Partisan Interactions: Evidence from a Field Experiment in the United States

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We conducted a field experiment to study social influences on partisan political participation. We sent letters to 92,000 contributors during the 2012 presidential election campaign. We randomized features of the letters and measured the effects of these variations on the recipients' subsequent contributions. We find that making an individual's contributions more visible to her neighbors increases the contributions of supporters of the local majority party and decreases those of supporters of the minority party. Individuals contribute more when they perceive higher average contributions from own-party supporters in their area and contribute less if there is a higher share of own-party contributors.

I. Introduction

Many forms of political participation, including making campaign contributions, attending rallies, and commenting on social media, reveal the

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party or cause that an individual supports. As these activities take place in a domain of social observation, they are subject to social effects. In this paper, we use a novel field experiment to study social influences on partisan participation.

Federal law dictates that campaign committees must report the identity of individuals who contribute over $200 to the Federal Election Commission (FEC) along with personal information. The FEC makes these contribution records not only publicly available but, more importantly, easily accessible online. The FEC website provides up-to-date disaggregated information about contributors, including full name and address, occupation, employers, contribution amount and date, and the party and candidate to which the contribution was made. Moreover, the FEC website offers an online tool that allows visitors to search for contributors on the basis of each of these characteristics (e.g., name, address).[1] The fact that an individual's contributions are observable by others and that an individual can observe (and thus be influenced by) the contributions of others makes campaign contributions in the United States an excellent context for studying partisan interactions.

In May 2012, we sent letters with individualized information related to campaign contributions to a sample of 91,998 individuals from all US states who, according to the FEC records, had made a contribution to a presidential campaign between April 2011 and April 2012. Sample individuals contributed about $500 on average during this period. The letters sent to these subjects were identical except for subtle variations in the information displayed that were randomly assigned to nondeceptively manipulate perceptions of the observability of campaign contributions and of the nature of neighbors' contributions. We used FEC records to measure the effect of the letter's content on the subject's subsequent contributions during the 6 months between the mailing delivery and the end of the 2012 presidential campaign.

The first treatment arm was designed to test for what we call the *conformity channel*, which is the hypothesis that disclosing one's party affiliation through political participation results in better treatment by supporters of one's own party and in harsher treatment by supporters of the opposite party. This type of letter provided information about the public nature of campaign contribution records and how to access the FEC's online search tool. We randomly assigned these recipients to one of two subtreatments. Individuals in one subtreatment received a letter indicating that theirs was

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[1] Online app. A.9 provides more details about the FEC's online search tool.

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the only household in the area randomly chosen to be sent a letter of this type. Individuals in the other subtreatment received a letter that was identical to the first except that it indicated that their household along with other households in the area had been randomly selected to receive a letter of this type. The second subtreatment differs from the first in that other individuals in the area also received information about how to access the FEC records. We interpret the difference between these two subtreatments as a change in the recipient's perceived probability that her neighbors will observe her future contributions.

The second treatment arm was designed to test for what we call the *comparison channel*, which is the hypothesis that an individual's political participation depends on the observed political participation of her peers. We sent a letter that listed the semi-anonymized names, the amounts contributed, and the parties contributed to by nine contributors from the recipient's area of residence. We randomly selected those nine individuals from the 30 contributors nearest to the recipient's address. This selection was based on a series of parameters that we varied randomly to create nondeceptive exogenous variation along multiple dimensions of the list of contributors, such as the average amount contributed. We interpret the differences between these letters as differences in the recipient's perception of the contributions of others.

We find strong evidence for the conformity channel. When contributions are made more visible to others, an individual's contribution increases if a majority of her neighbors support her party but decreases if a majority support the opposite party. These effects are not only statistically significant but also large in magnitude. Our preferred treatment-on-the-treated estimates, which adjust for the possibility that mail is discarded, suggest that in highly polarized areas, with 75 percent of neighbors supporting one party, our higher-visibility treatment reduced the amount contributed by minority supporters by 41 percent (relative to the baseline amount) and increased the amount contributed by majority supporters by 15 percent.

We also find evidence for the comparison channel. Recipients contributed more when neighbors of the same party were shown to contribute higher average amounts. This effect is significant both statistically and economically. Our preferred treatment-on-the-treated estimate indicates that for each additional $100 in the average amount contributed by own-party neighbors, the recipient's own contribution increased by $13.60. Individuals did not contribute significantly more, however, when neighbors from the opposite party were shown to contribute higher average amounts. This finding is consistent with theories of identity (Akerlof and Kranton 2000), according to which individuals form social norms based on the behavior of peers with whom they identify (in this case, neighbors who support the same party). In addition to the information on the average amount contributed, we also examined whether individuals care about the distribution of contributors across parties. We find that individuals contributed less

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when there was a higher number of own-party relative to opposite-party contributors. This effect goes in the opposite direction of social norms but is consistent with free-riding behavior.

