Social Incentives in Contributions
Field Experiment Evidence from the 2012 U.S. Presidential Campaigns*

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Abstract

This paper provides new evidence on the mechanisms through which social incentives operate, based on a field experiment with 2012 U.S. presidential campaign contributors. We sent letters with individualized information to 91,908 contributors and measured how those letters affected the recipients’ subsequent contributions, using administrative data. We found that exogenously making an individual’s contributions more visible to her neighbors significantly increased her subsequent contributions if the majority of her neighbors supported her same party, but it decreased contributions if the majority of neighbors supported the opposite party. This constitutes evidence that contributions are used as a signal of political affiliation. In another treatment arm we induced non-deceptive exogenous variation in the information observed by an individual about the contribution behavior of her neighbors. Consistent with social norms, individuals contribute more when neighbors of the same party contribute higher average amounts, although the effect is absent for neighbors of the opposite party. We also find evidence of moral free-riding: i.e., perceiving that one’s party is raising higher total contributions relative to the opposite party causes the individual to contribute less. We complement the experimental results with quasi-experimental evidence from an event-study of residential mobility including nearly 100,000 contributors. Consistent with the experimental evidence, an individual’s contribution is significantly affected by the political composition in her reference group. These reference-group effects can explain about 20% of the observed geographic polarization in contributions in the United States. We discuss the implications of the findings for fundraising strategies and for the design of optimal disclosure policies.

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

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1 Introduction

A growing body of research shows that social incentives are important factors motivating contributions and other forms of pro-social behavior. For example, there is evidence that social incentives can affect environmentally-responsible behavior (Allcott, 2011), voter turnout (Gerber et al., 2008) and charitable giving (Martin and Randal, 2008). Research on social incentives will be of interest to policymakers and organizations that rely on private contributions as it may present more cost-effective alternatives to traditional economic incentives. Understanding the precise mechanisms through which social incentives operate is important for the optimal use of social incentives. For example, if there are social mechanisms operating in opposite directions, we would want to identify and use the social incentive that operates in the desired direction. However, the empirical identification of these precise mechanisms has proved elusive. In order to address these open questions, we exploit the unique institutional setting of U.S. campaign finance. We provide evidence on social incentives in the context of campaign contributions based on a field experiment with nearly 200,000 contributors during the 2012 U.S. presidential election.

Federal law dictates that campaign committees must report the identity and detailed information of individuals who contributed $200 or more to the Federal Election Commission (FEC). The FEC makes these contribution records public and easily accessible online. The data is updated on a monthly basis over the course of an election cycle and all contributions and contributors are listed in a disaggregated way. The FEC offers an online tool to search contributors by full name, address, and other characteristics (e.g., employer). The database displays this information as well as the amounts contributed, the dates of the contributions, and the recipient candidate. We selected 191,832 early contributors from this database to form our subject pool. We sent letters with individualized information about campaign contributions in the area of residence to a random sample of 91,908 these subjects. We observed the subsequent contributions for all subjects, again, using the FEC administrative data. We created non-deceptive exogenous variation in the information contained in the letters to test the specific mechanisms by which social incentives may operate and estimated the effect of the information contained in each letter on the recipients’ subsequent contribution patterns.

The first set of experimental findings concern signaling effects: i.e., how an individual’s contribution is affected by the fact that her contribution is visible to others. For instance, contributions may signal unobserved characteristics of the individual, such as her wealth, party of political affiliation and strength of affiliation. To test the presence of signaling effects, we induced experimental variation in the information contained in the letters to test the specific mechanisms by which social incentives may operate and estimated the effect of the information contained in each letter on the recipients’ perceptions of the visibility of
their contributions to their neighbors. We sent letters providing information about the public nature of campaign contribution records and about the Federal Election Commission’s online search tool to a random sample of our experimental subjects. We introduced variation in the visibility of the recipients’ contributions by randomly assigning the recipients to one of two sub-treatments. One sub-treatment indicated that the household was the only household randomly chosen to be sent a letter of this type. The other sub-treatment indicated that the household and other households in the area were randomly chosen to be sent a letter of this type. Since that was the only difference among the sub-treatments, the differential effect between these two sub-treatments measures the effect of making one’s contribution more visible to others. We find that making the recipient’s contribution more visible significantly increased her subsequent contributions if the majority of her neighbors supported the recipient’s party, but it decreased contributions if the majority of her neighbors supported the opposite party. These effects are not only statistically significant but also large in magnitude. This constitutes evidence that individuals use contributions as a signal of political affiliation. These results are consistent with a signaling model where contributions signal the contributor’s party and strength of affiliation.

The second set of experimental findings are about informational effects: i.e., how an individual’s contributions depend on the individual’s perception on the contribution behavior by other individuals in her reference group. That is, while signaling effects measure how the individual’s contribution is affected by being observed by others, informational effects measure how the individual’s contribution is affected by the individual’s observation of contribution behavior by others. The typical example of informational effects is shaped by social norms, which relate to how individuals form beliefs about what is the “right” amount to give based on the contributions made by others in their reference group. Our experimental design allows to test for this and other types of informational effects. The type of letter designed to test the presence of these effects did not provide any information about the FEC search tool, but instead provided a semi-anonymized list of 10 contributors from the recipient’s area of residence and their respective contributions. We introduced exogenous variation in this list in a non-deceptive way by randomly varying the parameters employed to select which of the many individuals from the individual’s neighborhood were to be included in the list of 10 contributors shown in the letter. This created exogenous variation along multiple dimensions of the neighbors’ contribution patterns: e.g., some recipients were mailed lists with a higher proportion of own-party contributors, while others were mailed lists with higher average contributions by opposite-party contributors, and so on. The results from this treatment suggest that an individual’s contribution is affected by the perceived contribution behavior of others. Consistent with the formation of a social norm about contributions, individuals
contribute more when neighbors of the same party contribute higher average amounts, although the effect is absent for neighbors of the opposite party. Individuals contribute less when they perceive that their party is raising more money than the opposite party, which we interpret as a moral free-riding effect. Individuals do not seem to contribute more when campaign contributions appear to be even, which serves as evidence against the hypothesis that individuals contribute because they think they may be making a pivotal contribution.

Our research design builds on a body of work that exploits laboratory and field experiments to study the role of social incentives in contribution and related behavior. In a seminal contribution, Gerber, Green and Larimer (2008) present results from a large field experiment with registered voters in the United States. They sent letters containing information about past voter turnout in that household and other households in the neighborhood, and promised to send the recipient and her neighbors updated information following the upcoming election. The authors find that these letters induced substantially higher levels of turnout, which they interpret as arising from some combination of social norms and social pressure. This evidence is consistent with other studies that find that an individual’s contribution increases when the individual’s behavior is observable to others (Andreoni and Bernheim, 2009; Andreoni and Petrie, 2004; Karlan and McConnell, 2012) and when the individual is informed about the contributions of others (Frey and Meier, 2004; Allcott, 2011).\footnote{There is related evidence in other contexts, such as charitable giving (Dellavigna et al., 2012), voting turnout (Dellavigna et al., 2013) and campaign contributions (Augenblick and Cunha, 2011).}

One of the goals of this research is to provide original experimental evidence about the relevance of social incentives for campaign contributions. Individual campaign contributions represent a large fraction of the resources devoted to funding campaigns in the United States: e.g., about 80% of the $1.7 billion raised in the 2012 presidential race was comprised of individual contributions. For this reason, economists have shown interest in the political economy implications of campaign contributions (e.g., Campante, 2011). In order to understand the consequences of campaign contributions, we must first understand what factors drive these contributions. However, these factors are not yet well understood (Ansolabehere et al., 2003). Our evidence indicates that social incentives play a significant role in campaign contributions. We find that individuals do not seem to contribute more when campaign contributions appear to be more even, which is an indication that they do not contribute because they expect their donation to affect the election’s outcome. This evidence is consistent with the predominant, although debated, view that individuals contribute to campaigns primarily...
because of a consumption motive (Ansolabehere et al., 2003).

A second goal of this research is to address open questions about the mechanisms through which social incentives operate. The institutional setting of campaign contributions allowed us to accomplish this goal. Our evidence supports the notion that contributions can serve as signals of unobserved contributor characteristics. Furthermore, our findings illustrate that the value of the signal can depend on audience characteristics - in our application, the most relevant audience characteristic was its political composition. Our findings also support the notion that individuals form a social norm about the amount that they contribute and show that, when forming this norm, individuals do not weigh everyone’s action equally (Akerlof and Kranton, 2000), but they construct a norm based on information about individuals with whom they share characteristics - in our application, the most relevant characteristic was area of residence and political party. We also identified a social incentive that we denominate more free-riding for which, to the best of our knowledge, there was no prior evidence.

Insights on informational effects are of interest to political parties, charities and other organizations that seek to improve fundraising strategies. Our experimental findings suggest that disseminating unbiased information about contribution patterns in a given area can increase contributions. However, the evidence also suggests that some of the information dissemination efforts regularly employed by these organizations may be self-defeating. For example, while the social norm mechanism suggests that individuals respond positively to the average amount contributed by others, they may react negatively to the total amount contributed by others, due to moral free-riding. The results also provide valuable insights for the design of optimal disclosure policies. Technological change is constantly reducing the cost of collecting and disseminating information. The government, but also the private sector, must decide how to disclose this data, and this question has attracted relatively little attention (for an exception see Card et al., 2012). Irrespective of the desirability of information transparency, our evidence indicates that disclosure policies, through social incentives, may affect the actions of the individuals whose information is being disclosed, for at least two reasons. An individual’s actions may depend on the belief that others are observing, and also on what the individual observes from others’ actions. These two factors correspond to the signaling and informational channels covered in our analysis. In addition to providing insights on how to evaluate the effects of disclosure policies in the context of campaign contributions, our experimental design can be replicated to study the impact of similar policies in other contexts, such as charitable giving or tax evasion.

Our study of social incentives is also relevant to a body of research that uses non-experimental evidence to examines the role of social interactions in campaign contributions and other forms of political participation (e.g., Cho, Gimpel and Dyck, 2006). Social effects
may provide a powerful explanation for the stark patterns of geographic polarization observed in U.S. campaign contributions and other forms of political participation. In line with this literature, our experimental evidence indicates that an individual’s contributions are affected by the political composition of her reference group. However, some mechanisms have conflicting effects: e.g., the evidence on signaling effects and contribution norms suggest that an individual should contribute more when living in an area with a higher share of supporters of her same party, while the evidence on moral free-riding makes the opposite prediction. We complement the experimental evidence with quasi-experimental evidence on the relationship between individual contributions and the political composition of the area of residence. We combined the FEC administrative data on individual contributions with residential mobility information from the USPS’ National Change of Address database to form a panel of nearly 100,000 individuals who contributed during the 2008 presidential campaign and moved before or after the 2012 election. We used this framework to estimate reference group effects: i.e., how the party composition in her area of residence affects an individual’s contributions. We find that an increase in the share of neighbors supporting an individual’s own party causes the individual’s contribution to increase.\(^2\) Reference group effects are large in magnitude and make a significant contribution to geographic polarization: i.e., a counterfactual analysis indicates that geographic polarization in campaign contributions during the 2012 election cycle was 20\% higher than what it would have been in absence of reference group effects.

The paper is organized as follows. Section 2 presents the institutional context, the data sources and the general design of the field experiment. Sections 3 and 4 discuss the experimental evidence on signaling effects and informational effects, respectively. Section 5 presents the quasi-experimental evidence regarding the relationship between an individual’s contribution and the political composition of its reference group. The final section concludes.

## 2 Experimental Design and Data Description

### 2.1 Institutional Context and Data Sources

Our research design builds on the unique institutional setting of U.S. campaign finance. Federal law dictates that campaign committees must report the names, addresses and other information of individuals who make contributions of $200 or more to the Federal Election

\(^2\)We provide evidence that this correlation has the presumed direction of causality with a falsification test based on an event-study analysis: while an individual’s contribution is affected by the composition of the reference group to which the individual moved before the start of the election, the individual’s contribution is not affected by the composition of the reference group to which the individual moves after the end of the election.
Commission (FEC). The FEC makes these contribution records public and easily accessible online. The data is updated on a regular basis over the course of the election cycle and all contributions and contributors are listed in a disaggregated way. On its website, the FEC offers a tool that allows users to search individuals by full name, address and other characteristics (e.g., employer). The database displays personal information as well as amounts contributed, candidates contributed to and the dates of the contributions.\(^3\)

We conducted our field experiment during the 2012 U.S. presidential election. Using the records published by the FEC, we identified a group of contributors to the 2012 presidential campaign and sent personalized mailings to them. The letters included information on campaign contributions drawn from the FEC online database. Some of the information contained in the letter was randomly varied in a non-deceptive way. Finally, using the same FEC records, we measured the effect of the information contained in the mailings on the subsequent contribution behavior of the recipients.

We began with a subsample from the FEC contribution records consisting of 280,456 unique individuals who made at least $200 in contributions to a presidential campaign committee from April 1, 2011 to April 1, 2012, as listed in the FEC database.\(^4\) We did not include contributions to PACs, SuperPACs, or political parties.\(^5\) While the FEC’s records are remarkably comprehensive and complete, there were still some instances of missing or inconsistent information. Since the number of individuals in this initial sample was substantially higher than the number of subjects needed for our experiment, we took a highly conservative approach by limiting the subject pool to individuals with the highest quality of available information (e.g., quality of address information). Additionally, we applied a number of arbitrary criteria, such as excluding contributors from Washington D.C. and excluding individuals geographically isolated from other contributors. Appendix E provides further details about the criteria used to select the subject pool.

After applying these criteria, our final subject pool included 191,832 individuals.\(^6\) Table 1 presents summary statistics of individual characteristics from our experimental sample of earlier contributors (first column) compared to all contributors to presidential campaigns from

\(^3\)See Appendix D for more details about this online search tool.

\(^4\)This sample was obtained from the FEC’s public records April 25, 2012, and includes contributions made until April 1 of that year.

\(^5\)For the sake of simplicity, we only consider direct individual contributions to presidential committees, which excludes other types of contributions, such as a loan to a candidate.

\(^6\)This sample also excludes 1,002 individuals who were sent letters deemed undeliverable or redirected by USPS. The results are robust to alternative treatments of those observations. We took several measures to clean the address information from the FEC database, including geocoding, cross-checking information across different records for the same individual, and matching the data to the USPS National Change of Address database. Our mailing provider indicated that even carefully cleaned databases of addresses usually resulted in about 5% of undeliverable letters, so in this respect our efforts were successful.
the 2012 election cycle (second column) and to the general U.S. population (third column). The comparison between the first two columns indicates that the average contributor in our sample was fairly representative of all contributors in the 2012 presidential election insofar as they exhibit similar socio-economic characteristics, including racial composition and income. There are, however, some differences in contribution patterns between the two groups. Our subject pool contains a lower share of contributors to the Obama campaign. This is due to the fact that our subjects were early contributors and, because of the Republican primary, Republican candidates started their campaigns earlier. Our subject pool also has higher average contributions, which can be partly attributed to the fact that Republicans, who are over-represented in the experimental sample, contributed higher amounts. Finally, the comparison of the first two columns with the third column illustrates the well documented fact that contributors are significantly different from the average U.S. citizen in several ways: e.g., contributors are more likely to be males, white and more likely to live in urban and wealthier areas.

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

2.2 Timing of the Experiment and Outcomes of Interest

Figure 1 provides a timeline of the key events in both the election cycle and the implementation of our field experiment. The letters were sent on May 6, 2012. We chose this date for a number of reasons. Power calculation exercises based on contributions during the 2008
election cycle indicated that sending the letters in May would be optimal since the expected post-treatment probability of making contributions would be close to 50%. Moreover, the Republican National Committee declared Mitt Romney the party’s presumptive nominee a week earlier on May 2, 2012. Sending the letters when each party had a single presidential candidate simplified the outcome variable as we would not have to compare contributions from the same individual to different candidates. The outcomes of interest throughout our study (unless stated otherwise) are the individual campaign contributions made to the Obama or Romney committees from the time of delivery of our letter (as indicated by USPS records) to the end of the election cycle, December 31, 2012. We label these contributions from individuals in our subject pool “post-treatment.” The pre-treatment amounts, which we employ for the purpose of falsification tests, correspond to the total contributions made from April 1, 2011, to the date the letters were delivered.

Table 3 describes the contribution patterns before and after treatment for individuals in our subject pool. The top panel presents detailed statistics for the pre-treatment period, during which 100,541 individuals (52% of the subject pool) contributed to Obama, 24,910 (13%) to Romney, and 66,381 (35%) to other Republican candidates. On average, individuals contributed $523. The average amount contributed pre-treatment was higher for Republican than for Democratic contributors (again, this was expected given that the Republican candidates had a primary election campaign). The bottom half of Table 3 presents similar statistics for post-treatment contributions. In the post-treatment period, 49% of our sample of earlier contributors made at least one contribution to either Obama or Romney, and for those making at least one post-treatment contribution, the average amount was about $589, only slightly higher for Republican ($610) than for Democrats ($567). There were, however, substantial partisan differences in the likelihood of making a post-treatment contributors: while 76% of pre-treatment Obama contributors made at least one post-treatment contribution, 38% of Romney supporters and only 12% of other Republican contributors did the same. In order to account for these differences, we include measures of the pre-treatment contributions as control variables in the regression analysis. In practice, the results are similar if we do not include these control variables.

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8See Appendix E for details about how we define the date of delivery for each individual according to USPS records. For individuals in the Control group, the date dividing pre- and post-treatment contributions corresponds to the median date when the letters were delivered in that 5-digit ZIP code. The results are robust if instead we define pre- and post- contributions using the date that the letters were mailed.

9When the dependent variable is the amount contributed, we use an interval regression model to take into account the censored nature of the outcome. Note that if a Republican did not make a pre-treatment contribution to Romney, the fact that she appears as not making a post-treatment contribution to Romney is consistent with having made a contribution to Romney below $200. We use the interval regression model instead of a simple censored regression model to take that information into account. In practice, the interval regression model and the censored model yield very similar results.
2.3 Treatment Types and Content of the Letters

As mentioned above, our field experiment included three main treatment types (Awareness, List and Placebo) as well as sub-types. Appendices A.1-A.5 show samples of the letters for different treatment types and sub-types.

All the letters shared basic characteristics. They all included the same header (“Boston, April 25th 2012”) and the same last paragraph: “This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University. You can find more information about this project, including contact information, on our website.” The letters included the web address of the project’s website, shown in Appendix B, which provided basic information about the research project, and contact information to reach the research team and the University’s Institutional Review Board. The main purpose of the website was to provide contextual information about our study to interested subjects, and to dissipate any doubts about its legitimacy, emphasizing its academic and non-partisan nature. Although the website provided some general information about the main research objective, to avoid the contamination of the experimental results, the website did not provide any details about the precise hypotheses to be tested, nor about the existence of several different treatment types.\(^\text{10}\)

The outside of the mailpiece, a sample of which is shown in Appendix A.6, was also the same for all treatment types.\(^\text{11}\) 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.

