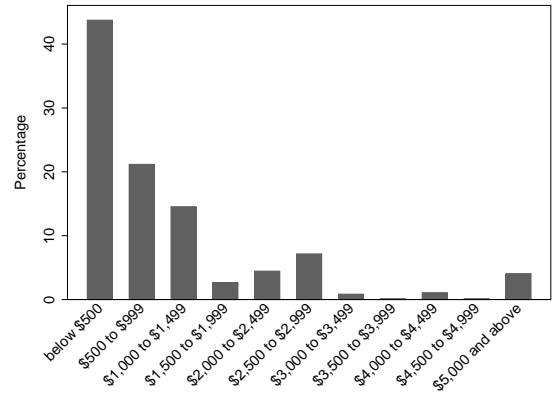
One reason why these two estimates differ is that the Website letter did not affect some individuals who reported to be somewhat or very sure that campaign records were confidential. Figure 3 presents the distribution of beliefs about the publicity of contribution data for survey respondents from the No-Letter and from the Website treatment groups. Receiving a Website letter did not modify the perception of respondents who reported to be very sure or somewhat sure that the contribution records were confidential. A possible interpretation is that those individuals report this because they interpret the FEC disclosure policy differently: e.g., they may argue that records are confidential because SuperPACs can be used to make veiled contributions, or because the contribution records are confidential for small donors (i.e., with contributions below the \$200 disclosure threshold).

Figure D.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: N=3,060 in panel (a) and 2,854 in panel (b). The data corresponds to the responses to our post-election mail-in survey by subjects in the No-Letter group. Panel a presents the histogram of responses to a question about the respondent’s knowledge of the maximum contribution level per committee (see question 3 from the questionnaire in Appendix D). 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 A.7).

E Evidence on the Uses of the FEC Online Search Tool

One potential concern with our findings is that the experimental effects for the conformity channel could be artificial, in the sense that individuals felt social pressure because of our mailing intervention, but would feel this type of pressure in their everyday lives. In this Appendix, we present some descriptive evidence based on the use of the FEC online search tool to argue that this type of social pressure based on campaign contribution records may arise in the course of normal (i.e., not experimentally induced) social interactions. This evidence is based on a proprietary dataset with anonymized records of the browsing history for a sample of millions of Americans from February 1st 2013 to October 31st 2014,^{xiii} a period corresponding to the 2014 U.S. Congressional election campaign cycle. This sample includes a small but non-trivial fraction of all visitors to the FEC website and related websites.

A first question is whether the FEC records are being accessed at all. We found that 10% of visitors to the FEC website (www.fec.gov) during that time period used the search tool for individual contributors (www.fec.gov/finance/disclosure/norindsea.shtml). This is a noticeably large share considering that the FEC website provides a number of tools that are not related to searching for individual contributors, and that are used routinely by public officials, politicians, campaign staff members, reporters, and academics. The link to the individual contributor search tool is not even featured on the FEC website's main page: during this time period, a visitor who wanted to reach the search tool on the FEC's website had to click first on "Campaign Finance Disclosure Portal," then on "Search," and finally on "Individual Contributor Search."

The FEC's search tool and the underlying information on contribution records are accessible through other websites as well. According to our browsing data, the number of searches conducted on each of those websites is even larger than the number of searches conducted on the FEC website. For instance, in two of these websites, the number of searches amounted to 133% and 150% of the comparable searches conducted on the FEC's website. While we do not have a reliable way of estimating the total traffic to these websites, our back-of-the-envelope calculations suggest that it is likely that millions of users conduct several million searches each year. Consistent with this, OpenSecrets.org, one of the websites providing a search tool that uses the FEC contribution records, reported nearly 35 million page views to its website in 2012, from more than 5 million unique visitors.^{xiv}

A second relevant question is what the results from searches of the FEC records are being used for. This is, of course, a much harder question, but we can nonetheless offer some suggestive evidence. First, we can make inferences based on the timing of visits to the FEC online search

^{xiii}Unfortunately, we do not have information on the socio-economic characteristics of the individuals included in the sample, so we cannot compare those characteristics with that of the U.S. population or that of contributors.

^{xiv}Source: <https://www.opensecrets.org/about/tour.php>, accessed on January 1st, 2015.

tool. If the purpose of accessing contribution records is to learn about candidates, the search tool should be used at a much higher rate in the months preceding an election. Figure E.1 depicts the proportion of visitors to the FEC website that use the online search tool on a monthly basis from February 2013 to October 2014. The percentage of visitors to the FEC’s website using the online search tool was roughly stable over the entire period, and it actually decreased as the election (held on November, 2014) drew closer.