We perform some simple back-of-the-envelope calculations to quantify the effects of the conformity and comparison channels on a measure of geographic polarization: the standard deviation of the Democratic share of contributors across zip codes. First, we analyze the counterfactual scenario in which individuals cannot identify the contributions of others—that is, without conformity effects. We find that eliminating conformity effects would decrease geographic polarization by 20 percent. Second, we analyze a counterfactual scenario in which individuals ignore the contribution behavior of others—that is, without comparison effects. The counterfactual analysis suggests that eliminating the comparison channel would increase geographic polarization by only 1.1 percent.

Our paper relates to a series of recent studies on political participation. Regarding voting turnout, the literature has long emphasized the importance of social pressure (Knack 1992) and social norms (Riker and Ordeshook 1968). In a seminal contribution, Gerber, Green, and Larimer (2008) conducted a field experiment in which, close to election day, individuals were sent letters with lists of neighbors and their previous voting turnout history. The letters also promised to publicize the recipient's future voting behavior to her neighbors. The authors found that these letters had a large positive effect on subsequent turnout, which they interpret as arising from some combination of social norms and social pressure. Unlike most other forms of political participation, though, the act of voting does not in itself reveal the party or cause that the individual supports. As a result, Gerber et al. (2008) and other related studies (Funk 2010; DellaVigna et al. 2017) do not present any evidence about how individuals interact with peers from the same and the opposite party. Relatedly, Gerber et al. (2013) show that some individuals do not vote because they do not trust the privacy of voting, which suggests that they may not want to disclose their partisan affiliation. To the best of our knowledge, our paper is the first to provide field experimental evidence about partisan interactions.

Also to the best of our knowledge, few papers present field experimental evidence on campaign contributions. A recent exception is the study by Augenblick and Cunha (2015), who conducted an experiment in the United States with randomly assigned solicitation messages to a group of Democrat contributors, referencing the past contribution behavior of members of the Democratic or Republican Party. We make an additional methodological contribution by developing an experimental design to disentangle the effects of being observed by others from the effects of observing the behavior of others.

Our findings are also informative for the ongoing debate about the reasons behind individual contributions to political campaigns. Individ-

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ual contributions represent a large portion of campaign funding in the United States, accounting for approximately 80 percent of the $1.7 billion raised in the 2012 presidential race. We interpret our evidence as consistent with the hypothesis from Ansolabehere, de Figueiredo, and Snyder (2003) that individual contributions are largely motivated by personal and social factors rather than just by the desire to obtain direct private benefits from the political system.

Our findings on social incentives are also related to the literature on prosocial behavior. With respect to the conformity channel, there is evidence that social pressure is effective for inducing prosocial behavior. Individuals are also more likely to give money to a charity when they cannot avoid the solicitor (DellaVigna et al. 2012), and academics review journal articles faster when their review times are made public (Chetty, Saez, and Sandor 2014).[2] In terms of political participation, individuals are more likely to vote when their participation in elections is observable to others (Gerber et al. 2008; DellaVigna et al. 2017). Related to the comparison channel, there is evidence that students are more likely to donate to a university when told that a higher share of other students donated in the past (Frey and Meier 2004). There is also evidence that households' energy consumption changes when they are provided with information about the consumption of neighbors (Allcott 2011). The fact that social pressure and social comparisons are also relevant factors for individuals' campaign contribution decisions suggests that making campaign contributions can be considered, to a significant extent, as another form of prosocial behavior.

The paper is organized as follows. Section II discusses the relevant hypotheses and the experimental design used to test them. Section III presents the data sources and the implementation of the field experiment. Section IV presents the main results. Section V provides a counterfactual analysis of the conformity and comparison effects on geographic polarization. Section VI presents conclusions.

II. Hypotheses and Experimental Design

A. The Conformity Channel

1. Hypotheses

In their social interactions, individuals care about how they are perceived by others and thus behave strategically to affect those social perceptions (Benabou and Tirole 2006, 2011). In this paper, we consider

[2] One traditional interpretation for these findings is that individuals use these forms of prosocial behavior to signal their altruism to others (Benabou and Tirole 2006, 2011; Andreoni and Bernheim 2009; Ali and Lin 2013).