Beyond these common characteristics, the three categories of letters, Awareness, List and Placebo included different content. The key analysis in our experimental design does not

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\(^\text{10}\)We directed individuals who were interested in receiving a debriefing brochure (a non-technical summary of the study’s main hypotheses and results) to sent an email to an email address. We sent the brochure only after the data collection process was completed.

\(^\text{11}\)The mailing consisted of a single sheet of paper that folded and sealed to make a letter-size mailpiece.
rely on the comparison of outcomes between individuals who were sent a given type of letter and those in the control group who were sent no letter. Instead, the research design relies on the comparison of outcomes among recipients of a given letter type, but with randomly assigned subtle variations in the information contained in the letters. The Awareness letter was designed to test the presence of signaling effects by inducing experimental variation in the degree to which individuals may have felt observed in their contribution activity. The List letter was designed to test the presence of informational effects by generating experimental variation in the information individuals observed about their neighbors’ contributions. Sections 3 and 4 contain further details about the letter types and the hypotheses they were designed to test.

The key tests for the presence of social incentives do not rely on comparison of post-treatment outcomes between individuals who were sent a given type of letter and those in the control group who received no letter. However, this simple comparison can be useful for complementary analysis. The main limitation for making this comparison is that the potential effects of the letters may be unrelated to the information they contained. 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 informational treatment 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 feel observed by the researcher. In anticipation of these potential concerns, we devised a Placebo letter as a treatment arm. The outside of the mailpiece, the inside header and footer, and the paragraph explaining the general purpose of our mailing were identical to those of the other two treatments. However, instead of providing any meaningful information about the disclosure policy on campaign contributions, or about the recipient’s neighbors contributions, the Placebo letter contained standard regulatory information about contribution limits, taken verbatim from the FEC’s website. We did not expect this information to have an effect on contributions, because these regulations are generally well known, and, most importantly, because contribution limits were not binding for virtually all individuals in our subject pool. If the Placebo letters generated any effect, it would suggest that part of the differences between the Awareness and List treatments and the Control group could be the result of some of the confounding factors mentioned above.

12See for example Figure F.1.a in the Appendix.
2.4 Distinction between Intention to Treat and Treatment on the Treated Effects

The empirical analysis of our field experiment consisted of establishing the impact of sending letters with different types of information on the distribution of post-treatment contributions. It should be noted, however, that the nature of treatments implies that a substantial share of experimental subjects (most likely a majority) may not have opened or even received the letters sent. Those letters that were received and opened may have been discarded before being read. Our estimates thus measure the intention to treat effects of sending these letters. The treatment effect on the individuals who read the letters is a multiple of this intention to treat effect. For example, if we assume that half of the individuals who were sent a letter actually read it, the treatment effect for those who read the letter would be twice the intention to treat effect. As described above, the letters were designed to minimize the likelihood that they would be discarded before being read. Despite our best efforts, however, the letters were still likely to be disregarded as unsolicited mail. Indeed, the Environmental Protection Agency estimates that about 50% of unsolicited mail is discarded without even being opened. This statistic provides a conservative upper bound to the share of experimental subjects who read the letters. A lower bound for the number of individuals who read the letters and paid attention to their content is given by the number of unique visitors to the project’s website, which amounted to about 5% of the total number of letters sent. These lower and upper bounds, however, are not very informative, since they imply scale-up factors for the intention to treat effects ranging from 2 to 20. Motivated by this limitation of the data, we conducted a post-election survey that - among other things - would give us a more precise estimate of the magnitude of the scale-up factor.

2.5 Post-Election Mail-In Survey

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

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13Note that the treatment effects for individuals who read the letter could be different than the treatment effects for individuals who did not read the letter (in the counterfactual case of reading it) if these two groups of individuals differ in key characteristics (e.g., if those prone to reading unsolicited mail are more sensitive to social incentives).
objective was to exploit the survey data in combination with the experimental results to scale-up the intention to treat effects into treatment effects on the treated.

The survey was sent by mail on December 6, 2012, one month after the 2012 presidential election day. The intended recipients, 34,966 in total, were a random sample of individuals from the Control group (one third) and from the Awareness treatment group (two thirds). The envelope contained a letter and the survey on two sheets of paper, and a smaller prepaid business-reply envelope. The recipient was asked to fill out the survey and mail it back in the provided envelope by dropping it in a USPS mailbox. Appendix C presents a sample of the survey instrument. As an incentive for participation, the letter informed recipients that there were lottery prizes for individuals who responded and mailed back the survey before January 31, 2013.

The survey instrument included questions on gender, age, five questions about knowledge of campaign finance law, and a final subjective question about how much an individual should contribute to presidential campaigns. We received 9,414 responses, which implies a response rate of 21.21%. 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 survey results discussed below thus over-represent Democrat contributors. Half of the recipients were randomly assigned to be eligible for 10 lottery prizes of $100 each, while the other half were eligible for 10 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. However, the small effect of this intervention on the response rate rendered it ineffectual.

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14 In order to maximize statistical power, we excluded from the survey sample those contributors whose largest pre-treatment contribution went to Ron Paul, because of their very low baseline rate of post-treatment contributions (below 2%).

15 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.

16 Most of the respondents mailed back the completed survey over the two months following the delivery date of the mail-in survey.

17 The response rate was only half a percentage point higher in the group eligible to the higher lottery prize, which implies an increase in the response rate of approximately 2.6%. This effect is relatively small and not statistically significant (p-value of 0.16). The fact that contributors did not react to this economic incentives probably reflects the fact that most of the recipients were relatively well-off. Nevertheless, we must note that the odds of receiving a prize were low, which could have resulted in very small differences in the perceived expected value of the prize.


3 Experimental Results: Signaling Effects

3.1 Relevant Hypotheses

Literature on charitable giving and other pro-social behavior suggests that individuals condition their behavior based on whether their actions are observed by others. For example, Andreoni and Petrie (2004) find that individuals contribute more in laboratory experiments when contributions are not confidential. Karlan and McConnell (2012) implemented a field experiment with contributions to a university, and find evidence of higher donations for a randomly selected group of potential contributors who were told that the names of donors would be publicized. One possible interpretation for those findings is signaling: i.e., the actions of the individuals may serve as a signal of the individual’s unobserved characteristics. For instance, Andreoni and Bernheim (2009) find in laboratory experiments that an individual acts more generously when her actions are observable because she believes that others will infer from her behavior how fair she is. Our methodological approach is also closely related to a literature on social pressure and voting turnout. In a seminal contribution, Gerber, Green and Larimer (2008) conducted a field experiment where individuals were sent letters with lists of neighbors and their previous voting turnout history. The letters also promised to publicize the recipient’s future voting habits to her neighbors. The authors find that these letters had a significant positive effect on subsequent turnout, which they interpret as arising from some combination of social norms and feeling monitored by neighbors.

Compared to charitable giving or voter turnout, campaign contributions present a more complex context for the study of signaling effects. While most would consider charitable contributions and voting to be socially desirable, contributing to a candidate may be viewed differently depending on the political affiliation of the audience. Unlike studies of voter turnout, where the data on the individual’s party was not publicized, the publicizing of campaign contributions reveals an individual’s candidate preference and the strength of her affiliation. As a result, making a higher contribution to a Democratic presidential candidate may generate positive reactions from Democratic neighbors and negative reactions from Republican neighbors. Indeed, there is some evidence suggesting that individual care about revealing or concealing their party affiliation to others: while some individuals seem to announce their affiliation with public statements like putting a sign in their yard (Makse and Sokhey, 2012), some other individuals do not want their political affiliation to be observable

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18Other field experiments that measured the role of social pressure include DellaVigna, List and Malmendier (2012) with door-to-door fundraising, Chetty et al. (2012) with peer review at an academic journal and Dellavigna et al. (2013) with voter turnout.

19A number of studies have extended this analysis. For an overview of this literature, see Green and Gerber (2010).
to others (Gerber et al., 2013).

We can illustrate types of signaling effects that can arise in the context of campaign contributions. Suppose an individual $i$ is considering whether to make a contribution of amount $c_i$ to her preferred political party. Each individual belongs to one reference group, and interacts with other members of the same group, which we refer to as neighbors. In these interactions, some characteristics of the individual are not perfectly observable: e.g., whether the individual has a sense of civic duty, whether the individual is wealthy, her party affiliation and strength of the individual’s political affiliation. The interaction may be more or less beneficial for the individual depending on what the neighbor believes about the individual’s characteristics. For instance, neighbors may act more positively toward individuals that are civically responsible, wealthier, and share the neighbor’s political preference.

While these characteristics are not directly observable to neighbors, contributions can be observed with some probability. We will assume that the individual’s contribution is observed by a given neighbor with probability $\nu$, and not observed with probability $1 - \nu$. For example, $\nu$ represents the probability that neighbors look up the individual’s contribution records using the FEC search tool. Individual’s $i$ expected utility from interacting with neighbor $j$ can be summarized by the following equation:

$$(1 - \nu) \Omega + \nu \cdot \{ \lambda \cdot q^j_i(c_i) + \gamma \cdot b^j_i(c_i) \}$$

With probability $(1 - \nu)$, the contribution $c_i$ is not observed by the neighbor, in which case the individual gets a utility $\Omega$ that does not depend on $c_i$. With probability $\nu$, the contribution is observed and the neighbor can infer some of $i$’s characteristics - in a probabilistically sense - from the observed contribution (or lack thereof). For instance, $q^j_i(c_i)$ can represent the neighbor’s perception about the individual’s civic responsibility. Thus, a $\lambda > 0$ would represent neighbors that give preferential treatment to individuals with more civic responsibility. If that was the case, then making contributions more visible to neighbors should increase the individual’s incentives to contribute, but this increase should be independent of the political composition of the reference group.

$b^j_i(c_i)$ can represent is the neighbor’s inference about how attached the individual is to the neighbor’s preferred party. This can take a positive value if the individual is believed to support the neighbors’ same party, and a negative value if the individual is believed to support the opposite party. A $\gamma > 0$ would represent neighbors giving preferential treatment to

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20 An alternative interpretation of the probability parameter $\nu$ is that the contribution information is always a matter of public record, but each individual is uncertain as to whether his/her neighbors know about the existence of these records or about their publicity, or that, if they know about it, the individual is uncertain as to whether others would try to access them.
individuals with similar political preferences. Making contributions more visible to neighbors will increase the utility of making a contribution if the neighbor is of the same party than the individual, but it will decrease the utility of making a contribution if the neighbor is of the opposite party. Therefore, the effect of increasing the visibility of contributions should depend on the political composition of the reference group. Intuitively, we would expect that increasing the visibility of contributions would raise an individual’s desired contribution if most neighbors support the individual’s same party, but decrease her desired contributions if most neighbors identify with the opposite party.

Even though the above framework outlines the basic intuition, it is not a substitute for a formal signaling model. Appendix H provides such a model, and addresses some important issues. This Appendix shows that, consistent with the intuition above, the effect of $v$ on contributions is increasing in the share of neighbors that sympathize with the individual’s own party. The model also illustrates that this prediction is robust to the introduction of homophily (i.e., a higher likelihood of interacting with more like-minded neighbors) and unaffected by equilibrium effects. Because of the tendency to increase contributions of the majority party and decrease contributions to the minority party, these effects are closely related to the so-called conformity effects (Bernheim, 1994).

### 3.2 Experimental Design

The purpose of the Awareness letters was to generate exogenous individual-level variation in $v$ (i.e., the belief that the recipient’s contributions could be observed by others) while holding everything else constant.

Our post-election survey provides some insights about contributors’ knowledge of the FEC disclosure policy that are valuable for understanding the experimental design. Figure 2.a shows that, among subjects in the control group, a large majority (86%) of respondents agreed with the statement that contribution records were a matter of public information, while the remaining 14% reported that they believed that the contribution records were confidential. Moreover, when asked about how confident they were about their response,

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21The irrelevance of equilibrium effects arise from the assumption that contributors are a relatively small share of the population. For example, Opensecrets.org estimates that only about 0.53% of the U.S. adult population gave $200 or more in campaign contributions during the last election cycle.

22The exact statement was: “The law says that the identity of contributors to political campaigns, the amounts contributed and the identity of recipients are all public information, and that information can be easily obtained by anyone with Internet access.” Strictly speaking, some contribution records are a matter of public record while others are not (e.g., records for contributions below $200 are not reported to the FEC). We chose a very general phrasing for the statements in the survey since we wanted to elicit a general awareness about the publicity of this information rather than specific knowledge about campaign finance regulations. Since the survey recipients made contributions over $200, we expected them to agree with the statement that contributions are a matter of public records.
only 11% of those who said that contribution information was public said that they were unsure about their answers. Survey results indicate that knowledge about the disclosure policy is even higher than knowledge about contribution limits, another important aspect of campaign finance regulations.\textsuperscript{23} Even though at the time of the experiment we had no survey evidence, we expected such high awareness about the publicity of contribution records given that campaign committees have to request the information required by the FEC and so they have to explain to the contributors why what they are requesting that information for.

The fact that a vast majority of contributors already knew about the public nature of contribution records implied that sending a contributor information about the FEC disclosure policy should have had a very limited effect on subsequent contributions. Results from our post-election survey suggest that, however, there was significant potential in informing the contributor’s neighbors about the publicity of contribution: Figure 2.b indicates that 40% of respondents considered that the majority or the vast majority of their neighbors believed that the contribution records were confidential. This probably reflects the fact that contributors believe that only some of the neighbors are contributors and that non-contributors are not very aware of the public nature of contribution records.

We tested the presence of signaling effects by comparing the impact of two variations of the Awareness letter: Awareness-Own and Awareness-Neighbours, samples of which are shown in Appendices A.1 and A.2. The two types of letter differed in one crucial dimension, but were identical in all other respects. Both included a table with a list of the individual and five of her neighbors who contributed between April 1, 2011 and April 1, 2012, with the party and the amount contributed by each of those listed. The five neighbors shown in the list correspond to individuals that are geographically close to the recipient.\textsuperscript{24} The recipient of the letter was always the second contributor in the list, highlighted and identified by full name - other contributors were only identified by their last name initial and their first name. We included this short list of contributors and contributions as a way to draw the recipient’s attention to the content of the letter, but also, by providing verifiable information (including their own contributions to date), the list should have reinforced the perception that individual contributions were public. The second paragraph of the letter identified the FEC as the data source and indicated that the name, the address and the details about campaign contributions were freely accessible online, along with a link to the FEC’s database search engine and an indication that one could use this website “to see which candidates or political parties your neighbors, friends, family and co-workers are contributing to.”

\textsuperscript{23}When asked about contribution limits, 70% of respondents selected the correct amount ($2,500) from a list, while 20% responded selected the Don’t Know option (see Figure F.1 in the Appendix).

\textsuperscript{24}The median pairwise distance between the recipients and those five neighbors was 0.35 miles.
The only difference between Awareness-Own and Awareness-Neighbors was in the message prominently displayed in a box located right below the list of contributors, which read:

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

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

The two sub-treatments thus differed in the information provided to the recipient about whether other households in their area would receive a similar letter or not. Moreover, this information was non-deceptive: we sent only one Awareness-Own letter and multiple Awareness-Neighbors letters in the corresponding geographic areas.\(^{25}\) Note that households were explicitly told that they were chosen at random, so that they would not make any inferences from being chosen to receive the letter.

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

### 3.3 Experimental Results

As discussed above, the effect of increasing the visibility of an individual’s contributions should depend on the political composition of the individual’s reference group, comprised by other people that the individual is likely to interact with, including but not limited to family, friends, neighbors and co-workers. While ideally we would use information on social interactions to define the individual’s reference group, because of obvious data limitations

\(^{25}\)Specifically, we divided the United States into disjointed geographical areas of similar population based on agglomerations of 9-digit ZIP codes. These areas were randomly assigned to one of two groups. In areas assigned to the Awareness-Own treatment, exactly one household (randomly chosen among all household in the area) was sent a letter of this type. In the areas assigned to the Awareness-Neighbors treatment, we randomly selected two households to be sent these letters. We assigned exactly twice as many areas to the Awareness-Own than to the Awareness-Neighbors type so that the expected number of households receiving each sub-treatment was the same.
we had to rely on geographic proxy. This is one of the most widely-used approaches in the literature.\textsuperscript{26} We defined groups by the individual’s 3-digit ZIP code (ZIP-3) of residence. The political composition of each ZIP-3 was computed as the share of contributors to the individual’s party over the three previous presidential campaigns. This same measure of reference groups is held constant throughout all the experimental and quasi-experimental results. We also discuss robustness checks using alternative definitions.\textsuperscript{27}

The main specification is a regression of individual post-treatment contributions on a treatment dummy. Since we expect heterogeneous effects, we include the share of own-party contributors in the ZIP-3 and the interaction between this variable and the treatment dummy. The panels in Figure 3 present the results from different specifications in graphical form. Figure 3.a shows the differential effect between Awareness-Neighbors and Awareness-Own on the amount contributed post-treatment, for different values of the share of own-party contributors in the ZIP-3. As previously mentioned, we interpret the difference between Awareness-Neighbors and Awareness-Own as the effect of increasing the visibility of the recipient’s contributions (i.e., the signaling effect). Consistent with the prediction from the model of party signaling, the signaling effect is increasing in the share of own-party individuals in the ZIP-3. It has positive and significant effects when the majority of neighbors support the individual’s own party, and negative effects when the majority of neighbors support the opposite party. Figure 3.a suggests that the signaling effect reduced post-treatment contributions by about $65 in areas where the individual’s own party represented only 20% of contributors, while the signaling effect increased contributions $25 in areas where the own party represented 80% of contributors. These effects are not only statistically significant but also economically significant relative to the average post-treatment contributions ($589), specially if we take into account that these are just intention to treat effects.

Figure 3.b shows that the results are similar if, instead of using the amount contributed as the outcome of interest, we use a dummy variable that takes the value 100 if the individual made at least one contribution in the post-treatment period (i.e., the extensive margin). Note from Figures 3.a and 3.b that the point at which the signaling effects become zero is approximately where the share of own-party neighbors is 0.5. According to the model in Appendix H, this is consistent with a model of party-signaling where individuals are equally likely to interact with individuals of the own or of the opposite party, and where they value those interactions symmetrically.\textsuperscript{28}

\textsuperscript{26}For a discussion see for example Perez-Truglia (2013).
\textsuperscript{27}The advantage of using contribution data instead of electoral results is that we have the exact location for each contributor, which allows a finer measure of party composition. Electoral results are only available at the county level. In any case, our party composition variable, based on share of contributors, and an alternative, based on electoral results, have a very high correlation of 0.88 at the county level.
\textsuperscript{28}However, it is possible that there is an asymmetry in party-signaling effect that is compounded with other
Figure 3 also presents two straightforward falsification tests. Figure 3.e shows the effects of the Placebo letter on the post-treatment amount contributed, with the axis defined as above. As expected, the Placebo mailing did not have a significant effect on contributions. Figure 3.f depicts the signaling effects (i.e., Awareness-Neighbors - Awareness-Own) using pre-treatment contributions (instead of post-treatment contributions) as the dependent variable. As expected, there is no significant effect on this variable.