We can also infer the purposes behind uses of the FEC online search tool from the types of searches conducted by its users. Our data provides us the criteria used in each search (e.g., searching by name, address, candidate) for one of the alternative websites that offers an online search tool very similar to the FEC’s. This search tool allows visitors to search contributors by name, state, ZIP code, employer, and/or candidate. The analysis of these searches reveals that 86% of the searches were conducted with the name of a contributor as the only criterion. Of the remaining 14%, less than half included the name of a candidate, whereas the remaining searches focused on particular ZIP codes or employers. These patterns are also consistent with the conjecture that individuals use the FEC online search tool to seek information about their peers rather than to seek information about the candidates.

While individuals search mainly for specific names, they may not necessarily focus on their peers: they may use these search tools to learn about the contributions of actors, politicians, CEOs and other celebrities. For instance, from time to time U.S. newspapers publish articles about the contributions of celebrities using data from the FEC online search tool. To explore this hypothesis, Figure E.2.a depicts the percentage of the total searches corresponding to names that have been searched for only once, to names that have been searched for twice, and so on. If the primary use for the search tool was to search for celebrities, then most traffic should be directed towards a relatively small number of names (i.e., the celebrities) with hundreds or thousands of searches each. The data strongly reject this hypothesis: the vast majority of the searches are for names that are searched for once or twice in the entire 21-month period (Feb. 2013-Oct. 2014) for which we have internet browsing data.^{xv}

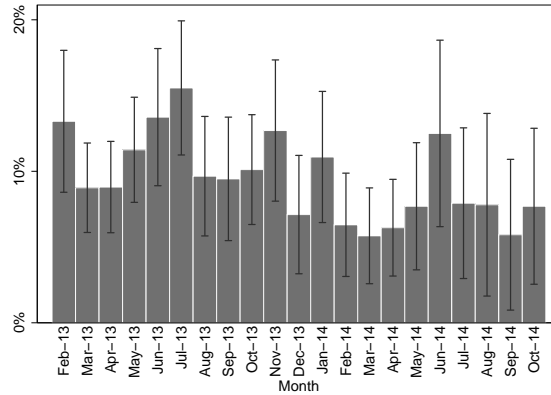
Finally, there would be less scope of social pressure from campaign contribution records if only a few professional users (i.e., campaign staff, fundraisers, etc.) account for the vast majority of searches. We can test this conjecture with our internet browsing data because it has (anonymized but unique) user identifiers. Figure E.2.b depicts the distribution of search activity across users of the FEC search tool. This figure shows the percentage of the total searches that can be accounted for by users conducting 1-10 searches, 11-20 searches, and so on. In the entire 21-month period

^{xv}Individuals may search for the same individual with different terms, i.e. “Angelina Jolie” and “Jolie, Angelina.” We did our best to clean up the data to deal with these cases, although of course we have not been able to capture all possible variations. We are confident however that the main patterns are robust to these measurement errors.

before the 2014 congressional election, the majority of users (62.5%) conducted between 1 and 10 searches. If we define professional users as those searching more than 100 times in the entire 21-month period, then this type of user only accounts for 13.7% of the total searches. Professional users, thus, explain a significant share of activity on the FEC online search tool, but they do not account for the majority of the traffic.

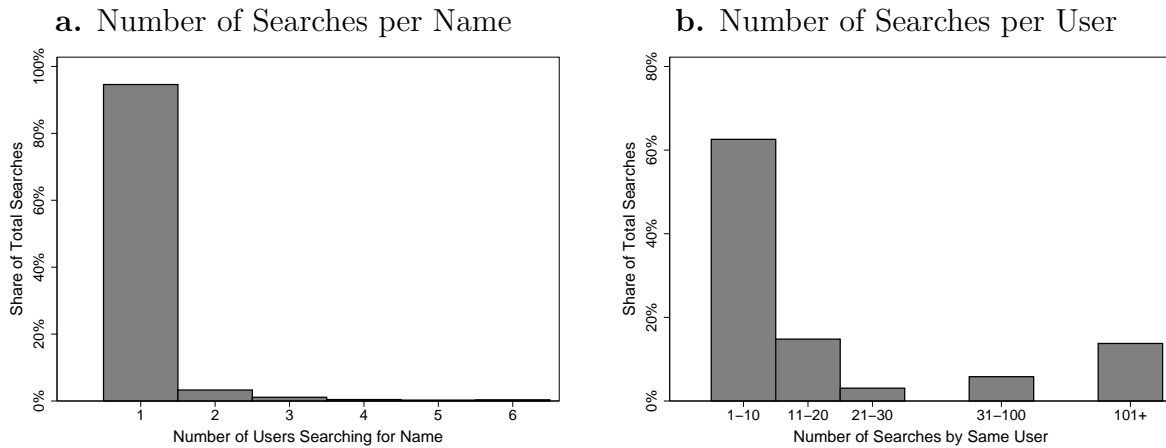
In summary, this evidence suggests that millions of Americans (and not only campaign professionals) access the FEC contribution records, possibly to learn about the political affiliation of their peers (neighbors, friends, coworkers, and subordinates) rather than to gather information about candidates.