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the possibility that peers care about the political party that the individual supports. The existence of partisan favoritism has been long recognized (e.g., Campbell et al. 1960) and is consistent with evidence that individuals report more sympathy with supporters of their own political party (e.g., Iyengar, Sood, and Lelkes 2012; Iyengar and Westwood 2015).

Most social-signaling models deal with actions that are unambiguously perceived as good by all peers, in which case higher visibility is predicted to increase prosocial behavior. Under partisan favoritism, however, contributors interact with supporters of both parties and thus face a dual audience. As a result, the effects of more visible contributions will depend on the partisan composition of the peers with whom the individual interacts. If an individual interacts only with supporters of her party, an increase in the visibility of contributions would be expected to make contributing more attractive, because of the positive effects on interactions with like-minded peers. If, on the contrary, the individual interacts exclusively with supporters of the opposite party, then higher visibility would make contributions less attractive, because of the resulting negative effects in social interactions. Online appendix F provides a simple signaling model in the spirit of Bernheim (1994) that formalizes this intuition.

2. Experimental Design

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

As background, figure 1 reports the results of a survey described in more detail in Section III.C. Figure 1*A* shows that contributors are aware that contributions are public: 86 percent of respondents agreed with the statement that contributions were a matter of public record.[3] This high awareness is not surprising, given that campaign committees are required to collect detailed information from individual contributors and to explain that this information is required by the FEC's disclosure policy.[4] By contrast, figure 1*B* shows that many contributors believe that their neighbors do not know about the disclosure policy: 40 percent of respondents believed that the majority of their neighbors thought that

[3] These figures correspond to responses from subjects in our sample of contributors who did not receive any letter from us (besides the survey). Strictly speaking, some contribution records are a matter of public record while others are not (e.g., records for contributions of $200 or less are not reported to the FEC). The wording in the survey was very general as we wanted to measure general awareness of the public nature of this information rather than test the subjects on the details of the regulation. Appendix D presents more details about the survey instrument, including a facsimile with the exact wording of this question and of response options.

[4] This awareness is also consistent with the internet browsing data discussed in app. E indicating that the FEC's search tool and other websites based on its information are widely accessed.

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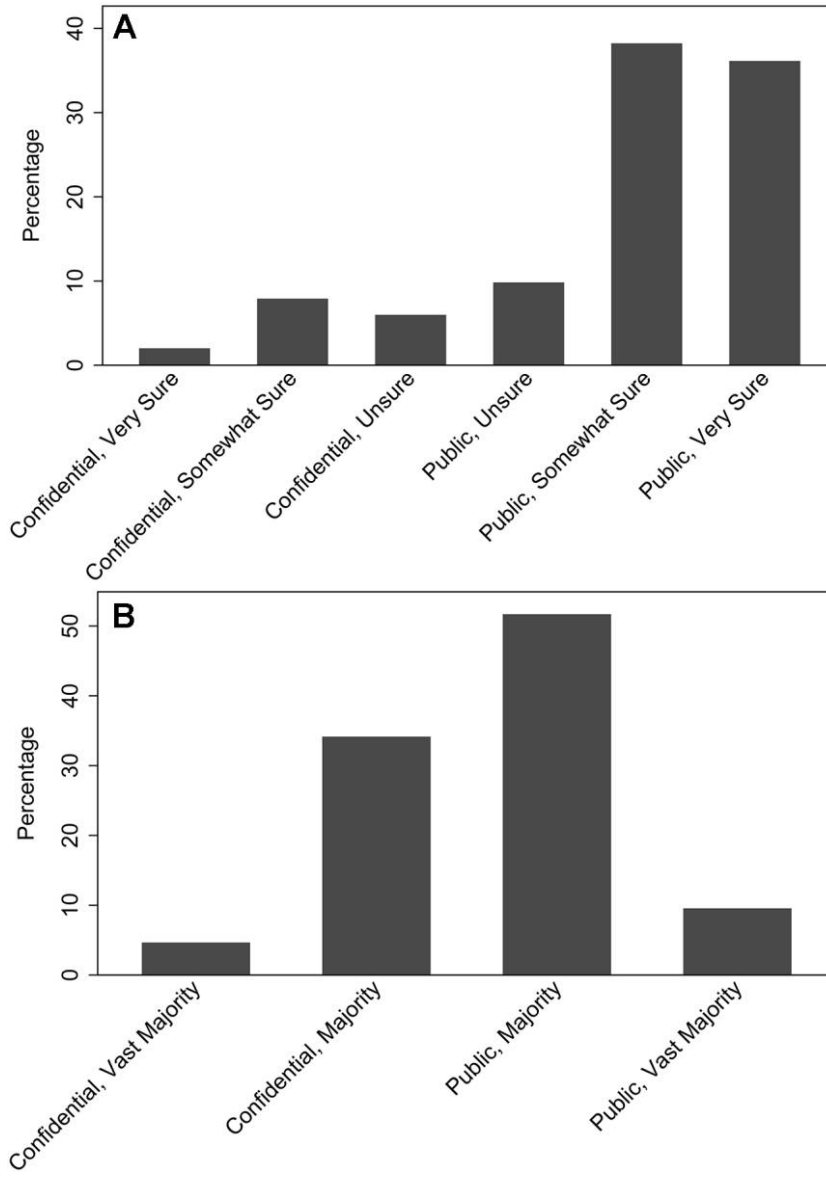