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

Figure 3.c depicts the difference in post-treatment contributions between the Awareness-Neighbors treatment and Control, while Figure 3.d shows the differences between Awareness-Own and Control. The comparison between these two figures illustrates the very different reactions induced by the Awareness sub-treatments. While Awareness-Neighbors decreased contributions for individuals surrounded by a majority of neighbors of the opposite party, Awareness-Own increased contributions for individuals in the same situation. Given that a vast majority of contributors already knew about the disclosure policy, we expected that the Awareness-Own was not going to induce significant signaling effects. If any, the signaling effects were overridden by other mechanisms that operated in the opposite direction. Most likely, these effects are due to some other information contained in the Awareness-Own letter (e.g., the contribution records shown in the table). Indeed, the findings on informational effects, discussed in the next section, provide a compelling interpretation for the effects of the Awareness-Own letter.

Table 4, in turn, presents additional robustness checks. Column (1) presents the results for the baseline specification used in Figure 3.a. The coefficient on Awareness-Neighbors - Awareness-Own, -$91.2, corresponds to the intercept in Figure 3.a: i.e., the signaling effect types of signaling effects and, thus, is not captured. For example, if contributions constitute signals of other desirable individual characteristics beyond party affiliation (e.g., being wealthy, or having more pro-social attitudes), this would imply that the true signaling effects are more negative for every level of the share of own-party individuals.
when the share of the recipient’s party is 0. The coefficient of the interaction of this variable with the share of own-party individuals, $146.9, corresponds to the difference between the two extremes of the signaling effects (i.e., when the share of own-party individuals is 1 and 0). A comparison between the results in columns (1) and (2) indicates that the inclusion of control variables has virtually no effect on the estimates. Columns (5) and (6) confirm the falsification tests depicted in Figures 3.e and 3.f: we reject the null hypotheses that the coefficients on column (1) are the same as those in columns (5), corresponding to the Placebo letter, or (6), corresponding to pre-treatment contributions.

In column (3), besides the interaction with the share of own-party individuals in the ZIP-3, we add the interaction with the share of own-party individuals in the ring of ZIP-3s that are adjacent to the recipient’s ZIP-3 of residence. In column (4) we add an interaction with the share of low-income households in the ZIP-3. The coefficients for the main signaling effects in columns (3) and (4) are practically identical to those in column (1), which indicates that the results are robust. Moreover, the coefficients on the additional interactions are close to zero and statistically insignificant - however, they must be interpreted with care because they are imprecisely estimated. We conducted a number of unreported robustness checks, such as using alternative definitions for the share of own-party individuals in the area, and always find similar results.

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

Column (2) reports the signaling effects with the probability of post-treatment contributions to all other types of campaigns (excluding presidential campaigns) as the dependent variable. The point estimates are very similar in magnitude to the estimates for the presidential campaigns, but the coefficients are not statistically significant. This lack of statistical significance is related to the fact that the baseline level of this variable is substantially lower, at about 11%, compared to 49% for the presidential campaign contributions, so that there is less statistical power to identify the effects on this alternative outcome. In any case, we cannot reject the null that the respective coefficients in columns (1) and (2) are equal. This is not
surprising, since contributions to a non-presidential campaign should signal party affiliation to a similar extent than contributions to presidential campaigns.

Columns (3) and (4) from Table 5 present the signaling effects on post-treatment contributions in two periods - before September 1, 2012 (about four months after our treatment), and from September 1 to December 31, 2012, the last four months of the post-treatment period. The post-treatment contributions are roughly equally distributed for these two time periods. The evidence suggests that the mailings elicited signaling effects mainly during the first four months after the mailings were sent. This finding may reflect that recipients, after some months, forgot that we sent mailings to their neighbors. However, while the effects on the second half of the post-treatment period were smaller than in the first half, we cannot reject the null hypothesis that there were non-zero but smaller effects during that period.

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

3.4 Scaling-Up the Intention to Treat Effects with Data from the Post-Election Mail-In Survey

As discussed in Section 2.4, a substantial share of experimental subjects - most likely a majority - may not have read the letters we sent to them. Some individuals may have not opened the letters, and even some who opened the letters may have discarded them before reading. Our estimates thus measure the intention to treat effects of sending these letters. The treatment effect on the individuals who read the letter is a multiple of this intention to treat effect. The unknown parameter is the reading rate, \( r \) (i.e., the proportion of the experimental subjects who read the letters). The intention to treat effects (ITT) can be scaled up into treatment effects on the treated (TOT) using the inverse of the reading rate: \( TOT = \frac{1}{r} ITT \). For instance, if we assumed that \( r \) was equal to the response rate from the
post-election mail-in survey, 21.21%, we should scale-up the TOT by a factor of 4.71.\textsuperscript{29}

One of the goals of sending the post-election survey was to obtain an estimate of $r$. The main information contained in the Awareness letters was that contribution records were public. This was explicitly stated in the letters and it was also demonstrated by some pieces of evidence in the mailing, such as the tables with contribution records for a sample from the recipient’s neighborhood (including the recipient) and the link to the FEC’s online search tool. We can expect that individuals who thought that contribution records were confidential updated their beliefs in the direction of considering them to be public. Thus, $r$ can be inferred from the impact of the Awareness treatments on the recipients’ knowledge about the disclosure policy for campaign contributions.

Figure 4 presents the distribution of beliefs about the publicity of contribution data for survey respondents from the Control and the Awareness groups. As expected, subjects in the Awareness group were more likely to report that the contribution records are public information. 25.6% of respondents from the Control group stated in the mail-in survey that contribution records were not public (i.e., they did not respond to be “Somewhat sure” or “Very sure” that contribution information was public). This share is 21.2% for respondents from the Awareness group, i.e. 17.1% lower.\textsuperscript{30} We assume that, if a contributor did not know that information about contributions were public were to read the letter, she would go on to report that contribution records were public in the mail-in survey after receiving the Awareness letter. Then, the effect of the Awareness letter on the knowledge of disclosure policy implies a reading rate of $r = 0.171$, with a 90% confidence interval ranging from 0.117 to 0.225. This result suggest that the treatment effects on the treated are about 5.8 times the intention to treat effects reported above.\textsuperscript{31} For instance, the intention to treat signaling effect was 4.6% on the extensive margin for a share of own-party individuals of 80%. The scale-up factor of 5.8 implies that the corresponding treatment effect on the treated was 26.7%. While large, this effect size is perfectly plausible.

Interestingly, the 90% confidence interval for this reading rate estimate includes the value of the naive reading rate estimate based on the mail-in survey response rate. This 90% confidence interval also includes our prior belief of the reading rate, $r = 0.15$, which we used for the power calculations while designing the experiment. However, there may be some caveats with this simple reading rate estimate. A primary concern is that individuals who

\textsuperscript{29}It should be noted that the mail-in survey was sent in a closed envelope, whereas the experimental mailings consisted of single sheets of paper that folded and sealed to make letter-sized mailpieces. In this sense, the mail-in survey was less likely to be discarded as junk mail than the experimental mailings.

\textsuperscript{30}In unreported results, we find this difference to be higher in ZIP-3s with a higher share of own-party, although this heterogeneity is not statistically significant.

\textsuperscript{31}The corresponding 90% confidence interval ranges from 4 to 7.6.
are more likely to read the letter may have also been more or less likely to respond to the mail-in survey. For instance, the Awareness letter may have induced a lower willingness to participate in the mail-in survey, which would lead to an under-estimation of the reading rate. There is a simple way of testing this possibility: if individuals who read the letter were more (conversely less) likely to respond to the survey, then we should observe that individuals who were sent the Awareness letter were more (conversely less) likely to respond to the survey relative to individuals who were not sent the Awareness letter.\textsuperscript{32} The evidence strongly rejects this hypothesis: the response rate to the survey was 21.0\% for subjects in the control group and 21.3\% for subjects in the Awareness group, and this 0.37 percentage points difference is not only very small but also statistically insignificant (p-value of 0.357).\textsuperscript{33} Alternatively, some individuals may be more likely to read the letter and respond to the survey than others (e.g., they have more free time). However, this could only lead to an over-estimation of the reading rate.

A second concern about this reading rate estimate is that it is based on the assumption that 100\% of the individuals who did not know that the contribution records were public and read the Awareness letter reported in the mail-in survey that the records were public. However, it is possible that some recipients who read the letter were still not induced to report that the contribution data is public, which would imply that the above estimate of $r = 0.171$ is below the actual reading rate. More precisely, some individuals may still report that the contribution data is not public because they believe that SuperPACs can be used to make confidential contributions, or because information about contributions is not public for amounts below $200$.\textsuperscript{34} Consistent with this intuition, Figure 4 suggests that the Awareness letter did not have a significant effect on the share of respondents who reported to be very sure or somewhat sure that the contribution information was confidential. In order to deal with this source of bias, we can use the alternative assumption that a contributor who was unsure about the publicity of contribution records and who read the Awareness letter should not report to be unsure in the mail-in survey. While 15.8\% of the individuals in the Control group reported to be unsure about the publicity of the data, this proportion was 19.2\% for those in the Awareness group (21.5\% higher). This evidence implies a reading rate of $r = 0.215$ (with a 90\% confidence interval between 0.146 and 0.284), and a scale-up factor of

\textsuperscript{32}Those who were sent a letter but did not read it can be expected to behave like those who were not sent any letter.

\textsuperscript{33}A further concern is that the reading rate is estimated from the mail-in survey sample, which is not representative of the subject pool (e.g., it over-represents Democrats). This reading rate may thus not be representative of the reading rate in the subject pool. In any case, we can still use the $r = 0.171$ to scale-up the intention to treat effects computed with the restricted sample of survey respondents.

\textsuperscript{34}It is also possible, although unlikely, that other recipients read the letter but forgot the information contained in the letter at the time of responding to the survey several months later.
4.6. This is lower than the 5.8 estimated above but still substantial in magnitude.

### 3.5 Discussion

Our results indicate that increasing the visibility of an individual’s contribution has a significant effect on her contribution, increasing her contribution if she shares the same party affiliation than most individuals in her area of residence, but decreasing her contribution if a majority of individuals belong to the opposite party. While our study examines campaign contributions, the evidence suggests that similar signaling effects could operate with respect to other forms of observable political participation including openly discussing politics or political issues, or even running for office.

Even though contributions may be observable in other contexts (e.g., fundraising events), the FEC disclosure policy may be the major source of contributions’ visibility. This policy is unique in the sense that a similar data source does not exist for other forms of contributions (e.g., charitable giving) or even other pro-social behavior. This disclosure policy may affect contributions through signaling but also through other mechanisms, like the informational effects discussed in the following section. As suggestive evidence about the relative importance of the signaling channel, Figure 5.a shows the correlation between the amount contributed (from the FEC records) and the belief that neighbors know about the publicity of contribution data (from the survey data) for a different values of the share of own-party individuals in the ZIP-3. The reported correlation is significant and, most importantly, it is consistent with the direction of the signaling effects. This observational correlation and the experimental results on signaling effects suggest that the FEC disclosure policy may have a first-order effect on actual campaign contributions in the U.S.

### 4 Experimental Results: Informational Effects

#### 4.1 Relevant Hypotheses

In this section, we study how observing the contribution behavior of others, rather than being observed by others, affects the individual’s willingness to contribute, which we denominate informational effects. There is evidence from the literature on charitable contributions that an individual’s contributions change when the individual is provided with information about others’ contributions. For example, Frey and Meier (2004) implemented a field experiment in which they sent a randomly-assigned sample of potential donors to a university’s social fund information about past contributions from fellow students. Those who were told that a high percentage of students had contributed in the past gave more than those who were told that
only a small fraction contributed.\textsuperscript{35} One typical interpretation for this type of findings is that individuals make contributions following a social norm about the right amount that one should contribute, and that, in turn, individuals form beliefs about that social norm based on the observation of contributions made by others in the same reference group.\textsuperscript{36}

In the context of campaign contributions, the contribution norm could depend on contribution behavior by individuals of the same and opposite party. An individual’s desired contributions should increase according to the perceived average amount contributed to her own party. It may also increase, to a lower extent, according to the perceived average amount contributed to her opposite party, if the individual considers that - beyond partisan considerations- campaign contributions are socially desirable. With respect to the total number of contributors, the social norm predicts that - if anything - observing a higher number of contributors to one’s own party (relative to the opposite party) should increase the individual’s own likelihood of making a contribution.

Besides social norms, we consider two additional informational mechanisms. The investment motive refers to the notion that an individual contributes - at least partly - because she perceives that, with some probability, her marginal contribution will change the election outcome (Ansolabehere et al., 2003). This intuition is similar to the idea of being a pivotal voter in a model of voter turnout (e.g., Dhillon and Peralya, 2002). This investment motive is subject to the typical criticism that a rational individual should expect the marginal effect of a single contribution to be virtually zero. This argument is particularly strong in the context of small contributions to a presidential campaign, where the average individual contribution of a few hundred dollars is infinitesimal when compared to the several hundreds of millions of dollars raised by each candidate. If contributions really responded to an investment motive, then the incentives to contribute should be higher when campaign contributions are more even (e.g., both parties raised similar amounts).

Last, an individual may contribute to a candidate because of altruistic concerns. For example, the contributor may experience a feeling of warm glow when making the contribution (Andreoni, 1989). Alternatively, the contributor may feel good about the services that his marginal contribution is financing (e.g., advertising the party’s ideals). In both cases, the perceived contributions by others may change the marginal benefit from contributing an extra dollar. First, the marginal contribution may provide a stronger feeling of warm

\textsuperscript{35}Similar findings were reported by Martin and Randal (2008) with contributions to an art gallery and by Allcott (2011) in the context of energy saving measures.

\textsuperscript{36}Another interpretation is that charitable donations by others may signal the quality of the charitable organization (Karlan and List, 2012). A priori, this explanation is not likely to apply to the case of presidential campaign contributions. Most contributors have beliefs about the candidates, and while these may be either biased or unbiased, they are probably unlikely to be significantly affected by the observation of the contributions made by others.
glow if the contributor perceives that her favorite candidate is doing worse than the opposite candidate. Second, the individual may perceive that there are decreasing marginal returns to campaign spending. Both cases make a similar prediction: the attractiveness of making a contribution should be decreasing in the total amount contributed to one’s favorite candidate and increasing in the total amount contributed to one’s opposite candidate. In order to distinguish this mechanisms from the traditional free-riding in public goods, we denominate these mechanisms moral free-riding.

To the best of our knowledge, the only existing experimental evidence related to informational effects in campaign contributions was provided by Augenblick and Cunha (2011). They compared contributions by potential Democratic donors who received a postcard from the Democratic candidate with a picture of the candidate and a message. They find that the messages “Small Republican contributions have been averaging $28” and “Small Democratic contributions have been averaging $28” elicited higher contributions than a message that did not mention others’ contributions, which may be consistent with the presence of a contribution norm. They also find that the first message increased contributions more than the second, which they interpret as a competition effect.\footnote{Our experimental setting differs from Augenblick and Cunha’s (2011) in several ways. The first and most important difference is that our experiment allows us to disentangle the effects of several simultaneous competing mechanisms. A second difference is that we look at contributions to both campaigns, not just one of the parties. Finally, the source of the information we provided to our subjects was explicitly non-partisan.}

Even though their evidence is not inconsistent with our evidence, our findings shed light on different informational mechanisms.

### 4.2 Experimental Design

The mechanisms discussed in the previous subsection highlighted different dimensions of others’ contribution that may be important for an individual’s own willingness to contribute. For example, social norms predict that contributions should be increasing in the perceived average amount contributed to the individual’s own party, and perhaps to the opposite party. The List letter was designed to generate exogenous variation in the recipient’s perceptions of these various dimensions of the contribution patterns by other individuals in the area, and then measure the effects of that information on the recipient’s subsequent contributions. We then compare these effects with the predictions of each potential informational mechanism discussed (e.g., social norms, moral free-riding) to verify if the evidence is consistent with one or more of these mechanisms.

A sample of the List letter is presented in Appendix A.3.\footnote{Appendix A includes samples for two sub-treatments, List-Once (A.3) and List-Update (A.4). The difference between the two is discussed at the end of this subsection.} This type of letter presented a list of presidential campaign contributions made by the recipient and nine other individuals.
from the recipient’s area during the period April 1, 2011 to April 1, 2012, with the party
and the amount contributed by each of those listed. A key difference between the Awareness
letter and the List letter is that in the latter there was no mention of the FEC’s online search
tool. The recipient’s own contribution and her full name were included (and highlighted)
at the top of the list. This was meant to draw the recipient’s attention and to highlight
the credibility of the study, given that the individual was probably aware of her own past
contributions, and could recognize the information to be true. As in the Awareness letters,
other contributors were only identified by their last name initial and their first name. The
letter listed contributions to Democrats first and to Republicans next, with each entry within
these groups ordered from highest to lowest amount. This sorting was simply meant to make
it easier to read the information on the table.

The main informational treatment in the List letter was the list itself, which contained
subtle random variations in the information included: some had more Republican contrib-
utors, some had higher amounts for Democrats, and so on.39 We devised an algorithm to
select the other contributors that would be displayed in the recipients’ letters in a way that
allowed us to induce non-deceptive variation. We first obtained the geo-location for all indi-
viduals listed in our baseline FEC database. Then, for each individual i in the List treatment
group, we identified the 30 closest contributors, \( L_i \), which according to our definition are the
individual’s neighbors.40 Each list of ten contributors in the letters included the recipient
and a subset of nine neighbors from \( L_i \). These nine neighbors were selected by first ordering
the list of 30 neighbors according to a composite index, and then selecting the top nine con-
tributors from the ordered list. The value of the composite index for a given neighbor \( j \) was
a function of \( j \)’s party, \( \text{Party}(j) \), and of the amount contributed by \( j \) during the previous
twelve-month period, \( \text{Amount}(j) \):

\[
\text{Index}_i(j) = \theta_i^D \cdot 1[\text{Party}(j) = \text{DEM}] + \theta_i^A \cdot \text{Amount}(j)
\]

The parameters \( \{\theta_i^D, \theta_i^A\} \) are the recipient-specific weights assigned to each of those di-

mensions. Taking those parameters as a given, the list of the top nine neighbors is denoted
by \( g(L_i; \theta_i^D, \theta_i^A) \). The weight given to the political party component was randomly selected
from three possible values: \( \theta_i^D = -c_p, 0, c_p \), with \( c_p > 0 \). Similarly, the weight given to the

[39]The information provided by the List treatment is probably not the only information that recipients would
obtain. The letter might have prompted recipients to find out more about their neighbors’ contributions,
by visiting the FEC website (or others providing similar information) even if we did not include a link to
this search tool. Because of that, the effect of the information provided by the list would be probably
attenuated. In any case, the possibility that some individuals acquired information on their own should
induce our estimates to under-estimation the real effect of the information provided by our treatment.

[40]\( L_i \) is constructed based on pair-wise distances as the crow flies. The median pairwise distance between
the recipients and their 30 closest contributors was one mile.
contribution amount was randomly selected from three possible values: \( \theta^A_i = -c, 0, c \), with \( c > 0 \). The list of nine neighbors given by the parameter values \( \theta^D_i = 0 \) and \( \theta^A_i = 0 \), was defined as the baseline list: \( g(L_i; 0, 0) \).