Figure E.1: Visits to the Online FEC Search Tool Over Time, 2013-2014 (Internet Browsing Data)



Notes: Share of visitors to the FEC website (www.fec.gov) who conduct at least one search on the FEC online search tool for individual contributors (www.fec.gov/finance/disclosure/norindsea.shtml). The whiskers represent 95% confidence intervals for each monthly average. Source: panel of Internet users.

Figure E.2: Type and Distribution of Searchers using the FEC Online Search Tool (Internet Browsing Data)



Notes: Panel (a) presents the percentage of the total searches corresponding to names searched only once, twice, and so on. This data corresponds to all searches conducted in a popular search tool (similar to the FEC’s search tool, and based on the same records) where the user completed the “name” field. Panel (b) presents the percentage of the total searches in the FEC online search tool for individual contributors (www.fec.gov/finance/disclosure/norindsea.shtml) that can be accounted for by users conducting 1-10 searches, 11-20 searches, and so on. Source for both figures: panel of Internet users, February-2013 to October-2014.

F Conformity Effects in a Model of Partisan Signaling

F.1 The Model

In this Appendix, we formalize the intuition behind the conformity channel. For that, we present a model where contributions can be used as a signal of the political affiliation of the contributor. This model follows the tradition of a signaling approach to social interactions, as in Bernheim (1994) and in Bénabou and Tirole (2006).

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 this may represent something more general than just geographic proximity. Reference groups 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 α_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 α_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. Each individual belongs to one reference group, and interacts with other members of the same group, which we also 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 civic 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 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. It is thus unlikely that individuals use campaign contributions primarily to signal those traits.

When an individual interacts with a neighbor, the utility an individual can expect from this interaction 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 $1 - v$.^{xvi} Contributions are made prior to the interactions with neighbors.^{xvii} 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 μ in conjunction with the proportion

^{xvi}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.

^{xvii}Note that the individual does not know whether her contribution will be observable to others when deciding about her contributions.

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 interact with sympathizers of their own political party. The parameter μ 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 $\mu = \frac{1}{2}$ corresponds to 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.^{xviii} Alternatively, $\mu = \frac{1}{2}$ 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 own-party neighbors relatively more.

We make two simplifying assumptions to make the model tractable. First, we assume that α_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 $\alpha_R^* \in (0, \bar{\alpha})$.^{xix} 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 Ω_R (Ω_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:

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

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:

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

^{xviii}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.

^{xix}Note that we implicitly assume an interior solution.

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, α_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:

$$\alpha_R^* = v \gamma (S_R - 1 + \mu) \frac{\min \{\alpha_L^*, \bar{\alpha}\}}{\min \{\alpha_R^*, \bar{\alpha}\} - \max \{\alpha_L^*, \underline{\alpha}\}} + K \quad (6)$$

The analogous expression for a left-wing individuals is:

$$-\alpha_L^* = v \gamma (S_R - \mu) \frac{\min \{\alpha_R^*, \bar{\alpha}\}}{\min \{\alpha_R^*, \bar{\alpha}\} - \max \{\alpha_L^*, \underline{\alpha}\}} + K \quad (7)$$

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 Θ is that the solution is interior, i.e., $\underline{\alpha} < \alpha_L^* < \alpha_R^* < \bar{\alpha}$.^{xx} 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 also 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.^{xxi}

Proposition 1. *Given parameter values in a non-empty set Π , a signaling equilibrium exists, it is unique and it belongs to Θ .*

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 Θ , 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.*

^{xx}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.

^{xxi}Even though the propositions focus on the more plausible equilibria in Θ (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.

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 Θ , 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 $S_j < 1 - \mu$, 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.^{xxii}

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.

^{xxii}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.

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 Θ , an increase in visibility of contributions (v) will increase geographic polarization.*

F.2 Proof of Propositions

F.2.1 Proof of Proposition 1

From equation (7) we obtain:

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

Using the quadratic formula:

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

We only use the left root because the right root cannot have simultaneously $\alpha_L^* < -\frac{K}{2}$ and $\alpha_R^* > 0$. Note that we also need $(v\gamma(S_R - \mu) + K) > 0$, which implies that $\alpha_L^* < 0$. We can replace in equation (6):

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

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 α_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 (6) 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 Θ :

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

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

F.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.

F.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 $S_j < 1 - \mu$.