FIG. 1.—Contributors' perception of the confidentiality of contributions (postelection mail-in survey, No-Letter group). *A*, Perception of whether contribution records are confidential/public (N = 3,068). Answers to questions 4 and 5 from the questionnaire are combined. *B*, Perception of the proportion of neighbors who believe that contribution records are confidential/public (N = 3,018). The figure is based on question 7 from the questionnaire. For a copy of the questionnaire, see appendix D.

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contribution records were confidential. These results suggest that informing a contributor's neighbors about how to access contribution records may affect her perception of the visibility of her contributions.

We designed a type of letter, which we labeled Website, for the purpose of providing information on how to use the FEC website to search for individual contributors (for a sample, see app. A.1). This flyer-like mail piece consisted of a single sheet of paper that was folded and sealed to form an envelope (see the sample in app. A.6). As with all the other types of letters used in this experiment, we identified the research purpose of the communication: "This letter is part of a study of political campaign contributions made by individuals which is being conducted by researchers at Harvard University" (when the field experiment was conducted, Ricardo Perez-Truglia was affiliated with Harvard University). The letters included the URL of the project's website, which provided basic information on the project as well as contact information for the research team and for Harvard's Institutional Review Board (see app. A.8 for the content of this site). The main purpose of the site was to provide interested subjects with contextual information about our study and to clear up any doubts about its legitimacy by placing emphasis on its academic and nonpartisan nature.

This Website letter contained a list that included the name of the recipient and the five contributors nearest to the recipient's address, along with the party and the amount given by each of those listed.[5] The recipient of the letter was always the second name on the list; the full name of the recipient was used (other contributors were identified only by first name and initial of last name) and highlighted on the list. This short list of contributors was included to draw the recipient's attention to the content of the letter and to reinforce the perception that contribution records are indeed publicly available by providing verifiable information. The second paragraph of the letter identified the FEC as the source of the information and explained that the name, address, and other details about contributions were readily accessible online. That paragraph also included the URL of the FEC's search tool along with the statement that the website could be used "to see which candidates or political parties your neighbors, friends, family and co-workers are contributing to."

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

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

[5] The median pairwise distance between the recipients and their five closest neighbors who were contributors was 0.35 mile.

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

This information was nondeceptive: we conducted the randomization such that those receiving the Website-Self letter were the only ones in their area to receive the letter, while there were multiple recipients of the Website-Neighbors letter within an area.[6] As a result, relative to recipients of the Website-Self letters, recipients of Website-Neighbors letters should have considered it more likely that their neighbors would use the FEC search tool to monitor their future contributions. In other words, we interpret the difference between these two subtreatments as a difference in the perceived visibility of the recipients' contribution.

To allow falsification tests for our key hypothesis, we added a treatment arm called the Placebo letter, which had the same format as the Website letter but presented only standard regulatory information about contribution limits, taken verbatim from the FEC's regulations. We did not expect this information to have an effect on contributions because these regulations are generally well known and, most importantly, because contribution limits were not binding for virtually all of the individuals in our subject pool.[7]

3. Econometric Framework

To estimate the effect of higher visibility, we proceeded as follows. Let Y_i be a measure of the recipient's posttreatment contributions. The econometric specification is

$$Y_i = \beta_0 \cdot \text{HigherVisibility}_i + \beta_1 \cdot \text{HigherVisibility}_i \cdot \text{ShareOwn-Party}_i + \alpha \cdot \text{ShareOwn-Party}_i + \delta X_i + \varepsilon_i, \quad (1)$$

where Higher Visibility is a dummy variable that takes the value of one if the subject was assigned to the subtreatment Website-Neighbors and the value of zero if she was assigned to the subtreatment Website-Self. Share Own-Party is the proportion of the recipient's neighbors who support the recipient's party, measured as the share of contributors in the recipient's three-digit zip code (ZIP-3) who contributed to the subjects's party