This composite index induced exogenous variation in the list printed on each letter. For instance, a composite index computed with a higher weight for Democratic neighbors would move Democratic neighbors further up in the ordered list of 30 neighbors, which would result in a higher number of Democratic neighbors in the final list of nine neighbors. Table 6 presents three possible lists of nine neighbors generated by different combinations of the weights. Compared to the baseline list (left panel), when \( \theta^D_i \) takes the negative value some of the Democratic neighbors leave the list and are replaced by Republican neighbors (center panel). Alternatively, when \( \theta^A_i \) takes a positive value, some Republican contributors are replaced by other Republicans with higher contributions (the same would be the case for Democrats). We calibrated the probability distribution of the parameter values so that the experimental variation in the key dimensions of the list, the average amounts contributed, and the number of contributors to each party, were orthogonal. Most importantly, we calibrated the distribution of the parameters so that the lists were not biased on an average sense. It is important to note that the letters were not deceptive because they always contained a subset of the 30 contributors closest to the recipient, which always satisfied the promise of “10 individuals from your neighborhood.”

Just like in the estimation of signaling effects, we do not rely on comparisons between post-treatment contributions by individuals who received the List letters and those who did not receive any letters. Instead, we rely on comparisons between the contributions of individuals who received List letters, but with exogenous differences in the information contained in the list. To understand the intuition of the estimation of these effects, consider the following example: a random half of the recipients receive a letter with an average contributions of $500 and the other half receive a letter where the average contribution is $600. We could run a regression of the post-treatment contribution on a variable that takes the value 0 for the recipients shown the $500 average contribution and the value 100 for the recipients shown the $600 average contribution. If the estimated coefficient on that variable is 0.1, it would imply that each additional dollar in average contributions shown by the letter caused the recipient to contribute an additional ten cents. We must generalize this intuition for the case when we simultaneously randomize multiple dimensions of the information set. Let \( f^k(\cdot) \) represent any statistic \( k \) from a given list (e.g., the mean of contributions in the list to the recipient’s own party). We regress the recipient’s post-treatment contributions on \( \Delta f^k_i = f^k(g(L_i; \theta^D_i, \theta^A_i)) - f^k(g(L_i; 0, 0)) \). Intuitively, this is the value of the particular statistic in the list shown to the individual compared to the value that would have been shown if we
had sent the baseline list instead. Since the variation in $\Delta f^j_i$ is driven entirely by the random assignment of $\{\theta^D_i, \theta^A_i\}$, the coefficient on $\Delta f^j_i$ can be interpreted as the causal effect of the $f^j_i$ shown in the list on the recipient’s post-treatment contributions.

Last, we randomly assigned subjects to one of two variations of the List treatment: List-Once (illustrated in Appendix A.3) and List-Update (Appendix A.4). The only difference between these two variations was that in the List-Update letter we stated that an updated list with contributions by neighbors may be sent at the end of the election cycle, whereas the List-Once letters specified that a letter of this type would not be sent again in the future. The purpose of including these sub-treatments was to test if the informational effects were being confounded or, on the contrary, augmented by signaling effects. Details about this test are presented with the experimental results in the following subsection.

4.3 Experimental Results

Table 7 presents the baseline regression results. The independent variables refer to multiple dimensions of the information on the contributions of neighbors listed in the letter. Some of these dimensions include the average amount contributed to the recipient’s own party ($\bar{c}_{own}$) and to the opposite party ($\bar{c}_{opp}$), the number of contributors in the list to the recipient’s party ($N_{own}$), and the total amounts contributed by neighbors of own- and opposite- party ($\sum c_{own}$ and $\sum c_{opp}$). All independent variables that refer to amounts are expressed in hundreds of dollars. The dependent variable in columns (1) through (6) is the amount contributed post-treatment. Column (1) includes $\bar{c}_{own}$ and $\bar{c}_{opp}$ as regressors. The coefficient on $\bar{c}_{own}$ indicates that for each $100 increase in this variable there is a statistically significant increase in the recipient’s own contributions of about $2.5. This evidence is consistent with the social norms mechanism, which predicts that individuals will contribute more the higher that they perceive the average amount contributed in their reference group to be. The coefficient on $\bar{c}_{opp}$ indicates that, on the contrary, the recipient is not affected by the average contribution made by neighbors of the opposite party. This result suggests that, when forming social norms, individuals pay attention to the average contribution among supporters of their own party but not among supporters of the opposite party.

The economic significance of the estimates of the social norms effect needs to be qualified by two considerations. First, as in the case of signaling effects, these are intention to treat estimates, since we do not know which recipients actually received and read the letter, and this implies that the actual treatment effects on the treated effects are a multiple of the latter. For instance, according to the calculations from subsection 3.4, the treatment effect on the treated may be 5.8 times the intention to treat effect. This would imply that for each $100
increase in $c_{own}$ the recipients who actually read the letter increased their contributions by $14, resulting in a elasticity of 0.14 between the average amount observed and the amount contributed. Second, this elasticity of 0.14 would be a reduced form elasticity between the information provided, $c_{provided}$, and $c$, the contribution made. The structural parameter of interest is the elasticity between the perceived social norm, $c_{norm}$, and $c$. The estimated reduced form effect, $\frac{dc}{dc_{provided}}$ is the multiplication of two effects: $\frac{dc}{dc_{provided}} = \frac{dc}{dc_{norm}} \times \frac{dc_{norm}}{dc_{provided}}$. Since $\frac{dc_{norm}}{dc_{provided}}$ is most likely between 0 and 1, the structural elasticity of interest, $\frac{dc}{dc_{own}}$, is a multiple of the reduced-form elasticity reported in the table, $\frac{dc}{dc_{own}}$.

Column (2) from Table 7 presents the results from a specification that includes as regressors $c_{own}$ and $c_{opp}$ along with the number of individuals in the list who contributed to the recipient’s party, $N_{own}$. The social norm mechanism predicts that a higher $N_{own}$ could increase the recipient’s contribution, whereas the moral free-riding mechanism predicts an effect with the opposite sign. The negative and statistically significant coefficient of $N_{own}$ suggests that, even though both mechanisms may be operating simultaneously, the net effect is largely dominated by the moral free-riding effect. For each additional individual of the same party shown on the list, the recipients reduced contributions by $6.22. This effect is comparable to the effect of a decrease of about $225 in average contributions to the own-party in the list. The effects of $c_{own}$ and $N_{own}$ are quantitatively similar insofar a one standard deviation increase in $N_{own}$ has an effect on the recipient’s contribution almost equal to the effect of a one standard deviation decrease in $c_{own}$.

The discussion of the moral free-riding mechanism suggested that contributions could be a function of the total amounts contributed to each party. Instead of including $N_{own}$, the specification reported in column (3) includes as regressors the sum of contributions to the recipient’s own and opposite parties, $\sum c_{own}$ and $\sum c_{opp}$. Consistent with the moral free-riding effect, the recipient’s contribution increases with the total amount raised by the opposite-party and decreases with the total amount raised by the own-party. We cannot reject the hypothesis that the absolute value of the two coefficients are equal. While each of the two coefficients is not statistically significant on its own, the coefficient of the difference between the two, reported in column (4), is statistically significant at the 5% level.

The specifications reported in columns (5) and (6) from Table 7, in turn, test the predictions of the investment motive. According to this theory, individuals should be most willing to contribute when the total amounts contributed to each party are close to each other. The specification in column (5) introduces as a regressor the absolute value of the difference between the total amounts contributed to the recipient’s own and opposite-party. This variable

\[^{41}\text{It should be noted that in this specification, the marginal effect of } c_{own} \text{ is the composition of the direct effect through } c_{own} \text{ and the indirect effect through } \sum c_{own}.\]
does not have a significant effect on recipient’s post-treatment contributions. A potential issue with this specification, however, is that individuals may react to the total contributions to each party not because they care about those values at a local level, but because they use those local values to extrapolate them to the national level.\textsuperscript{42} If that was the case, then individuals should react to that information differently depending on where they are located. For instance, if a recipient was expecting the Democratic party to win by a large margin in his local area, showing him that the contribution race is even in this area should make him infer that the Republican party is winning by a large margin at the national level. Column (6) uses an alternative definition of the evenness of the campaign that accounts for this issue. Intuitively, this variable takes the value zero when the race in the list shown to the recipient is equally even with respect to the value in the baseline list (which proxies for the individual prior belief). Once again, the investment motive captured by this indicator does not have a statistically significant effect on subsequent campaign contributions. This result is robust to a number of alternative specifications not reported.

Column (7) from Table 7 reports the results of the specification in (2) with an indicator that takes the value 100 if the recipient made at least one post-treatment contribution as the dependent variable. The coefficients from column (7) have the same sign as the corresponding coefficients from column (2), but individually each of those coefficients are statistically insignificant. This result may imply that informational effects are relevant for the intensive margin but not for the extensive margin. However, the absence of statistically significant effects could be at least partly due to the fact that there is more variation in amounts contributed than in the extensive margin alone, so the results for the extensive margin are less precisely estimated. Column (8), in turn, presents the results of the usual falsification test, where the dependent variable is the amount contributed pre-treatment. As expected, all of the coefficients are close to zero and none of them are statistically significant.

Table 8 presents results using alternative outcome variables and allowing for group-level heterogeneity. We report results for the specification with regressors $c_{own}$, $c_{opp}$ and $\sum c_{own} - \sum c_{opp}$.\textsuperscript{43} Columns (1) and (2) from Table 8 compare the informational effects by contribution type: the outcomes in column (1) correspond to presidential contributions, while the outcome in column (2) corresponds to the amount contributed to other committees (e.g., campaigns for state governor). Since the baseline probability is much lower for the latter outcome, the coefficients are very imprecisely estimated, and we cannot draw any meaningful conclusions from the comparison between columns (1) and (2). Columns (3) and

\textsuperscript{42}For evidence on how people extrapolate from local information see for example Cruces et al. (2013).

\textsuperscript{43}The specification, reproduced from column (4) in Table 7, is used as baseline because it is the specification that seems to fit the data best. However, the results are similar with alternative specifications.
(4) split the contributions during the post-treatment period in two halves: before and after September 1, 2012. The effect of $\bar{c}_{own}$ is similar for the contribution made during the two periods, implying that our letter had a lasting effect on the contribution norm. The moral free-riding effect, on the contrary, is only present for contributions made during the first half of the post-treatment period. One potential explanation for this finding is that, as the election neared, the recipients obtained new information about the total contributions to the two presidential campaigns that replaced the information provided by our letter. Information about the total contributions raised by both campaigns was frequently reported on and discussed in the media during the election cycle.

Columns (5) through (10) from Table 8 present heterogeneous effects, captured by the interaction of the variables of interest with a group dummy (e.g., whether the contributor is Democratic). As discussed previously for the signaling effects, the available statistical power implies that we could only reject that the effects are statistically the same if we find a large difference between groups. Columns (7) and (8) show that we cannot reject the null hypothesis that the coefficients are jointly the same for individuals of the two parties. Columns (9) and (10), in turn, show that we cannot reject the null hypothesis that the coefficients are the same for females and males.

The last two columns from Table 8 present a test of whether the informational effects are contaminated by the type of signaling effects discussed in the previous section. The moral free-riding effect suggests that a recipient’s contribution is decreasing in the number of own-party neighbors displayed in the list. This effect could be confounded by signaling effects, because the recipient may be learning about the political composition of her neighborhood from inspecting the number of own-party neighbors in the list. However, signaling effects would predict that the recipient’s contribution should be increasing in the number of own-party neighbors in the list. If anything, the moral-free riding effects are under-estimated because of the presence of signaling effects. Indeed, one of the goals of not including information about the FEC search tool and semi-anonymizing the records shown in the List letter was to minimize the effects of this letter on the perceived visibility of the recipient’s contributions. However, we cannot guarantee that these measures had the intended effect. We included two variations of the List letter, List-Once and List-Update, in order to test whether the informational effects were confounded with signaling effects. The difference between List-Once (sample in Appendix A.3) and List-Update (Appendix A.4) is that in the latter we specified that we may send more letters at the end of the election cycle with updated information. If the List letter had any effect on the perceived visibility of the recipient’s

\footnote{To reinforce the message that an updated list of neighbors’ contributions would be sent at a later point in time, the table in the List-Update variation included two columns – one labeled “Past contributions: April}
contribution, that effect should have been higher in the List-Update sub-treatment. Columns (5) and (6) in Table 8 explore whether the information contained in the list had different effects in the List-Once and List-Update sub-treatments. The difference between the coefficients in the two sub-treatments is not statistically significant, which constitutes evidence that the informational effects were not confounded with signaling effects.45

We can also measure the differences in post-treatment contributions for individuals who received a List letter and individuals who did not. Table 9 compares the average post-treatment amounts contributed in the Control, Placebo and List treatments. Column (1) shows that, compared to the Control group, sending an individual a Placebo letter did not induce a significant change in post-treatment contributions. On the contrary, sending an individual a List letter did increase contributions by about $17, which is equivalent to about 3% of the average amount contributed post-treatment - and this is just the intention to treat effect. Column (5) shows that the letters did not have a significant effect on the extensive margin, which is consistent with the lack of informational effects on the extensive margin. Column (6) presents the result from a further falsification test, showing that the pre-treatment contributions are statistically indistinguishable among the Control, List and Placebo treatment groups. Columns (2) to (4) present specifications where the treatment dummies are interacted with some pre-treatment characteristics. Column (2) shows that the effect of the List treatment was statistically indistinguishable between the List-Once and List-Update sub-treatments. And column (3) shows that there are no statistically significant differences between the informational effects for Republican and Democratic contributors.

The last column of Table 9 provides some suggestive evidence about the reason why the List letter had a positive average effect on subsequent contributions. Given the evidence on the informational effects, one compelling hypothesis is that the unbiased information contained in the List letter corrected the systematic biases in the recipients’ belief about the contributions of others.46 For example, if everyone had the same prior belief about what others are contributing, recipients from areas with higher average contributions should have a more positive reaction to the List letter. Following this intuition, column (4) presents the effect of the interaction of the List treatment with some characteristics of the contribution

1, 2011 to April 1, 2012, with past contributions as in List-Once, and a second column with the same format but with empty cells for each individual in the list and with the header “Future contributions to be reported: May 1, 2012 to December 1, 2012”.

45It is possible, however, that the informational effects are stronger due to visibility of contributions: e.g., while the recipient may dislike making a lower contribution as compared to others, even if that contribution was confidential, that disutility may be even higher if she perceives that others might use the FEC search tool to learn about this behavior.

46Alternatively, our mailing may have made it clear to recipients that researchers were studying campaign contributions, which may have increased their perception of the importance of these contributions and thus indirectly affected their behavior.
patterns in the individual’s area, as measured in the baseline lists. If the List letter corrected systematic biases in perceptions about others’ contributions, we should expect more positive effects in areas with higher average contributions by own-party neighbors (due to social norms), and more positive effects in areas where the total contributions to the own-party are surpassed by the total contributions to the opposite-party (due to moral free-riding). Indeed, the coefficients on the interaction terms reported in column (4) from Table 9 provide evidence supporting this hypothesis.\textsuperscript{47}

4.4 Discussion

The evidence from this section suggests that individuals take into account contributions by others in their area when deciding about their own contributions. Individuals contribute more if neighbors from the same party contribute more, which is consistent with the existence of a social norm. Indeed, the results from the post-election survey provide some suggestive evidence about the importance of social norms as drivers of campaign contributions. Figure 5.b shows the relationship between an individual’s actual contribution (from the FEC records) and her survey response to a subjective question about how much an average-income individual should contribute to a presidential campaign. The strong positive relationship suggests that social norms are an important factor for understanding differences in contribution behavior. Besides social norms, we present evidence that individuals contributed less when they perceived that their party was raising more money than the opposite party, which we interpreted as a moral free-riding effect. One of the remarkable features of these findings is that our informational treatments did not provide descriptive statistics about contributions, such as if he had sent a letter with a message like “the average contribution in your neighborhood was $500.” Instead, we provided itemized information about individual contribution records, which the individuals were free to analyze in whatever way they wished. These findings are thus not the result of the experimenter prompting the individual to look at certain information, which is a common criticism waged against experiments with information provision.

These findings may be useful for fundraisers for political campaigns, charitable giving and other pro-social activities, such as environmentally-responsible behavior. Campaign managers disseminate all types of information in an effort to increase contributions. For example, in the case of our mailing campaign, the average effect of the List treatment (about $17) is an order of magnitude higher than the cost of sending each letter (about $0.25). Our evidence, however, suggests that fundraisers should be careful when disseminating in-

\textsuperscript{47}Note that moral free-riding decreases contributions for individuals in areas with a higher share of own-party contributors, which can explain the effects of the Awareness-Own letter in the previous subsection.
formation, because their efforts may be self-defeating. The experimental results suggest that disseminating information about the average amount contributed can increase contributions through the social norms effect. However, disseminating information about the total amount raised may decrease further contributions through the moral free-riding effect. Further research is needed to establish whether moral free-riding effects of a similar magnitude exist in other forms of contributions, such as charitable giving.

Beyond fundraising, this evidence applies to a more general question about the consequences of information disclosure by third-parties, such as governmental or non-governmental organizations. In the case of campaign contributions, the FEC makes an active effort to disseminate aggregate and individual level information about contributions through its website. Our experimental findings indicate that individuals who received unbiased information about others’ contributions were significantly more likely to contribute. Even though the evidence is just suggestive, one plausible explanation is that the List letters increased contributions because they corrected systematic biases in the beliefs about the contribution behavior of others.

5 Quasi-Experimental Evidence on Social Incentives in Campaign Contributions

5.1 Motivation

The experimental evidence presented in the previous sections illustrated and quantified a variety of mechanisms through which social incentives operate. These mechanisms implied that individuals’ contributions were affected by the political affiliation and contribution behavior by others in the same reference group. Furthermore, some of the mechanisms operated in opposite directions. For example, the signaling channel predicts that an increase in the visibility of contributions will increase the contributions of supporters of the majority party while the moral free-riding effect generates the opposite effect. It is difficult to use the experimental evidence to establish the net effect of these social mechanisms. First, it is difficult to assess the relative importance of the experimental findings outside of the experimental setting. For example, the moral free-riding effect may irrelevant in practice if people rarely see information about the total amounts raised by the candidates. Second, there may be other social mechanisms at play that our experiment was not designed to identify. As a complement to the experimental evidence, in this section we present a quasi-experimental analysis of how individual contributions are affected by the political composition of their area of residence, which we refer to as reference group effects. Even though we cannot disentangle the relative
contribution of different social mechanisms to these aggregate reference group effects, the 
estimates of these effects provide suggestive evidence about the direction and magnitude of 
social incentives.

5.2 Quasi-Experimental Estimates of Reference Group Effects

5.2.1 Identification Strategy

Individual contributions are highly correlated to those of others from the same reference 
group, such as other individuals from the same area of residence (Cho et al., 2006). There 
are three mechanisms that may explain this intra-cluster correlation: direct selection, indi-
rect selection and reference group effects. Direct selection refers to an individual’s preference 
to live in areas with a higher share of residents identified with her own political preference 
(Cho et al., 2002). Indirect selection refers to the tendency of individuals to live near others 
with similar characteristics, which are in turn correlated to political preferences. For exam-
ple, highly educated individuals tend to concentrate geographically, and, on average, highly 
educated individuals are more likely to be Democrats. This indirect selection induces a con-
centration of Democrat contributors in certain areas. Finally, reference group effects imply 
that, holding location choices fixed, individuals are more likely to participate in politics (e.g., 
make a campaign contribution) if they live in an area with a majority of supporters of their 
own political party. The social incentives identified by the experimental evidence from the 
previous sections (i.e., signaling and informational effects) are a subset of this broader family 
of reference group effects.