[6] Specifically, we divided the United States into disjointed geographical areas of similar population. These areas were randomly assigned to one of two groups. In areas assigned to the Website-Self treatment, exactly one household (randomly selected among all households in the area in our FEC database of contributors) was sent a letter of this type. In the areas assigned to the Website-Neighbors treatment, we randomly selected three households to be sent these letters, assigning more areas to the Website-Self than to the Website-Neighbors type so that the expected number of households receiving each subtreatment was the same.

[7] See, e.g., fig. D.1.a in the appendix.

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over the three previous presidential campaigns. The marginal effect of the higher visibility treatment in a given area is given by $\beta_0 + \beta_1 \cdot \text{ShareOwn-Party}_i$. The conformity channel predicts that higher visibility will discourage participation in areas where a vast majority of neighbors support the opposite party (i.e., $\beta_0 < 0$) but will encourage participation in areas where the vast majority of neighbors support the same party (i.e., $\beta_0 + \beta_1 > 0$). Finally, X_i is a group of control variables such as the pretreatment contributions made by the recipient (the inclusion of these variables makes little difference for the results, as reported in app. C).

It must be noted that Share Own-Party was not randomized as part of the experiment. Thus, we rely on the assumption that there are no other characteristics of the area of residence that affect the marginal effect of Higher Visibility$_i$ and are correlated with Share Own-Party$_i$, that is, $E[\text{HigherVisibility}_i \cdot \text{ShareOwn-Party}_i \cdot \varepsilon_i] = 0$. We provide a series of robustness checks related to this assumption in the empirical section below.

B. The Comparison Channel

1. Hypotheses

According to social norms theory, individuals are more motivated to engage in prosocial behavior when they perceive peers are acting prosocially (Cialdini 1984; Akerlof and Kranton 2000), especially if the individual feels identified with these peers. Applying this framework to campaign contributions, we hypothesize that individuals will contribute more if other individuals of the same party make a larger contribution or have a higher contribution rate.

Learning that others of the same party donate at a high rate could also act to reduce an individual's contributions as a result of free riding. For instance, because of diminishing marginal returns, an individual's marginal contribution has a smaller effect on the campaign when others give more to her own party, thus making contributing less attractive.[8]

2. Experimental Design

We devised a treatment arm called the List letter that provided information about the behavior of other contributors in the recipient's area. A sample letter is presented in appendix A.3. The letter contained the same contextual information as the Website letter regarding the purpose

[8] And a similar crowding out may also occur if individuals make campaign contributions with the intention to buy access to politicians (e.g., Kalla and Broockman 2016).

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of the communication (research), the project's website, and contact information. The bulk of the List letter, though, consisted of information about presidential campaign contributions made by the recipient and by nine other individuals from the recipient's area of residence from April 1, 2011, to April 1, 2012.[9] The information included, in table form, first name, last name initials, party, and the amount contributed by each of those listed.[10] The recipient's own contribution and name (highlighted) were at the top of the list in order to draw the recipient's attention and to demonstrate the credibility of information. To facilitate the assimilation of the information, contributions were ordered from highest to lowest amounts, first for Democratic candidates and then for Republican candidates.

We used an algorithm that randomly selected the nine contributors to be included in the table in order to introduce experimental variation in the lists presented to the recipients. We first obtained the geolocation for all individuals listed in our baseline FEC database. Then, for each contributor i assigned a List letter, we identified the 30 closest contributors, L_i, which we defined to be the individual's neighbors.[11] The nine neighbors included in the table were selected from L_i first by ordering the contributors in L_i according to a composite index and then by picking the top nine contributors from the ordered list. The value of the composite index for a given neighbor j was a function of j's party, Party(j), of the amount contributed by j during the preceding 12-month period, Amount(j), and a set of constants, $\epsilon_i(j)$:

$$\text{Index}_i(j) = \theta_i^D \cdot 1[\text{Party}(j) = \text{DEM}] + \theta_i^A \cdot \text{Amount}(j) + \epsilon_i(j). \quad (2)$$

The parameters $\{\theta_i^D, \theta_i^A\}$ are the recipient-specific weights assigned to each of those dimensions. The vector ϵ_i (which was randomly generated) dictates the order of the 30 neighbors in the baseline (i.e., where the two weights are set to zero). The list of the top nine neighbors is a function of

[9] The median pairwise distance between the recipient and the nine neighboring contributors was 1.2 miles.

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

[11] The list L_i is constructed on the basis of pairwise distances as the crow flies. These neighboring contributors were selected from all FEC records, not only from our selected subject pool. Only 0.08 percent of early contributors had simultaneously made contributions over $200 to the Obama campaign and to at least one of the Republican presidential candidates. For the sake of simplicity, we excluded them from L_i.

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those parameters, denoted by $g(L_i; \theta_i^D, \theta_i^A; \epsilon_i)$. The baseline list refers to the list of nine neighbors given by setting the two weights to zero, $g(L_i; 0, 0; \epsilon_i)$. The weight assigned to the political party component was randomly selected from three possible values: $\theta_i^D = -c_p$, 0, and c_p, with $c_p > 0$. Similarly, the weight assigned to the contribution amount was randomly selected from three possible values: $\theta_i^A = -c_a$, 0, and c_a, with $c_a > 0$. We calibrated the distribution of the parameter values so that the average characteristics of the lists were not biased relative to the baseline. We used this index rather than simply selecting list members at random to ensure sufficient variation in the statistics of interest.[12] Note that the information provided was not deceptive insofar as the letters stated that the table included nine of the recipient's neighbors and, given our definition of neighbors, this claim always holds true.

This composite index induced exogenous variation in the contribution patterns shown in each List letter. Table 1 presents three possible lists of nine neighbors generated by different combinations of the parameter weights. Panel A presents the baseline list ($\theta_i^D = 0$, $\theta_i^A = 0$). Panel B presents the list obtained when the Democratic weight is assigned a negative value ($\theta_i^D < 0$, $\theta_i^A = 0$): two of the Democratic contributors that appear on the baseline list are replaced by two Republican contributors. In panel C, the weight on the amount is assigned a positive value ($\theta_i^D = 0$, $\theta_i^A > 0$): two of the Democrat contributors that appear on the baseline list are replaced by two other Democrats who contributed higher amounts, and one of the Republican contributors is replaced by one Republican who contributed a higher amount. Note that the List letter did not contain messages that would prime individuals to pay attention to a particular dimension of the information provided, such as the average contribution amount.[13]

3. Econometric Framework

Our identification strategy does not rely on a comparison of posttreatment contributions by individuals who received the List letters and those who did not receive any letter. It relies, rather, on comparison between

[12] As a measure of how much exogenous variation was induced, the correlation between the mean amount contributed in the actual table sent to the recipient and the mean amount in the baseline table is about .75. See app. B.3 for additional details about the randomization of neighbors included in the List letter.

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

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TABLE 1
IDENTIFICATION OF COMPARISON CHANNEL: SAMPLE TREATMENT LISTS GENERATED WITH DIFFERENT PARAMETER VALUES

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

NOTE.—Example of how the algorithm generates different lists of nine neighbors from a given sample of the recipient's 30 closest contributing neighbors. See Sec. IV.B for a detailed description of the algorithm.

individuals who received List letters. A simple example conveys intuition on the estimation of these effects. Imagine that we sent some contributors a table with an average contribution of $500 while we sent others a table with an average contribution of $600. In that case, we could estimate a regression of the posttreatment contributions on a variable that takes the value of zero for the recipients randomly assigned to the $500 letter and the value of 100 for the recipients assigned to the $600 letter. If the estimated coefficient on that variable is 0.1, we would infer that each additional dollar in average contributions shown in the letter caused the recipient to contribute an additional $0.10.

We can generalize the above framework for the case in which we simultaneously randomize multiple dimensions of the information contained in the letter. Let $f_i(\cdot)$ represent some (possibly vector-valued) statistic of the list shown to individual i, such as the mean contribution to the recipient's own party. Recall that Y_i denotes the recipient's posttreatment contributions. The econometric specification is

$$Y_i = \beta \cdot \Delta f_i + \delta X_i + \varepsilon_i, \tag{3}$$

where $\Delta f_i \equiv f_i(g(L_i; \theta_i^D, \theta_i^A; \varepsilon_i)) - f_i(g(L_i; 0, 0; \varepsilon_i))$ are the values of the statistics in the list shown to the individual compared to the values that would have resulted if she had been assigned the baseline list (e.g., the mean contribution in the list sent minus the mean contribution in the baseline list). We wish to interpret the coefficient on Δf_i as the causal effect of the f_i included in the list on the recipient's posttreatment contributions.