The key limitation of the existing cross-sectional evidence on the geographic concentration 
of contributions is that it cannot disentangle the contribution of selection and reference group 
effects to the observed intra-cluster correlation. Intuitively, it cannot disentangle whether 
a Democrat contributes more to Obama because of the implicit pressure from Democrat 
neighbors, or simply that Democrats (or Republicans) who contribute more are more likely 
to move next to other supporters of their party. An ideal experimental design to disentangle 
these effects would consist in taking a pair of identical Republican (or Democrat) contribu-
tors and, before the beginning of an election cycle, assign them randomly to residential areas 
with different political compositions. The effect of interest would be the difference in cam-
paign contributions during election cycle as a function of the composition of the randomly 
assigned area of residence. In the presence of reference group effects, we should observe a 
relationship between the political composition of this randomly assigned reference group and 
the individual’s contributions. Although this ideal experiment is not feasible, we exploit a 
quasi-experimental design that provides similar variation in the composition of geographic
reference groups. Our quasi-experimental research design follows a group of about 100,000 individuals who were living in the same area and contributed similar amounts to the same presidential candidate in the 2008 presidential election cycle, but changed residence afterward. The analysis exploits the timing of residential moves in an event-study framework.

5.2.2 Data and Results

The data used in this study originates from the FEC database on individual contributions to presidential campaigns, which includes contributors who gave at least $200 to a presidential committee in a given election cycle. As with the experimental evidence presented in previous sections, we focus on presidential campaigns because the candidates are the same irrespective of the contributor’s area of residence, which allows for more straightforward comparisons of contributions across space. We identified contributors to the 2008 election cycle who registered a change in address with the USPS’ National Change of Address (NCOA) database in the time between the elections. This data provided information about where and when these contributors moved between January 2009 and July 2012.48

In practice, we focus on individuals who moved between the end of the 2008 election cycle and the beginning of the 2012 election cycle (between January 2009 and March 2011), and those who moved after the end of the 2012 election cycle (January 2012 and July 2012). Finally, we use the FEC records again to measure the contributions of these individuals during the 2012 election cycle.

The idea of the thought experiment described above was to compare the behavior of pairs of identical individuals with exogenously different reference groups. In the quasi-experiment, we do this by comparing individuals who - by the end of the 2008 election - were living in the same ZIP-3, and had contributed similar amounts (in $100 intervals) to the same political party. According to this criteria, the 82,820 contributors who moved between the 2008 and the 2012 election cycles were subsequently divided into 8,971 groups. In this sample, the average contribution during the 2008 cycle was $580, with 81% donating to the Democratic Party. A 19.8% of this sample of 2008 contributors appear in the FEC records as making a contribution during the 2012 election cycle.49 Among this 19.8% of individuals who contributed again, the average amount contributed was about $700.

Figure 6 illustrates this data on a map of the United States. The arrows denote changes of residence for the sample of individuals who moved between the 2008 and 2012 election cycles. The red arrows correspond to a group of Democratic contributors in a single ZIP-3 who made contributions in the same $100 interval during the 2008 election cycle. The color

48The timeframe for the analysis is limited to the 2008 election cycle because the NCOA data goes back only 48 months.

49This does not include contributions below $200 because those are not reported by the FEC.
of each ZIP-3 denotes its political composition, with a darker shade denoting an area with a higher density of Democrat contributors. On average, individuals move to ZIP-3s with similar political composition similar to the ZIP-3s of origin. Nevertheless, the correlation between the political compositions of the origin and destination ZIP-3s is about 0.7, which indicates that there is substantial variation in the political composition of the destination ZIP-3s.

The first part of the analysis assesses the relationship between the contributions made during the 2012 election cycle and the political composition of the destination ZIP-3. We estimated a model where the dependent variable is the amount contributed during the 2012 election cycle by contributors to the 2008 campaigns who moved between the two elections. We use a censored regression model to account for the fact that about 80% of those who moved did not contribute again in 2012.\textsuperscript{50} The key regressor is the percentage of own-party contributors in the destination ZIP-3.\textsuperscript{51} Figure 7.a shows the regression results in graphical form. The red dots correspond to the results for the subset of contributors who moved between the 2008 and 2012 election cycles. These results suggest that individuals contribute more when there are more supporters of their own party in their area of residence. Contributions are $200 higher for individuals residing in areas in the top quintile of own party contributors compared to those for residents of areas in the first quintile. This economically significant difference represents 29% of the average amount contributed in this sample.

There are two potential confounding factors in this observational analysis. First, it is possible that individuals moving to ZIP-3s with more contributors from their own party had a stronger initial affiliation with their party. Second, it is possible that individuals who moved to ZIP-3s with more contributors from their own party moved there because they were becoming more strongly affiliated to their party over time. These alternative explanations can be tested by means of an event-study framework. In the context of our application, this falsification exercise consists in reproducing the results by comparing individuals who moved before the 2012 election cycle with those who moved after the end of the 2012 cycle. Intuitively, if individuals moving to areas with a higher share of own-party neighbors initially had a stronger affiliation with their party, their contributions during the 2012 election cycle should be higher even if they moved after the end of the 2012 cycle. Similarly, if individuals moved to areas with a higher share of own party neighbors because they were becoming more strongly affiliated with their party over time, we would expect that their contributions during the 2012 election cycle should be higher even if they moved after the end of the 2012 cycle.

\textsuperscript{50}We obtained similar results based on a Poisson regression model.
\textsuperscript{51}The regression also included a set of individual-level characteristics (e.g., gender, race) and ZIP-3-level characteristics (e.g., average income) as control variables - see footnote to Figure 7 for details.
In Figure 7.a, as described previously, the red dots correspond to individuals who moved between the 2008 and 2012 election cycles. For this sample, the y-axis measures the contributions during the 2012 cycle and the x-axis measures the political composition of the ZIP-3 where the individuals moved right before the beginning of the 2012 election cycle. The falsification test is shown by the blue squares, which correspond to the results for the sample of individuals who moved after the end of the 2012 cycle. For this sample, the y-axis also measures the contributions during the 2012 cycle and the x-axis measures the political composition of the destination ZIP-3. The difference is in the timeframe of the change of residence: the x-axis refers to the destination ZIP-3s for individual who moved right after the end of the 2012 election cycle. As expected, there is no significant correlation between an individual’s contributions during the 2012 election cycle and the political composition of the ZIP-3 where these individuals moved after the end of the 2012 cycle. This suggests that the previous estimates of the reference group effects have the presumed direction of causality.

A potential concern with the results is whether the ZIP-3 is an appropriate level of geographical aggregation for reference group effects. We can test if these effects are relevant for a wider geographical area by computing the share of own-party contributors in the ring of ZIP-3s adjacent to that of each individual in the sample (see also Bottan and Perez-Truglia, 2011). In Figure 7.b, the blue squares represent the relationship between individual contributions and the share of own-party contributors in adjacent ZIP-3s. The lack of significant correlation suggests that reference group effects do not extend beyond the individual’s ZIP-3 of residence. This evidence is consistent with the notion of social effects, which depend on social interactions that are more likely to occur between individuals that live and work close to each other.52

Table 10 presents further regression results. The main independent variable, \(\log \left( \frac{N_{\text{own}}}{N_{\text{opp}}} \right)\), is the logarithm of the ratio between the number of own-party contributors and the number of other party contributors in the ZIP-3.53 Columns (1) through (3) confirm the findings from Figure 7. Column (1) indicates that a 100% increase in \(\frac{N_{\text{own}}}{N_{\text{opp}}}\) generates an increase in contributions of $81, roughly equivalent to 11.6% of the average for those with positive

\footnote{As discussed in the context of our experimental results, the data limitations for measuring the share of own-party individuals does not allow us to explore more localized effects, such as the party composition among next-door neighbors. If the mechanisms driving our results operate mostly in such close-knit networks, the ZIP-3-level effects we find would represent under-estimations of the true magnitude of the social effects, because they would result from the average between high effects for nearby neighbors and low or zero effects for more distant neighbors in the same ZIP-3.}

\footnote{We use this specification because it simplifies the algebra in the counterfactual analysis of the following subsection, but in practice this variable is almost identical to the share of own-party contributors, \(\frac{N_{\text{own}}}{N_{\text{own}}+N_{\text{opp}}}\), with a correlation of 0.99.}
contributions. Column (2) confirms that the political composition of the ZIP-3s adjacent to where the individual moved before the election cycle does not have a significant effect on contributions. The difference between the coefficients on own ZIP-3 and adjacent ZIP-3 is statistically significant (p-value of 0.05). The results in column (3) indicate that the political composition of the ZIP-3 where the individuals moved right after the 2012 election cycle (Future ZIP-3) does not have a significant effect on their 2012 contributions. The difference between the coefficients on the political composition for ZIP-3 and Future ZIP-3 is also statistically significant (p-value of 0.02). Table 10 presents some additional robustness checks. Column (4) presents the regression results excluding the ZIP-3-level control variables. The point estimate on log \( \frac{N_{own}}{N_{opp}} \) from column (4) is slightly higher than that of column (1), but the difference between the two coefficients is not statistically significant. Column (5) splits the independent variable log \( \frac{N_{own}}{N_{opp}} \) in two terms: log \( N_{own} \) and log \( N_{opp} \). Both coefficients are statistically and economically significant. The difference between the absolute value of the two coefficients is borderline statistically significant (p-value of 0.09), which suggests that the effect of own-party and opposite-party neighbors may not be symmetric.

Columns (6) and (7) from Table 10 report the results for the baseline specification in (1), but for the extensive margin: i.e., the dependent variable is 100 if the individual made at least one contribution and 0 otherwise. The results in Column (6) indicate that a 100% increase in \( \frac{N_{own}}{N_{opp}} \) generates an increase in the probability of contributing that is roughly equivalent to 7.3% of the average contribution rate. We must recall that the independent variable used is not the actual political composition in the individuals’ ZIP-3, but our proxy computed from contribution data, which may be subject to various sources of measurement error. This potential attenuation bias can be addressed with an instrumental variables estimation. Column (7) reports the 2SLS regression results where log \( \frac{N_{own}}{N_{opp}} \) is instrumented with the same ratio computed with county-level electoral results rather than contributions. Consistent with the possibility of attenuation bias, the coefficient on log \( \frac{N_{own}}{N_{opp}} \) is roughly 50% higher in column (7) compared to that of column (6). The estimate in column (7) implies that a 100% increase in \( \frac{N_{own}}{N_{opp}} \) generates an increase in contributions roughly equivalent to 11.2% of the average contribution rate.

Table 11 presents further regression results that allow for heterogeneous effects. These estimates can help us assess the robustness of the results by establishing whether the average effects are driven entirely by particular subgroups of individuals. Column (1) presents the

54 The sample size increases because in this specification we include the sample of individuals who moved right after the 2012 election cycle.

55 For instance, this proxy does not include all contributors, only those giving more than $200. Moreover, there may be sampling variation in the computation of those averages, and the measure does not incorporate the intensity of the neighbors’ affiliation.
reference group effects for individuals who moved at a date closer (or further away) from the beginning of the 2012 cycle. We cannot reject the null hypothesis that the effects are the same for the two groups (p-value of 0.33). Column (2) shows the effects for contributors who reported to be retired and non-retired during the 2008 election cycle. The reference group effects are significant for both subgroups, but almost twice as high for retirees (p-value of 0.02). Column (3) compares the reference group effects for individuals moving within a state compared to those moving out of their original state of residence. The difference in effects between the two groups is not statistically significant (p-value of 0.82). Column (4) compares the reference group effects for Republican and Democrat contributors. The effects are about 60% higher for Republicans, and the difference is statistically significant. Given that this group represents only 19% of our sample, this would imply that we are under-estimating the average magnitude of the reference group effects. However, the 19% of Republican contributors in the sample may not be representative of average Republican contributors. Column (5) shows the effects for individuals with contributions above and below the median during the 2008 campaign. Even though the effects are significantly higher for larger contributors (p-value of 0.08), this difference can be explained entirely by the corresponding difference in the baseline contribution rates.

The quasi-experimental evidence suggests that individuals contribute more when there are more supporters of their own party in their reference group. The shortcoming of the quasi-experimental evidence, however, is that it does not allow us to disentangle the mechanisms that drive these reference group effects. The direction of these effects is consistent with the experimental evidence on signaling effects. However, the reference group effects may also result from other related types of social effects, or even to other mechanisms unrelated to social effects, such as differential degrees of solicitation by geographical area (Weatherford, 1982). Since social effects are a subset of reference group effects, a conservative interpretation is that the magnitude of the reference group effects constitutes an upper bound for the magnitude of the social effects. The following subsection provides a benchmark for the magnitude of these effects.

5.3 Reference Group Effects and Geographic Polarization

5.3.1 Measuring Geographic Polarization

In a given geographical area, reference group effects increase the willingness to contribute among supporters of the majority party and decrease the willingness to contribute among supporters of the minority party, thereby exacerbating geographical polarization. We exploit the quasi-experimental estimates from the previous subsection to quantify the contribution
of these reference group effects to geographic polarization.

We start by presenting a measure of geographic polarization and its magnitude of polarization in campaign contributions. Consider a fixed number of contributors in area $j$: $N^j$. Each of the $N^j$ contributors may choose to contribute to the Democratic Party ($c^j_i = 1$) or the Republican Party ($c^j_i = 0$), as determined by a Bernoulli process. Let $p^j$ denote the probability of the Bernoulli process in area $j$, and let $q^j$ be the proportion of actual Democratic contributions. The distribution of $q^j$ (or, more precisely, its dispersion) will depend on whether the Bernoulli events are independent or not within areas. A first possibility is that the Bernoulli events are fully independent within areas, i.e., $p^j = p \forall j$. This scenario is represented by a Binomial model. In this model, sampling variation implies that some areas have a few more Democratic contributors than others. However, the distribution of $q^j$ is compressed around $p$ as $N^j$ becomes larger. In the case of campaign contributions, $N^j$ is not extremely large (the median in our sample is 500 contributors per ZIP-3), so this source of variation may be substantial. An alternative assumption would be that the Bernoulli events are correlated between any pair of individuals in the same area - a result, perhaps, of selection forces (e.g., individuals choosing to leave near others with similar political leanings), or social effects (e.g., Democrats more likely to contribute when they living near other Democrats). This case can be represented by the beta-binomial model, according to which $p^j \sim Beta(\alpha, \beta)$. This model implies that the probability of Democratic contributions is $p = \frac{\alpha}{\alpha + \beta}$ with an intra-cluster correlation coefficient of $\rho = \frac{1}{1+\alpha+\beta}$.

The two panels in Figure 8 show the fit of these two alternative models. The histogram shows the dispersion of the share of Democratic contributors across the 890 ZIP-3s in the United States for the 2012 election cycle. Figure 8.a shows that the binomial model is extremely inaccurate: assuming that party contributions are independent within ZIP-3s, it predicts that 90% of these areas will have between 50% and 60% Democratic contributors, whereas the actual data is significantly more dispersed: 90% of ZIP-3s have between 28% and 84% Democratic contributors. Figure 8.b shows that, once we take the intra-ZIP-3 correlation in contributions into account, the model fits the observed data well. Since the binomial model is nested within the beta-binomial model, we can perform a Log-Likelihood Ratio test. The LR statistic of 126,603 strongly rejects the null hypothesis that the model is binomial rather than beta. Finally, we can define a geographic polarization index as the fraction of the coefficient of variance of $q^j$ that can be attributed solely to the intra-ZIP-3 correlation.$^{57}$ This index is 0.3 for campaign contributions. Using U.S. Census data for 2010,

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$^{56}$We assume that the decision to make a contribution and the choice of which party to donate to are independent decisions. If that assumption were relaxed, at least one extra parameter would be added to the model. However, as shown below, the data fits reasonably well without this extra parameter.

$^{57}$This calculation uses the coefficient of variance instead of the standard deviation as a normalization to
we computed the same index for other population characteristics. The index of geographic polarization is 0 for gender, 0.11 for age, 0.16 for income, 0.25 for education and 0.32 for race.\textsuperscript{58} The degree of polarization of campaign contributions is thus high relative to the socio-economic characteristics of the population: it is close to that of racial polarization, which is widely recognized to be high in the U.S. (Schelling, 1969).

5.3.2 Counterfactual Analysis with the Quasi-Experimental Estimates

The measure presented above can be used to estimate the counterfactual level of geographic polarization without reference group effects. Consider the following stylized model that links reference groups to geographic polarization. Let \( M_D^j \) and \( M_R^j \) represent the fixed number of “sympathizers” to the Democratic and Republican Party, respectively. We allow the number of sympathizers to be geographically clustered, in order to represent the selection forces. In practice, we do not observe the number of sympathizers, but we do observe the number of contributors. Each sympathizer has certain probability of making a contribution to his or her favorite party. Let \( N_D^j \) and \( N_R^j \) represent the number of contributors to the Democratic and Republican party, respectively. Let \( p_D^j \) be the probability that a given sympathizer of the democratic party makes a contribution:

\[
\log \left( p_D^j \right) = \gamma \cdot \log \left( \frac{M_D^j}{M_R^j} \right) + \log \left( \tilde{p}_D^j \right)
\]

Reference group effects

The parameter \( \gamma \) represents the reference group effects: i.e., the elasticity between the probability of making a contribution and the ratio of own to opposite party sympathizers. \( \tilde{p}_D^j \) represents the counterfactual probability: i.e., what the probability of contributing would have been in absence of reference group effects. It is straightforward to show that if we assume \( \tilde{p}_D^j = \varphi \forall j \), then we can estimate the above equation replacing the unobservable ratio \( \frac{M_D^j}{M_R^j} \) by the observable ratio \( \frac{N_D^j}{N_R^j} \).\textsuperscript{59} Intuitively, we allow each ZIP-3 to have a different average contribution rate as long as the ratio of contribution rates between Democratic and Republican sympathizers is constant. A \( \varphi > 1 \) would mean that, on average, a Democratic sympathizer is more likely to make a contribution than a Republican sympathizer. The

\[\frac{\sum_j \sqrt{\frac{1}{\alpha + \beta} \frac{N_D^j}{N_R^j}}}{\sqrt{\sum_j \frac{1}{\alpha + \beta}}}\]

\textsuperscript{58}The binary outcomes are, respectively: male/female, above/below 25 years of age, over/under $30,000 of individual income for the population with earnings, above/below college degree for the population above 25 years old, and white/non-white.

\textsuperscript{59}As discussed in the previous section, the non-negligible sampling variation in \( N_D^j/N_R^j \) will generate an attenuation bias that we can correct with instrumental variables.
regression results from the previous subsection indicate that \( \gamma = 0.11 \).