The orthogonality assumption $E[\Delta f_i \cdot \varepsilon_i] = 0$, required for causal inference, implies that there are no omitted dimensions of the informa-

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tion about the list that can affect the recipient's contributions that are correlated with the characteristics included in the regression equation. For instance, recipients may care about both the mean contribution amount and the maximum contribution in the list, which are likely to be correlated. Including only the mean in the regression equation would yield a biased estimate, because its coefficient would pick up part of the effects of the maximum contribution. An advantage of our research design is that, as econometricians, we observe the same information as our subjects, and thus we can include in the analysis additional dimensions of the list that we believe might be driving the results.

III. Data Sources and Implementation of the Field Experiment

A. Subject Pool and Data Sources

Our subject pool was based on a subsample of the FEC contribution records, specifically, some 280,456 individuals who had made over $200 in contributions to a presidential campaign committee from April 1, 2011, to April 1, 2012, drawn from the online FEC records as of April 25, 2012. While the FEC's records are remarkably comprehensive, there were some instances of missing or inconsistent information. Since the number of individuals in this initial sample was substantially higher than the number of subjects needed for our experiment, we adopted a conservative approach and limited the subject pool to those individuals for whom the highest-quality information was available (e.g., quality of address information). We applied a number of additional arbitrary criteria, such as excluding contributors from Washington, DC, and those geographically isolated from other contributors (for more details, see app. B). After applying these criteria, our final subject pool consisted of 191,832 individuals.[14] Appendix B provides descriptive statistics and shows that the observable characteristics of our subject pool are similar to those of the universe of individuals who contributed during the 2012 presidential campaign.

Of the 191,832 contributors in the subject pool, 91,998 were randomly assigned to be sent a letter: 36,773 were sent a Website letter, 36,795 a List letter, and 18,430 a Placebo letter. Within each treatment arm, we randomly assigned them to the subtreatments described in the previous section (e.g., Website-Self and Website-Neighbors). We refer to the 99,834 in-

[14] The sample also excludes 1,002 individuals who were sent letters later deemed undeliverable or redirected by the US Postal Service (the results are robust to the inclusion of these observations, as reported in app. C). We took several measures to clean the address information in the FEC database, including geocoding, cross-checking an individual's information across different records, and matching the data with the USPS National Change of Address database.

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dividuals who were not assigned a letter as the No-Letter group. The random assignment was conducted at the household level and was stratified at the ZIP-3 level.[15] Appendix B shows that the treatment groups are balanced in observable pretreatment characteristics, as we would expect from the random assignment to treatments. Finally, since contributors to the Rand Paul primary (constituting 12 percent of the original sample) made virtually no contributions in the posttreatment period to the Romney presidential campaign, our baseline results exclude this group. The results are robust to inclusion of this group in the analysis, as shown in appendix C.1.3.

B. Timing of the Experiment and Outcomes of Interest

The letters were sent on May 6, 2012, 4 days after the Republican National Committee declared Mitt Romney the party's presumptive nominee. The outcome variable was simplified by sending the letters once each party had a single presidential candidate, which meant we did not have to compare contributions from the same individual to different candidates. Unless stated otherwise, the outcomes of interest throughout our study are the individual campaign contributions made to the Obama or Romney committee from the estimated mail delivery until the official end of the election cycle, that is, December 31, 2012.[16] We label these "posttreatment" contributions. The "pretreatment" contributions, which were used in falsification tests, correspond to total contributions made between April 1, 2011, and the date when the letters were delivered.

In the pretreatment period, 52 percent of individuals in the No-Letter group contributed to Obama and the remaining to Republican candidates. The average amount contributed during the pretreatment period was $524. During the posttreatment period, 48.9 percent of our subjects made at least one contribution. For those who made contributions during the posttreatment period, the average amount contributed was $587. For more details about the contribution patterns of subjects, see appendix B.2. When the dependent variable in the regressions presented below is the amount of posttreatment contributions, we use an interval regression model to take into account the censored nature of this outcome: because of the disclosure threshold, for any Republican subject who did not make a pretreatment contribution to Romney, the absence of recorded contributions to Romney in the posttreatment period implies that she

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

[16] In practice, there were virtually no contributions shortly after election day, November 6, 2012. For individuals in the No-Letter group, we defined the date dividing pre- and posttreatment contributions as the median date when other letters were delivered in their five-digit zip code (see app. B for details).

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must have contributed to Romney anywhere between $0 and $200. As shown in appendix C, the results are very similar using other regression models.