We can compute how this reference group effect translates into excess polarization. A simple definition of excess polarization allows us to obtain some tractable results:

\[
EP_j = \frac{\hat{p}_D^j M_D^j}{\hat{p}_R^j M_R^j} - \frac{\hat{p}_D^j M_D^j}{\hat{p}_R^j M_R^j}
\]

\( \hat{p}_D^j M_D^j \) denotes the polarization in \( j \), equal to the ratio of expected Democratic contributors to expected Republican contributors, and \( \hat{p}_R^j M_R^j \) denotes the polarization in the counterfactual scenario with no reference group effects. Thus, the \( EP_j \) indicates how much more polarized, in proportional terms, a given area is expected to be relative to the counterfactual scenario with no reference group effects. It is straightforward to prove that:

\[
EP_j = \left( \frac{M_D^j}{M_R^j} \right)^{2\gamma} - 1
\]

The reference group effects amplify the existing geographic polarization in the number of sympathizers, \( \frac{M_D^j}{M_R^j} \). \( EP_j \) is a function of \( 2\gamma \) instead of \( \gamma \) because reference group effects come in pairs: e.g., a higher share of Democratic sympathizers increases contribution rates for Democrats at the same rate than it decreases them for Republicans. If the number of sympathizers was exactly even, the ratio \( \frac{M_D^j}{M_R^j} \) equals 1, and the reference-group effects on polarization cancel out, resulting in \( EP_j = 0 \). In an area where the number of Democratic sympathizers is three times the number of Republican sympathizers (i.e., 75%-25%) the reference group effects generate an excess polarization of about 27%.

The above formula provides the intuition for why the average excess polarization at the national level does not depend on \( \gamma \) alone, but on the interaction between \( \gamma \) and the distribution of \( \frac{M_D^j}{M_R^j} \) across the country. Even though \( \frac{M_D^j}{M_R^j} \) is unobservable, we can proxy for it using some extra assumptions.\(^{60}\) Using these estimates, we can reproduce the histogram in Figure 7 for the counterfactual scenario with no reference group effects. We present the results in Figure 9. The actual coefficient of variation in the share of Democratic contributors is about 19.2% higher than it would be in the counterfactual scenario with no reference group effects.

\(^{60}\) It is straightforward to show that \( \frac{M_D^j}{M_R^j} = \left( \frac{1}{\varphi} \frac{E[N_D^j]}{E[N_R^j]} \right)^{1/1+2\gamma} \). At the cost of ignoring the sampling variation, we can replace the expected contributions, \( E[N_D^j]/E[N_R^j] \), by the corresponding actual contributions, \( N_D^j/N_R^j \). In that case, the only missing piece of information is \( \varphi \). It is easy to prove that \( \sum_j M_D^j/\sum_j M_R^j \) is a function of \( \varphi \) and other known information (\( \gamma \) and the distribution of \( N_D^j/N_R^j \)). Thus, if we assume \( \sum_j M_D^j = \sum_j M_R^j \) (i.e., an even distribution of total sympathizers in the country), we can infer \( \varphi \) by computing the unique \( \varphi \) such that \( \sum_j M_D^j/\sum_j M_R^j = 1 \), which results in an estimate of \( \hat{\varphi} = 1.53 \).
In other words, geographic polarization in the United States is nearly 20% higher because of these reference group effects.

In addition to the direct contribution of reference group effects to geographical polarization, we must note that reference group effects may also make a significant indirect contribution to geographic polarization by exacerbating positive assortative matching in terms of political affiliation. This implies that individuals may anticipate that they will be treated better in places where most neighbors share their political views and this may affect their decision on where to reside, which, in turn, provides incentives to live in areas with more sympathizers of the own party.

6 Conclusions

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

A first set of results on signaling effects indicate that feeling observed by others significantly affects individuals’ contributions. Unlike in the cases of voter turnout and charitable giving, individuals who make campaign contributions face a double audience that may approve or disapprove of their actions. We found that higher visibility increases contributions for individuals supporting the majority party in the areas, but decreases contributions for individuals supporting the minority party in the area. A second set of results, about informational effects, indicate that an individual’s contributions are also affected by her perception of others’ contribution patterns. An individual contributes more if she observes a higher average contribution by neighbors who support her same party, which is consistent with the existence of a social norm. Individuals do not react to information about the average contributions of supporters of the opposite party. We found no evidence that individuals contribute because they believe to be making a pivotal contribution. And individuals contribute less when their own party is raising higher total contributions than the opposite party, which we interpret as moral free-riding.

Some of these social mechanisms have conflicting effects. For example, while signaling decreases contributions by individuals of the minority party, moral free-riding has the opposite effect. Also, it is possible that there are other social mechanisms that our experiment
was not designed to identify. Thus, we complemented the field experiment evidence with quasi-experimental evidence from an event-study analysis of residential mobility for a panel of contributors. More precisely, we estimated the effect of the political composition in the ZIP-3 of reference on an individual’s contribution, which we denominated reference-group effects. The evidence indicates that an individual contributes more when there is a higher proportion of supporters of her same party party in the area of residence. The reference group effects work in the direction implied by signaling effects, which, in turn, are consistent with the notion of conformity effects (Bernheim, 1994). The reference group effects increase contributions from supporters of the local majority and decrease contributions from supporters of the minority party, thereby increasing geographic polarization. Indeed, the counter-factual analysis suggested that geographic polarization in contributions would be 20% lower in the United States if it was not for the reference group effects.

The evidence presented in this paper has direct practical applications. First, social incentives can be exploited to design better fundraising strategies. For example, one of the experimental findings suggests that disseminating unbiased information about contributions in a given area may be a cost-effective way of increasing contributions. The evidence also warns about potential self-defeating dissemination efforts: e.g., conveying the message that the total amount raised is high will decrease the individual’s desired contribution through the moral free-riding effect. The findings are also relevant for disclosure policies. Technological change is constantly reducing the cost of collecting and disseminating information. The government, but also the private sector, must decide how to disclose this data. Our evidence indicates that, due to social incentives, disclosure policies are non-neutral: i.e., the individual’s behavior depends on whether her actions are being disclosed or not. Our results provide some insights on how to evaluate the effects of disclosure policies in the context of campaign contributions, but our experimental design can also be used to study the impact of disclosure policies in other contexts, such as charitable giving or tax evasion.61

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61Distinguishing between multiple social mechanisms allows for a better design of disclosure policies. For example, if the FEC wanted to avoid signaling effects in campaign contributions without affecting the informational effects, the FEC could semi-anonymize the contribution records posted online.
References


Figure 1: Timeline of the Field Experiment

- APRIL 1, 2011: List Letter (N=36,795)
- APRIL 1, 2011: Awareness Letter (N=36,774)
- APRIL 1, 2011: Placebo Letter (N=18,430)
- NOV. 6, 2012: Post-election surveys sent
- DEC. 6, 2012: Election day

Outcome of Interest:
Post-treatment contributions
(Source: FEC administrative data)

Pre-treatment period
Post-treatment period

Notes: See Section 2 for more details about the experimental design.
Figure 2: Contributors’ Perceptions about Confidentiality of Contribution Records (Post-Election Mail-In Survey)

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

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

Notes: The data corresponds to the responses to our post-election mail-in survey. The figures are based on responses from individuals in the experiment’s Control group (i.e., individuals that did not receive any mailings). Panel a combines the answers to two questions: a question about the disclosure policy of information on individual contributions (see question 4 from the questionnaire in Appendix C) and a further question on the respondent’s confidence on the answer provided to the previous question (see question 5 from the questionnaire in Appendix C). Panel b is based on responses to a question about what the respondent thinks that his or her neighbors believe about the disclosure policy, where the four possible responses range from “A vast majority of your neighbors believe that information on individual campaign contributions is public” to “A vast majority of your neighbors believe that information on individual campaign contributions is confidential” (see question 7 from the questionnaire in Appendix C).
Figure 3: Signaling Effects: Experimental Results in Graphical Form

a. Signaling effect (A-Neighbors - A-Own)

b. Signaling effect, extensive margin

c. Effect of Awareness-Neighbors letters

d. Effect of Awareness-Own letters

e. Effect of Placebo letters

f. Signaling effect, pre-treatment contributions

Notes: $N = 154,707$. Each regression line and its confidence interval were estimated from a regression of the relevant outcome (e.g., amount contributed post-treatment) on treatment dummies, the share of own-party contributors in the 3-digit ZIP code (ZIP-3) and the interaction between these variables, and individual-level controls. Interval regressions are used when the amount contributed is the dependent variable, and OLS for all others. Standard errors are clustered at the ZIP code/party level. For more details about the specification, see the notes to Table 4. The dots represent binned scatter plot versions of the partial regression plots.
Notes: Histograms are based on responses to the post-election mail-in survey. Control Group refers to respondents who did not receive any letters during the experimental stage, while Awareness Letters refers to respondents who received letters of type Awareness. This measure of perceptions about the publicity of contribution records combines the answers to a question about the type of disclosure policy (question 4 from Appendix C) and another question about the respondent’s confidence on that answer (question 5 from Appendix C).
Figure 5: Correlational Evidence on Signaling and Informational Effects (Post-Election Mail-In Survey)

**a.** Correlation between respondents’ contributions and perception of neighbors’ beliefs about confidentiality of contribution records:

**b.** Correlation between survey measure of social norms on contributions and own contributions

Notes: The data corresponds to the responses to our post-election mail-in survey and to respondents’ contributions matched from FEC records (see Table 3 for some descriptive statistics of this data). The figures are based on responses from individuals in the experiment’s Control group (i.e., individuals that did not receive any mailings). The outcome variable on the y-axis is the same as that of the experimental analysis (i.e., the amounts contributed to the Obama or Romney presidential campaign after the mailing). Panel a. was obtained from an interval regression equivalent to the one used in Figure 3, where the key independent variable is the interaction between the share of own-party contributors in the ZIP-3 and the (standardized) survey-measure of perception of neighbors’ beliefs about disclosure policy (question 6 from Appendix C). The x-axis in panel b. corresponds to the quintiles of the distribution of responses to the survey question about how much individuals “should” contribute to a presidential campaign (question 8 from Appendix C), whereas the y-axis represents the average amount contributed by recipients in the corresponding quintile.
Figure 6: Event Study of Residential Mobility: Sample of Contributors Moving between the 2008 and 2012 Election Cycles

Notes: The data corresponds to contributors from FEC records (see Table 3 for some descriptive statistics of this data) matched with the USPS National Change of Address database (NCOA). Each arrow indicates the origin and destination area of residence for an arbitrary sample of contributors who moved between the 2008 and 2012 election cycles (i.e., contributors during the 2008 cycle who moved between January 2009 and March 2011). The blue (red) arrows corresponds to a set of 24 (20) individuals who lived in ZIP-3 021 (752) during the 2008 election cycle and made a contribution in the same $100 interval to a Democratic (Republican) presidential committee. The color of each ZIP-3 denotes its political composition in terms of the share of Democratic contributors among all the contributors to presidential campaigns during the previous three election cycles, with a darker shade denoting a higher concentration of Democratic contributors.
Figure 7: Event Study of Residential Mobility: Correlation between Contributions and Political Composition in Area of Residence

**a. Current movers vs. Future movers**

**b. Same ZIP-3 vs. Adjacent ZIP-3**

Notes: The data corresponds to contributors from FEC records (see Table 3 for some descriptive statistics of this data) matched with the USPS National Change of Address database (NCOA). The dots represent the coefficients on a set of dummy variables for the quintiles of the distribution of the x-axis. The leftmost dot corresponds to the omitted category, so its coefficient is normalized to zero. Each line corresponds to a linear fit of the dots of matching color. In both panels, the red dots correspond to a regression of the amount contributed during the 2012 cycle on the quintile dummies of the political composition in the ZIP-3 where the individual was living during the 2012 cycle. The blue squares from the left panel correspond to the equivalent regressions where the independent variables correspond to the political composition in the ZIP-3 where each individual moved after the end of the election cycle. The blue squares from the right panel correspond to the equivalent regressions where the independent variables correspond to the political composition in the ring of ZIP-3s adjacent to the ZIP-3 where the individual lived during the 2012 cycle. All regressions include individual-level controls, ZIP-3-level controls and group dummies as described in the notes to Table 10. The x-axis is the share of Democratic contributors among all the contributors to presidential campaigns during the previous three election cycles.
Figure 8: Geographic Polarization in Campaign Contributions

a. Binomial Model

b. Beta-Binomial Model

Notes: $N = 890$. Each observation corresponds to a ZIP-3 area. The x-axis represents the share of Democratic contributors among all the contributors to presidential campaigns during the 2012 election cycle. The histograms correspond to actual contribution levels. The lines correspond to kernel density estimators: the blue line for the actual data and the red line for simulated values for the binomial and beta-binomial models. The parameter $p$ is the probability that a contributor is Democrat. The parameter $\rho$ captures the corresponding intra-ZIP-3 correlation.

Figure 9: Event Study of Residential Mobility: Counterfactual Analysis of the Importance of Reference Group Effects for Geographic Polarization

Notes: $N = 890$. Each observation corresponds to a ZIP-3 area. The x-axis represents the share of Democratic contributors among all the contributors to presidential campaigns during the 2012 election cycle. Actual refers to the histogram computed with observed contributions from FEC records. Counterfactual refers to the histogram for the counter-factual scenario with no reference group effects, according to the calculations described in subsection 5.3.
Table 1: Comparison of Individual Characteristics for Individuals in the Subject Pool, for All Contributors in the 2012 Election Cycle and for the General U.S. Population

<table>
<thead>
<tr>
<th></th>
<th>Subject Pool</th>
<th>All contributors</th>
<th>US Average</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent Democrat</td>
<td>52.60</td>
<td>64.55</td>
<td>51.40</td>
</tr>
<tr>
<td></td>
<td>(49.93)</td>
<td>(47.84)</td>
<td></td>
</tr>
<tr>
<td>Mean amount contributed ($)</td>
<td>811.69</td>
<td>559.39</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(880.61)</td>
<td>(846.59)</td>
<td></td>
</tr>
<tr>
<td>Percent Male</td>
<td>59.37</td>
<td>55.15</td>
<td>49.14</td>
</tr>
<tr>
<td></td>
<td>(48.27)</td>
<td>(48.82)</td>
<td></td>
</tr>
<tr>
<td>Percent White</td>
<td>79.14</td>
<td>78.77</td>
<td>62.99</td>
</tr>
<tr>
<td></td>
<td>(21.68)</td>
<td>(22.20)</td>
<td></td>
</tr>
<tr>
<td>Percent Black</td>
<td>11.95</td>
<td>12.01</td>
<td>12.07</td>
</tr>
<tr>
<td></td>
<td>(14.34)</td>
<td>(14.52)</td>
<td></td>
</tr>
<tr>
<td>Population density, zip-level</td>
<td>6177.42</td>
<td>6360.17</td>
<td>3907.85</td>
</tr>
<tr>
<td></td>
<td>(15505.87)</td>
<td>(16136.04)</td>
<td></td>
</tr>
<tr>
<td>Mean income ($), zip-level</td>
<td>105009.24</td>
<td>98097.34</td>
<td>55241.02</td>
</tr>
<tr>
<td></td>
<td>(114364.60)</td>
<td>(113653.43)</td>
<td></td>
</tr>
</tbody>
</table>

Observations 191,832 1,070,098

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

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Control</td>
<td>Placebo</td>
<td>Awareness</td>
<td>Awareness</td>
<td>List</td>
<td>List</td>
<td>Difference Test</td>
</tr>
<tr>
<td>Percent Democratic</td>
<td>52.59</td>
<td>52.67</td>
<td>53.33</td>
<td>52.53</td>
<td>52.46</td>
<td>52.00</td>
<td>0.24</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td></td>
</tr>
<tr>
<td>Mean Contribution Amount ($)</td>
<td>524.24</td>
<td>520.15</td>
<td>516.55</td>
<td>517.57</td>
<td>528.91</td>
<td>526.30</td>
<td>0.29</td>
</tr>
<tr>
<td></td>
<td>(2.00)</td>
<td>(4.51)</td>
<td>(4.56)</td>
<td>(4.70)</td>
<td>(4.74)</td>
<td>(4.76)</td>
<td></td>
</tr>
<tr>
<td>Percent Male</td>
<td>59.26</td>
<td>59.59</td>
<td>58.66</td>
<td>59.68</td>
<td>59.54</td>
<td>59.91</td>
<td>0.19</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td>(0.37)</td>
<td></td>
</tr>
<tr>
<td>Percent White</td>
<td>79.12</td>
<td>78.99</td>
<td>79.17</td>
<td>79.20</td>
<td>79.20</td>
<td>79.32</td>
<td>0.82</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.17)</td>
<td>(0.17)</td>
<td>(0.17)</td>
<td>(0.17)</td>
<td>(0.17)</td>
<td></td>
</tr>
<tr>
<td>Percent Black</td>
<td>11.91</td>
<td>12.11</td>
<td>11.86</td>
<td>11.98</td>
<td>12.10</td>
<td>11.90</td>
<td>0.40</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td></td>
</tr>
<tr>
<td>Percent Hispanic</td>
<td>4.12</td>
<td>4.16</td>
<td>4.03</td>
<td>4.06</td>
<td>3.84</td>
<td>3.99</td>
<td>0.23</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.10)</td>
<td>(0.11)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>99,834</td>
<td>18,430</td>
<td>18,314</td>
<td>18,459</td>
<td>18,396</td>
<td>18,399</td>
<td></td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Largest pre-treatment cont. to</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Obama</td>
<td>Romney</td>
<td>Other</td>
</tr>
<tr>
<td><strong>Pre-Treatment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total amount contributed to all candidates ($)</td>
<td>523.12</td>
<td>370.19</td>
<td>650.86</td>
<td>706.80</td>
</tr>
<tr>
<td>Percent Contributed to Obama/Romney</td>
<td>65.75</td>
<td>100.00</td>
<td>100.00</td>
<td>1.01</td>
</tr>
<tr>
<td>Amount to Obama/Romney ($), if &gt;0</td>
<td>417.94</td>
<td>370.19</td>
<td>610.13</td>
<td>477.42</td>
</tr>
<tr>
<td><strong>Post-Treatment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percent contributed to Obama/Romney</td>
<td>49.01</td>
<td>76.20</td>
<td>38.42</td>
<td>11.80</td>
</tr>
<tr>
<td>Amount to Obama/Romney ($), if &gt;0</td>
<td>588.84</td>
<td>567.12</td>
<td>609.89</td>
<td>775.66</td>
</tr>
<tr>
<td>Observations</td>
<td>191,832</td>
<td>100,541</td>
<td>24,910</td>
<td>66,381</td>
</tr>
</tbody>
</table>

Notes: The pre-treatment period ranges from April 1, 2011 to the date of receiving the letter, and the post-treatment period ranges from the date of receiving the letter to December 31, 2012. The other Republican candidates are: Bachman, Cain, Gingrich, Huntsman, Paul, Pawlenty, Perry and Santorum. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).
Table 4: Signaling Effects: Robustness Checks

<table>
<thead>
<tr>
<th></th>
<th>Post-Treatment Contribution</th>
<th>Placebo</th>
<th>Pre-Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Amount</td>
<td>Amount</td>
<td>Amount</td>
</tr>
<tr>
<td></td>
<td>(37.473)</td>
<td>(38.710)</td>
<td>(42.195)</td>
</tr>
<tr>
<td>Interaction with:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share own-party in zip3</td>
<td>146.902**</td>
<td>150.948**</td>
<td>157.048*</td>
</tr>
<tr>
<td></td>
<td>(61.381)</td>
<td>(63.651)</td>
<td>(86.320)</td>
</tr>
<tr>
<td>Share own-party in adj.-zip3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share low-income in zip3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Regression</td>
<td>Interval</td>
<td>Interval</td>
<td>Interval</td>
</tr>
</tbody>
</table>

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors clustered at the ZIP code/party level. The dependent variable is the amount in dollars contributed to the Obama or Romney presidential campaign after receiving the letter, except for column (6) which in which the dependent variable is the amount contributed before receiving the mailing (pre-treatment). All regressions include observations from the Control group and from the Placebo and Awareness treatment groups. The key independent variables are the treatment-type dummies interacted with ZIP-3-characteristics (e.g., share of own-party contributors). A-Neighbors - A-Own is the difference between the coefficients on the treatment dummies for Awareness-Neighbors and Awareness-Own, except in column (5) where it is the difference between the coefficients on the treatment dummies for Placebo and Control. The share own-party in ZIP-3 stands for the share of own-party contributors to presidential campaigns in the ZIP-3 in the three previous presidential election cycles. The “Share own-party in adj.-ZIP-3” refers to the same variable averaged over the ZIP-3s adjacent to the ZIP-3 where the individual resides. The “Share low-income in ZIP-3” refers to the share of income-earning adults with income below $30,000, according to U.S. Census Bureau data for 2010. Controls refers to individual-level control variables: a party dummy, amount contributed pre-treatment to each candidate, gender, race dummies and the date when received the mailing. Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).
<table>
<thead>
<tr>
<th></th>
<th>By Cont. Type</th>
<th>By Cont. Date</th>
<th>By Party</th>
<th>By Gender</th>
<th>By Pop. Density</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td></td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
<td>(10)</td>
</tr>
<tr>
<td><strong>Signalling Semi-elasticity:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>20% own-party in zip3</td>
<td>-0.064**</td>
<td>-0.057</td>
<td>-0.104***</td>
<td>-0.031</td>
<td>-0.025</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.068)</td>
<td>(0.038)</td>
<td>(0.038)</td>
<td>(0.025)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.070)</td>
<td>(0.045)</td>
<td>(0.045)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.045)</td>
<td>(0.045)</td>
<td>(0.043)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.053)</td>
<td>(0.053)</td>
<td></td>
</tr>
<tr>
<td>80% own-party in zip3</td>
<td>0.046**</td>
<td>0.061</td>
<td>0.066***</td>
<td>0.003</td>
<td>0.023**</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.050)</td>
<td>(0.023)</td>
<td>(0.024)</td>
<td>(0.011)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.065)</td>
<td>(0.033)</td>
<td>(0.028)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.055)</td>
<td>(0.055)</td>
<td>(0.025)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.040)</td>
</tr>
<tr>
<td><strong>Sub-group</strong></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
</tr>
<tr>
<td></td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
</tr>
<tr>
<td></td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
</tr>
<tr>
<td></td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
</tr>
<tr>
<td></td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
<td>155,037</td>
</tr>
</tbody>
</table>

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors clustered at the ZIP code/party level. The semi-elasticity denotes the proportional change in the probability of making a contribution in the post-treatment period from re-assigning the recipient from the Awareness-Neighbors to the Awareness-Own treatment. The semi-elasticity is computed for two values of the share of own-party contributors in the ZIP-3: 20% and 80%. The dependent variable is always a dummy variable for whether the individual contributed at all to either Obama or Romney after receiving the mailing, except in column (2) where it corresponds to contributions to other committees. Columns (3) and (4) consider contributions made during two different timeframes of the post-treatment period, before and after September 1, 2012. In columns (9) through (12) the sub-groups are defined according to the population density and the income in the individual’s ZIP-3, computed with 2010 data from the U.S. Census Bureau. The semi-elasticities are computed using the coefficients from a Logit regression, where the key independent variables are the treatment-type dummies interacted with ZIP-3-characteristics (share of own-party contributors in the ZIP-3). The group-specific effects are computed by interacting the main independent variables with the group-dummy (e.g., Democratic vs. Republican), except in columns (1) through (4) where each sub-group corresponds to the use of a different dependent variable. The regression specifications follow the same criteria described in the note to Table 4. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).
Table 6: Informational Effects: Sample Treatment Lists Generated with Different Parameter Values

<table>
<thead>
<tr>
<th>Baseline ($\theta_i^D = 0, \theta_i^A = 0$)</th>
<th>Low DEM ($\theta_i^D &lt; 0, \theta_i^A = 0$)</th>
<th>High Amount ($\theta_i^D = 0, \theta_i^A &gt; 0$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Contributor</td>
<td>Amount</td>
<td>Party</td>
</tr>
<tr>
<td>G., R.</td>
<td>$1,000</td>
<td>DEM</td>
</tr>
<tr>
<td>W., D.</td>
<td>$100</td>
<td>DEM</td>
</tr>
<tr>
<td>S., L. Y.</td>
<td>$100</td>
<td>DEM</td>
</tr>
<tr>
<td>W., T. K.</td>
<td>$100</td>
<td>DEM</td>
</tr>
<tr>
<td>A., S.</td>
<td>$90</td>
<td>DEM</td>
</tr>
<tr>
<td>B., R.</td>
<td>$50</td>
<td>DEM</td>
</tr>
<tr>
<td>W., S. B.</td>
<td>$1,100</td>
<td>REP</td>
</tr>
<tr>
<td>B., M. A.</td>
<td>$402</td>
<td>REP</td>
</tr>
<tr>
<td>A., E. A.</td>
<td>$120</td>
<td>REP</td>
</tr>
</tbody>
</table>

Notes: This is an example of how the algorithm generates different lists of 9 neighbors from a given sample of the recipient’s 30 closest neighbors. See Section 4 for a detailed description of the algorithm.
Table 7: Informational Effects: Main Regression Results

<table>
<thead>
<tr>
<th></th>
<th>Post-Treatment Contributions</th>
<th>Pre-Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Amount</td>
<td>(2) Amount</td>
</tr>
<tr>
<td>$\bar{c}_{own}$</td>
<td>2.452**</td>
<td>2.757*</td>
</tr>
<tr>
<td></td>
<td>(1.436)</td>
<td>(1.440)</td>
</tr>
<tr>
<td>$\bar{c}_{opp}$</td>
<td>-0.145</td>
<td>-0.667</td>
</tr>
<tr>
<td></td>
<td>(0.914)</td>
<td>(0.951)</td>
</tr>
<tr>
<td>$N_{own}$</td>
<td>-6.217**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.821)</td>
<td></td>
</tr>
<tr>
<td>$\sum c_{own}$</td>
<td></td>
<td>-0.546</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.528)</td>
</tr>
<tr>
<td>$\sum c_{opp}$</td>
<td></td>
<td>0.295</td>
</tr>
<tr>
<td>$\sum c_{own} - \sum c_{opp}$</td>
<td></td>
<td>-0.408**</td>
</tr>
<tr>
<td>$</td>
<td>\sum c_{own} - \sum c_{opp}</td>
<td>$</td>
</tr>
<tr>
<td>$</td>
<td>\sum c_{own} - \sum c_{opp}</td>
<td>^*$</td>
</tr>
<tr>
<td>Observations</td>
<td>155,059</td>
<td>155,059</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Regression</td>
<td>Interval</td>
<td>Interval</td>
</tr>
</tbody>
</table>

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors clustered at the ZIP code/party level. The dependent variable in columns (1) through (5) is the dollar amount contributed post-treatment. The dependent variable in columns (6) takes the value 100 if the individual made at least one contribution post-treatment and 0 otherwise. The dependent variable in column (7) is the dollar amount contributed pre-treatment. All the independent variables except $N_{own}$ are expressed in hundreds of dollars. $\bar{c}_{own}$ ($\bar{c}_{opp}$) corresponds to the average contribution of all the individuals in the list who contributed to the recipient’s own (opposite) party. $N_{own}$ is the number of individuals in the list who contributed to the recipient’s party. $\sum c_{own}$ ($\sum c_{opp}$) is defined as $\bar{c}_{own}$ ($\bar{c}_{opp}$) but refers to the sum of contributions instead of the average contribution. See Table F.1 for descriptive statistics for all these independent variables. These independent variables are included in the regression as the difference between the value computed with the list sent to the recipient and the corresponding value computed in the baseline list, except $|\sum c_{own} - \sum c_{opp}|^*$ for which we first compute the difference of $\sum c_{own} - \sum c_{opp}$ between the list sent and the baseline list, and then use the absolute value of this difference. All regressions include observations from the Control group and the Placebo and List treatment groups. The individual-level control variables are: dummies for Placebo and List, party dummy, amount contributed pre-treatment to each candidate, gender, race dummies and the date when received the mailing. Ethnicity and sex were imputed according to first and last name frequencies reported by the U.S. Census Bureau. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).
Table 8: Informational Effects: Treatment Effects on Alternative Outcomes and Group-Level Heterogeneity

<table>
<thead>
<tr>
<th></th>
<th>By Cont. Type</th>
<th>By Cont. Date</th>
<th>By Party</th>
<th>By Gender</th>
<th>By Once/Update</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Dep. Var.: Amount</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\bar{c}_{own}$</td>
<td>4.032***</td>
<td>10.113</td>
<td>2.910**</td>
<td>2.204*</td>
<td>5.180*</td>
</tr>
<tr>
<td></td>
<td>(1.562)</td>
<td>(14.038)</td>
<td>(1.280)</td>
<td>(1.224)</td>
<td>(2.756)</td>
</tr>
<tr>
<td>$\bar{c}_{opp}$</td>
<td>-1.450</td>
<td>-8.226</td>
<td>-1.068</td>
<td>-0.619</td>
<td>1.388</td>
</tr>
<tr>
<td></td>
<td>(1.077)</td>
<td>(7.902)</td>
<td>(0.828)</td>
<td>(0.876)</td>
<td>(3.244)</td>
</tr>
<tr>
<td>$\sum c_{own} - \sum c_{opp}$</td>
<td>-0.408**</td>
<td>-0.880</td>
<td>-0.464***</td>
<td>-0.043</td>
<td>-0.537</td>
</tr>
<tr>
<td></td>
<td>(0.171)</td>
<td>(1.260)</td>
<td>(0.137)</td>
<td>(0.135)</td>
<td>(0.351)</td>
</tr>
<tr>
<td>Observations</td>
<td>155,059</td>
<td>155,059</td>
<td>155,059</td>
<td>155,059</td>
<td>155,059</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Post-Treatment</th>
<th></th>
<th></th>
<th></th>
<th>Pre-Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Amount</td>
<td>(2) Amount</td>
<td>(3) Amount</td>
<td>(4) Amount</td>
<td>(5) P(Amount&gt;0)</td>
</tr>
<tr>
<td>Placebo</td>
<td>2.646</td>
<td>2.646</td>
<td>2.650</td>
<td>2.756</td>
<td>0.124</td>
</tr>
<tr>
<td></td>
<td>(7.939)</td>
<td>(7.939)</td>
<td>(7.937)</td>
<td>(7.938)</td>
<td>(0.327)</td>
</tr>
<tr>
<td>List</td>
<td>17.033***</td>
<td>18.012**</td>
<td>19.823***</td>
<td>26.237***</td>
<td>0.379</td>
</tr>
<tr>
<td></td>
<td>(6.034)</td>
<td>(8.239)</td>
<td>(6.336)</td>
<td>(8.524)</td>
<td>(0.245)</td>
</tr>
<tr>
<td>List * Update</td>
<td>-1.956</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(10.698)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>List * REP</td>
<td>-8.365</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(14.146)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>List * ($\bar{c}_{own} &gt;$500)</td>
<td>6.502</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(13.165)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>List * ($\sum c_{own} - \sum c_{opp} &gt; 0$)</td>
<td>-21.182*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(12.124)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>155,059</td>
<td>155,059</td>
<td>155,059</td>
<td>155,059</td>
<td>155,059</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Regression</td>
<td>Interval</td>
<td>Interval</td>
<td>Interval</td>
<td>Interval</td>
<td>Linear</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Amount Contributed</th>
<th>Prob. of Contributing</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>( \log(\frac{N_{own}}{N_{opp}}) )</td>
<td>81.202***</td>
<td>73.512***</td>
</tr>
<tr>
<td></td>
<td>(19.943)</td>
<td>(25.432)</td>
</tr>
<tr>
<td>Adjacent zip3</td>
<td>-35.021</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(38.469)</td>
<td></td>
</tr>
<tr>
<td>Future zip3</td>
<td>14.085</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(31.600)</td>
<td></td>
</tr>
<tr>
<td>( \log(N_{own}) )</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \log(N_{opp}) )</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>82,820</td>
<td>82,820</td>
</tr>
<tr>
<td>Groups</td>
<td>8,971</td>
<td>8,971</td>
</tr>
<tr>
<td>Zip3-level Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Model</td>
<td>Censored</td>
<td>Censored</td>
</tr>
</tbody>
</table>

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors clustered at the group-level. “Prob. of contributing” takes the value 100 if the individual made a positive contribution to a presidential committee during the 2012-cycle and 0 otherwise. “Amount contributed” corresponds to contributions to presidential campaigns during the 2012 election cycle. \( \log(\frac{N_{own}}{N_{opp}}) \) denotes the logarithm of the ratio of the number of presidential campaign contributors to the same party in the same ZIP-3 over the number of contributors to the opposite party, computed with contribution data from the three previous election cycles. The regression reported in Column (8) uses the county-level \( \log(\frac{N_{own}}{N_{opp}}) \) as an instrumental variable for the corresponding ZIP-3-level variable. All regressions include a set of individual-level controls, computed with FEC data combined with U.S. Census Bureau data, based on characteristics from the 2008 election cycle: party, amount, gender, race dummies, occupation dummies and number of months elapsed between January 2009 and the date of moving. The ZIP-3-level controls, computed with U.S. Census Bureau and IRS Statistics of Income data, include: total population, population density, share of college graduates, racial shares, index of ethnic fractionalization, mean household size, mean gross income and unemployment rate. All regressions include group-specific fixed effects, where each group is defined by individuals who were living in the same ZIP-3 and made a contribution in the same $100 interval to a presidential committee of the same political party during the 2008 election cycle. Data on amount contributed and recipient party from FEC public records (see Table 3 for descriptive statistics).
Table 11: Event Study of Residential Mobility: Further Regression Results

<table>
<thead>
<tr>
<th></th>
<th>Probability of Contributing</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>( \log(\frac{N_{\text{own}}}{N_{\text{opp}}}) )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Group 1</td>
<td>1.216***</td>
<td>2.569***</td>
<td>1.396***</td>
<td>1.198***</td>
<td>2.062***</td>
</tr>
<tr>
<td></td>
<td>(0.383)</td>
<td>(0.580)</td>
<td>(0.365)</td>
<td>(0.326)</td>
<td>(0.464)</td>
</tr>
<tr>
<td>Group 2</td>
<td>1.581***</td>
<td>1.262***</td>
<td>1.498***</td>
<td>3.117***</td>
<td>1.160***</td>
</tr>
<tr>
<td></td>
<td>(0.337)</td>
<td>(0.315)</td>
<td>(0.385)</td>
<td>(0.853)</td>
<td>(0.346)</td>
</tr>
<tr>
<td>Group 1</td>
<td>Recent</td>
<td>Retired</td>
<td>In-State</td>
<td>DEM</td>
<td>Larger</td>
</tr>
<tr>
<td>Group 2</td>
<td>Older</td>
<td>Not-retired</td>
<td>Out-State</td>
<td>REP</td>
<td>Smaller</td>
</tr>
<tr>
<td>P-value difference</td>
<td>0.33</td>
<td>0.02</td>
<td>0.82</td>
<td>0.04</td>
<td>0.08</td>
</tr>
<tr>
<td>Baseline, Group 1</td>
<td>20.5</td>
<td>20.8</td>
<td>20</td>
<td>20.5</td>
<td>23.2</td>
</tr>
<tr>
<td>Baseline, Group 2</td>
<td>19.4</td>
<td>19.6</td>
<td>19.4</td>
<td>17</td>
<td>17.9</td>
</tr>
<tr>
<td>Observations</td>
<td>82,820</td>
<td>82,820</td>
<td>82,820</td>
<td>82,820</td>
<td>82,820</td>
</tr>
<tr>
<td>Groups</td>
<td>8,971</td>
<td>8,971</td>
<td>8,971</td>
<td>8,971</td>
<td>8,971</td>
</tr>
<tr>
<td>Model</td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
</tr>
</tbody>
</table>

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors clustered at the group-level. 

Prob. of contributing takes the value 100 if the individual made a positive contribution to a presidential committee during the 2012-cycle and 0 otherwise. \( \log(\frac{N_{\text{own}}}{N_{\text{opp}}}) \) denotes the logarithm of the ratio of the number of contributors to the same party in the same ZIP-3 over the number of contributors to the opposite party. All regressions include individual-level controls, ZIP-3-level controls and group dummies as described in footnote to Table 10. The coefficients on group 1 and group 2 correspond to the interactions between the corresponding group-dummies and \( \log(\frac{N_{\text{own}}}{N_{\text{opp}}}) \). The regressions also control for the corresponding group-dummy. The grouping recent/older divides individuals according to whether they moved before or after April 1, 2010. The baseline for each group are the averages of the dependent variable for each group. The grouping retired/non-retired divides individuals according to whether they declared to be retired or not when they contributed in the 2008 election cycle. The grouping In/Out-State divides individuals according to whether they moved to location within the same state or to a different state with respect to their original location. The grouping DEM/REP divides individuals according to the political party of the 2008 presidential campaign to which they made contributions. The grouping Larger/Smaller divides individuals according to whether the 2008-contribution was larger/smaller than $500. Each p-value correspond to the test of the null hypothesis that the coefficient on \( \log(\frac{N_{\text{own}}}{N_{\text{opp}}}) \) have the same value for both groups. Data on amount contributed and recipient party from FEC public records (see Table 3 for some descriptive statistics of this data).
A.1. Sample Letter: Awareness-Own

Dear John,

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

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

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: Awareness-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:

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

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:

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

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:

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

We will send an updated list of future contributions to some households. Some of your neighbors may receive such a list.

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

Information Dissemination on Campaign Contributions

[www.campaign-information.info](http://www.campaign-information.info)
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. Sample Letter: Outside of the mailing

Outside of the mailing - front

DEAR JOHN:
IMPORTANT INFORMATION ABOUT POLITICAL CAMPAIGN CONTRIBUTIONS

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

Outside of the mailing - back

DEAR JOHN:
IMPORTANT INFORMATION ABOUT POLITICAL CAMPAIGN CONTRIBUTIONS
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)
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 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:
D  Snapshots of the FEC Search Tool

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

**Transaction Query By Individual Contributor**

Search Using the Following Criteria:

Individual Name (Last, First):

Get Listing  Clear Form

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

**Advanced Transaction Query By Individual Contributor**

Search From the Following Criteria:

Individual Name (Last, First):

City:

State (Select as many as appropriate):

- ALL
- ALABAMA
- ALASKA
- AMERICAN SAMOA

Zipcode:

Employer/Occupation:

Amount Range: From  To

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

Date Range: From  To

Types:

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

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

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

**Contributions to Political Committees**

**DOE, JOHN**  
ELIOT, ME 03903  
HOME MAKER

**PAUL, RON**  
VIA RON PAUL 2012 PRESIDENTIAL CAMPAIGN COMMITTEE INC.

12/16/2011  
250.00  
12345678900

The (fake) number 12345678900 has a link to the exact page of the Schedule A-P corresponding to the transaction. The following is a sample:
E Further Details on the Experiment’s Implementation

E.1 Experimental Sample and Subject Pool

A total of 280,456 unique individuals were listed as having made a contribution to a presidential candidate in that time period in the FEC records. This sample was obtained from the FEC’s public records as of April 25, 2012, which includes contributions made until April 1 of that year. This sample of contributors, by definition, excludes individuals contributing less than $200 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.62

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

E.2 Mailing Delivery

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

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

Figure E.1 compares our proxy of delivery date from the USPS tracking data with data on visits to the project’s website. The top panel of the Figure 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. Panel b shows the distribution of new visitors to the website over time and the USPS-based proxy for mail delivery. The two distributions are very similar, although visits to the website seem to have a three to four day lag with respect to the proxy for delivery date. This is consistent with the fact that individuals do not necessarily read the mail the same day they get it. Moreover, the difference in the right tail of the two distributions indicates that visits to the website sometimes occurred weeks after the letters were delivered. This probably corresponds to
individuals who accumulate mail over time, or to those who were absent from their homes for some time. However, the difference between the two distributions may simply correspond to the fact that our proxy for delivery is only a conservative lower bound estimate of the actual date of delivery.

E.3 Feedback from Recipients

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

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

a. Cross-state relationship

b. Time-series relationship

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”).

xvi
## F Summary Statistics

Table F.1: Informational Effects: Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>Sd</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\bar{c}_{\text{own}}$</td>
<td>6.33</td>
<td>4.88</td>
<td>0.00</td>
<td>25.00</td>
</tr>
<tr>
<td>$\bar{c}_{\text{opp}}$</td>
<td>5.92</td>
<td>5.42</td>
<td>0.00</td>
<td>25.00</td>
</tr>
<tr>
<td>$N_{\text{own}}$</td>
<td>5.31</td>
<td>2.22</td>
<td>0.00</td>
<td>9.00</td>
</tr>
<tr>
<td>$\sum c_{\text{own}}$</td>
<td>35.28</td>
<td>32.78</td>
<td>0.00</td>
<td>225.00</td>
</tr>
<tr>
<td>$\sum c_{\text{own}} - \sum c_{\text{opp}}$</td>
<td>11.07</td>
<td>48.97</td>
<td>-220.00</td>
<td>225.00</td>
</tr>
<tr>
<td>$</td>
<td>\sum c_{\text{own}} - \sum c_{\text{opp}}</td>
<td>$</td>
<td>36.70</td>
<td>34.25</td>
</tr>
</tbody>
</table>

Notes: $N = 36,795$. Summary statistics corresponding to the sample of individuals assigned to the List treatment. $\bar{c}_{\text{own}}$ ($\bar{c}_{\text{opp}}$) corresponds to the average contribution among all own-party (opposite-party) contributors in the baseline list, expressed in hundreds of dollars. $N_{\text{own}}$ is the number of own-party contributors in the baseline list. $\sum c_{\text{own}}$ ($\sum c_{\text{opp}}$) is defined as $\bar{c}_{\text{own}}$ ($\bar{c}_{\text{opp}}$) but refers to the sum of contributions instead of the average contribution (also expressed in hundreds of dollars).
<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>2008-cycle</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Contributed to democrat (%)</td>
<td>81.41</td>
<td>38.90</td>
<td>0.00</td>
<td>100.00</td>
</tr>
<tr>
<td>Amount contributed ($)</td>
<td>581.68</td>
<td>748.76</td>
<td>10.00</td>
<td>9200.00</td>
</tr>
<tr>
<td><strong>2012-cycle</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Prob. of contributing (%)</td>
<td>19.79</td>
<td>39.85</td>
<td>0.00</td>
<td>100.00</td>
</tr>
<tr>
<td>Amount contributed ($), if &gt;0</td>
<td>702.34</td>
<td>961.41</td>
<td>3.00</td>
<td>20000.00</td>
</tr>
<tr>
<td>Share of own-party indiv. in zip3</td>
<td>0.62</td>
<td>0.17</td>
<td>0.18</td>
<td>0.93</td>
</tr>
<tr>
<td>$\log\left(\frac{N_{\text{own}}}{N_{\text{opp}}}, \text{zip3-level}\right)$</td>
<td>0.55</td>
<td>0.82</td>
<td>-1.54</td>
<td>2.54</td>
</tr>
<tr>
<td>$\log\left(\frac{N_{\text{own}}}{N_{\text{opp}}}, \text{county-level}\right)$</td>
<td>0.49</td>
<td>0.75</td>
<td>-2.14</td>
<td>2.65</td>
</tr>
<tr>
<td>$\log(N_{\text{own}})$, zip3 level</td>
<td>16.91</td>
<td>1.26</td>
<td>12.30</td>
<td>19.71</td>
</tr>
<tr>
<td>$\log(N_{\text{opp}})$, zip3 level</td>
<td>16.36</td>
<td>1.03</td>
<td>12.30</td>
<td>19.71</td>
</tr>
</tbody>
</table>

Notes: $N = 82,820$. Data on individual contributions for a sample of individuals who contributed to a presidential campaign during the 2008 election cycle and - according to USPS NCOA data - changed residence between January-2009 and March-2011. *Share of own-party individuals in ZIP-3* is the share of contributors in the ZIP-3 that contributed to the same party than the individual. $N_{\text{own}}$ denotes the number of contributors to the own-party and $N_{\text{opp}}$ denotes the number of contributors to the opposite-party.
Figure F.1: Knowledge about Contribution Limits and Contribution Norms (Post-Election Mail-In Survey)

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

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

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

The falsification test consists in comparing contributors who changed residence before the beginning of the election cycle with contributors who changed residence after the end of the election cycle. As an illustration of this falsification test, Figure G.1 shows three possible scenarios. This figure displays the evolution of the strength of political affiliation ($type_i$), the share of own-party contributors in the ZIP-3 ($\tau_{-i}$) and the individual’s own contribution ($c_i$). The two lines denote the hypothetical evolution for a pair of Republican contributors: the red line corresponds to the Republican contributor moving to a more Republican area, and the blue line corresponds to the Republican contributor moving to a less Republican area. Scenario 1 represents the situation in which the correlation between $\tau_{-i}$ and $c_i$ is entirely due to reference group effects. Scenarios 2 and 3 represent situations in which the correlation between $\tau_{-i}$ and $c_i$ could be either partially or entirely spurious (i.e., not related to reference group effects). In scenario 2, the individual moving to the more Republican area has a higher degree of commitment to the Republican party to begin with. In scenario 3, the individual moving to the more Republican area becomes more committed to the Republican Party over time. Note that the three scenarios can rationalize that the individual moving to the more Republican area between $t - 1$ and $t$ contributes more in $t$. However, unlike the scenario of reference group effects, scenarios 2 and 3 also predict more contributions in $t$ for an individual who moved to a more Republican area between $t$ and $t + 1$. 
Scenario 1: Reference group effects

Scenario 2: Systematic difference in levels of political affiliation

Scenario 3: Systematic difference in trends of political affiliation

Notes: Each line corresponds to the evolution of one outcome. $type_i$ denotes the strength of the political affiliation. $\pi_{-i}$ denotes the share of own-party neighbors. $c_i$ denotes the individual’s own contribution. The red line corresponds to a Republican contributor moving to a more Republican area, and the blue line corresponds to a Republican contributor moving to a less Republican area.
H  A Signaling Model of Campaign Contributions

H.1  The Model

The following is a simple model of campaign contributions that captures the party signaling described in Section 3. In this model, individuals, indexed by subscript $i$, can make a discrete contribution denoted by $c_i \in \{-1, 0, 1\}$. $c_i = -1$ means that the individual contributes to the left-wing party, $c_i = 1$ means that the individual contributes to the right-wing party and $c_i = 0$ means that the individual does not contribute to any political party. The discrete nature of contributions is just a convenient simplification to facilitate the tractability of the model. The intuition of the model, however, extends to the case where individuals can make contributions of different amounts. Moreover, even though we are interested specifically in monetary contributions, $c_i$ may also be interpreted as other forms of potentially-observable forms of political participation, such as attending a rally, displaying candidate’s yard signs or simply speaking in favor of a candidate.

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

There are also indirect costs and benefits from making contributions. Individuals interact with neighbors in their reference group. The utility an individual can expect from these interactions is a function of the coincidence or divergence in political affiliations with the neighbors. Political preferences are not directly observable by others, but contributions may be observed. Whether the individual contributed, and the party contributed to, is visible to $i$’s
neighbors with some probability $\nu$, and unobservable with probability $1 - \nu$. Contributions are made prior to the interactions with neighbors.\textsuperscript{63} 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 $P^j_i = P^j (c_i, c_{-i})$ 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 $\delta \left( P^j_i \right)$. 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 $P^R_i$ 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:

$$S_R \mu \delta \left( P^R_i \right) + S_L (1 - \mu) \delta \left( 1 - P^R_i \right)$$

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

$$S_R (1 - \mu) \delta \left( P^R_i \right) + S_L \mu \delta \left( 1 - P^R_i \right)$$

The parameter $\mu \in \left[ \frac{1}{2}, 1 \right]$ captures what we denominate political homophily, the tendency of individuals to associate with other sympathizers of the same political party. The parameter $\mu$ can have one of the following two interpretations (or a combination of the two). First, it may represent differences in the likelihood of meeting a neighbor of each party. Second, it may represent party-based differences in how individuals value the interactions. The case where $\mu = \frac{1}{2}$ is given by a situation where individuals are matched with neighbors regardless of their political preferences, while in the case where $\mu > \frac{1}{2}$ each individual is relatively more likely to interact with neighbors supporting their own political party.\textsuperscript{65} Alternatively, $\mu = \frac{1}{2}$

\textsuperscript{63}An alternative interpretation of the probability parameter $\nu$ 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.

\textsuperscript{64}Note that the individual does not know whether her contribution will be observable to others when deciding about her contributions.

\textsuperscript{65}However, the fact that individuals are more likely to bond with neighbors of the same political party should not be interpreted as an exogenous parameter, i.e., $\mu > \frac{1}{2}$, but instead as part of the indirect costs embedded in $\delta (\cdot)$. That is, revealing oneself as a sympathizer of the opposite party (with respect to the neighbors) is disadvantageous because this results in fewer and/or poorer connections within the reference group.
could mean that individuals have the same valuation of interactions with neighbors from either party, while $\mu > \frac{1}{2}$ could indicate that individuals value interactions with same-party neighbors relatively more.

We make two simplifying assumptions to make the model tractable. First, we assume $\alpha_i$ is uniformly distributed. Second, we assume that $\delta(\cdot)$ is linear. Without any loss of generality, we normalize the intercept of $\delta(\cdot)$ to zero: i.e., $\delta(P) = \gamma \cdot P$. In the signaling equilibrium there will be three groups defined by two thresholds: $\alpha^*_L \in (\alpha, 0)$ and $\alpha^*_R \in (0, \overline{\alpha})$. 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 $\alpha_i \geq \alpha^*_L$ will contribute to the right-wing party. Let $\Omega_R$ ($\Omega_L$) denote a right-wing (left-wing) individual’s utility from interacting with neighbors when her own contribution is unobservable. The utility for a right-wing individual from contributing to her favorite party is:

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

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:

$$v \left[ (S_R - 1 + \mu) \gamma \frac{\min \{\alpha^*_R, \overline{\alpha}\}}{\min \{\alpha^*_R, \overline{\alpha}\} - \max \{\alpha^*_L, \overline{\alpha}\}} + (1 - S_R) (1 - \mu) \gamma \right] + (1 - v) \Omega_R$$

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

$$v \left[ (S_R - \mu) \gamma \frac{\min \{\alpha^*_R, \overline{\alpha}\}}{\min \{\alpha^*_R, \overline{\alpha}\} - \max \{\alpha^*_L, \overline{\alpha}\}} + (1 - S_R) \mu \gamma \right] + (1 - v) \Omega_L$$

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

$$\alpha^*_R = v \gamma (S_R - 1 + \mu) \frac{\min \{\alpha^*_L, \overline{\alpha}\}}{\min \{\alpha^*_R, \overline{\alpha}\} - \max \{\alpha^*_L, \overline{\alpha}\}} + K \quad (1)$$

The analogous expression for a left-wing individuals is:

$$-\alpha^*_L = v \gamma (S_R - \mu) \frac{\min \{\alpha^*_R, \overline{\alpha}\}}{\min \{\alpha^*_R, \overline{\alpha}\} - \max \{\alpha^*_L, \overline{\alpha}\}} + K \quad (2)$$

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Note that we implicitly assume an interior solution.
This system of two equations and two unknowns characterizes the signaling equilibrium. Denote $\alpha^* = \{\alpha^*_L, \alpha^*_R\}$ and let $\Theta = \{\alpha^* : \alpha^*_L \in (\underline{\alpha}, -\frac{K^2}{2}), \alpha^*_R \in (\frac{K^2}{2}, \bar{\alpha})\}$. We will focus on equilibria with $\alpha^* \in \Theta$. The first requirement in $\Theta$ is that the solution is interior, i.e., $\underline{\alpha} < \alpha^*_L < \alpha^*_R < \bar{\alpha}$. The second requirement, $\alpha^*_L < -\frac{K^2}{2} < 0 < \frac{K^2}{2} < \alpha^*_R$, basically restricts the analysis to equilibria in which the mass of non-contributors to each party is above the threshold $\frac{K^2}{2}$. This condition is consistent with the fact that only a small share of individuals contribute to political campaigns. This condition guarantees that the equilibrium effects described below are of second order and therefore do not override the direct effects of changes in the relevant parameters.

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

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

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

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

**Proposition 3.** In any signaling equilibrium from $\Theta$, an increase in visibility ($\upsilon$) 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 $\upsilon$ to have effects of opposite signs on contributions for individuals in two different groups: we should expect a negative effect for those with $S_j < \underline{\alpha}$.

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67 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.

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

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

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

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

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\footnote{If we allowed the scenario with \( \mu < \frac{1}{2} \) 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.}
H.2 Proof of Propositions

H.2.1 Proof of Proposition 1

From Equation 2 we obtain:

\[ \alpha^*_L + \alpha^*_L [K - \alpha^*_R] - (v \gamma (S_R - \mu) + K) \alpha^*_R = 0 \]

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 1:

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

and then define:

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

We thus need to prove that a fixed point of \( f_R(\alpha_R) \) exists and is unique in the domain \( \alpha_R \in \left( \frac{K}{2}, \alpha \right) \). 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 \( g_R (\alpha) < 0 \) to prove existence and uniqueness. Starting with \( g'_R(\alpha_R) \):

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

To prove that \( g'_R(\alpha_R) < 0 \), it is sufficient that:
\[ \frac{\alpha_R}{2} - \frac{K}{2} < \sqrt{\left(\frac{\alpha_R}{2} - \frac{K}{2}\right)^2 + (\nu \gamma (S_R - \mu) + K) \alpha_R} \]

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 \((\nu \gamma (S_R - \mu) + K) > 0\), 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:

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

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

We now need to reproduce the entire analysis for \( \alpha_L \): i.e., we need to prove that a fixed point of \( f_L (\alpha_L) \) exists and is unique in the domain \( \alpha_L \in \left( \bar{\alpha}, \frac{-K}{2} \right) \). From Equation 1 we obtain:

\[
\alpha^*_R = \frac{K}{2} + \frac{\alpha^*_L}{2} + \sqrt{\left(\frac{K}{2} + \frac{\alpha^*_L}{2}\right)^2 - (K - \nu \gamma (S_R - 1 + \mu)) \alpha^*_L}
\]

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

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

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

\[
\Pi = \left\{ \{K, \mu, \alpha, \bar{\alpha}, \nu, \gamma\} : g_R \left( \frac{K}{2} \right) > 0, g_R (\bar{\alpha}) < 0, g_R (\alpha) > 0, g_L \left( \frac{-K}{2} \right) < 0, \right\}
\]

Finally, it is trivial to prove that \( \Pi \) is non-empty by means of an example.
H.2.2 Proof of Proposition 2

Denote \( C_R = \frac{\pi - \alpha_R}{\pi - \alpha} \) as the mass of individuals contributing to the right-wing party and \( C_L = \frac{\alpha_L - \alpha}{\alpha - \alpha_L} \) as the mass of individuals contributing to the left-wing party. It follows that:

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

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:

\[
F(v, \alpha_R^*, \alpha_L^*) = \begin{bmatrix}
\alpha_R^* - v\gamma (S_R - 1 + \mu) \frac{\alpha_R^*}{(\alpha_R^* - \alpha_L^*)^2} - K \\
-\alpha_L^* - v\gamma (S_R - \mu) \frac{\alpha_L^*}{(\alpha_R^* - \alpha_L^*)^2} - K
\end{bmatrix}
\]

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

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

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

By the implicit function theorem, we know that:

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

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

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

Finally, we can replace back in \( \frac{dC_R}{dv} - \frac{dC_L}{dv} \):
\[
\frac{dC_R}{dv} - \frac{dC_L}{dv} = \frac{1}{\alpha - \alpha} \gamma \left[ \frac{\left(-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)\right)\left(S_R - \frac{1}{2}\right) - \left(\mu - \frac{1}{2}\right)\left[\alpha_L^* + \alpha_R^*\right]}{-\left(\alpha_L^* + \frac{K}{2}\right) + \left(\alpha_R^* - \frac{K}{2}\right)} \right]
\]

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

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

Plugging this expression in the previous equation, we obtain:

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

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.

**H.2.3 Proof of Proposition 3**

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

\[
\frac{dC_R}{dv} = -\frac{1}{\alpha - \alpha} \frac{d\alpha_R^*}{dv} = \gamma \frac{S_R - (1 - \mu)}{\alpha - \alpha} - \left(\frac{\alpha_{L}^* + \frac{K}{2}}{\alpha - \alpha}\right) - \left(\frac{\alpha_{R}^* - \frac{K}{2}}{\alpha - \alpha}\right)
\]

\[
\frac{dC_L}{dv} = \frac{1}{\alpha - \alpha} \frac{d\alpha_L^*}{dv} = \gamma \frac{S_L - (1 - \mu)}{\alpha - \alpha} - \left(\frac{\alpha_{L}^* + \frac{K}{2}}{\alpha - \alpha}\right) - \left(\frac{\alpha_{R}^* - \frac{K}{2}}{\alpha - \alpha}\right)
\]

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 \(S_j < 1 - \mu\).