C. *Postelection Mail-In Survey*

Data for the key outcome variable, the recipient's posttreatment contributions, were obtained from the FEC administrative records. Additionally, we conducted a mail-in survey with a subsample of our subjects with two goals. The first goal was to provide some descriptive evidence to complement the experimental results. The second goal was to adjust and aid the interpretation of the magnitudes of the effects identified by our mailing experiment, which are attenuated by the fact that not all subjects read the letter that was sent to them.

The survey included five questions about knowledge of campaign finance law and a final subjective question about how much an individual should contribute to presidential campaigns. The envelope contained a letter, a survey questionnaire, and a prepaid business reply envelope. Recipients were asked to fill out the survey and mail it back in the envelope provided.[17] We sent the envelopes on December 6, 2012, 1 month after the date of the 2012 presidential election, because we did not want any of the information contained in the survey mail piece to contaminate the effects of the letters sent in the experiment. The intended recipients, 44,380 in total, were a random sample of individuals from the No-Letter and Website groups. We received 9,414 responses, which implies a response rate of 21.21 percent (this response rate was statistically indistinguishable between subjects from the No-Letter and the Website letter treatment groups). Appendix D presents the survey instrument and provides further details on this survey and its response rate.

IV. Experimental Results

A. *The Conformity Channel*

Figure 2 depicts the effect of Higher Visibility on the probability of making a contribution in the posttreatment period for different values of Share Own-Party, in the spirit of a partial regression plot. Each dot corresponds to one decile of the distribution of Share Own-Party, with its position in the horizontal axis corresponding to the mean value of Share Own-Party in that decile. Thus, the horizontal dispersion of the dots reflects the distribution of Share Own-Party. For each dot, the position in the

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

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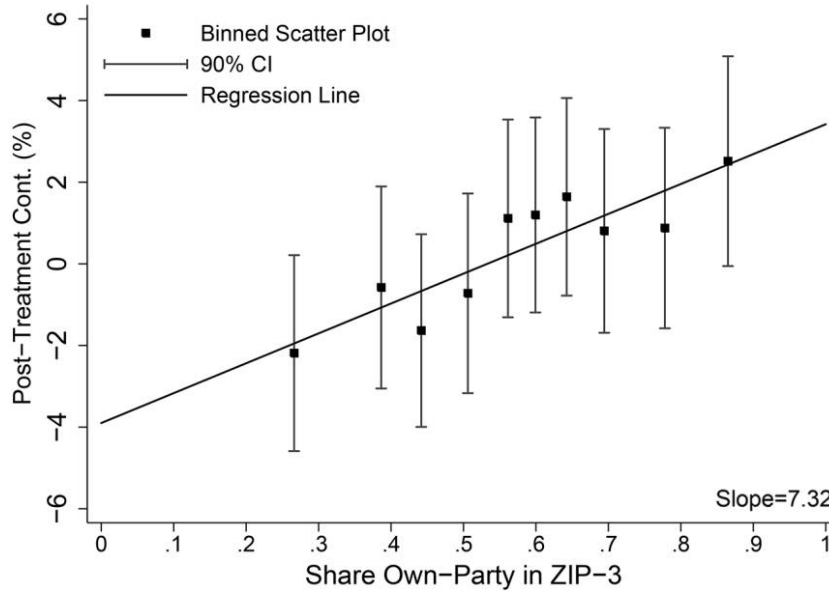

FIG. 2.—Effect of higher visibility on the probability that the recipient makes a posttreatment contribution, by partisan composition of the recipient's area; $N = 32{,}070$. Observations are taken from subjects assigned to the Website letter. The dots in the figure correspond to a binned scatter plot representation of the partial regression plot. Each dot corresponds to one decile of the distribution of Share Own-Party, with its position in the horizontal axis corresponding to the mean value of Share Own-Party in that decile. For each dot, the position in the vertical axis corresponds to the average effect of the Higher Visibility intervention on the probability of making posttreatment contributions within the corresponding decile of Share Own-Party, with confidence intervals based on heteroskedasticity-robust standard errors. The regression line corresponds to the linear relationship between the two variables. The binned scatter plot was estimated from a regression of an indicator of whether the individual made a posttreatment contribution on a dummy for whether the subject was assigned the Website-Neighbors subtreatment, dummies for the deciles of Share Own-Party, the interaction between the two latter sets of dummies, and a set of individual-level controls (for more details about the regression, see the note to table 2).

vertical axis corresponds to the average effect of the Higher Visibility intervention on the probability of making posttreatment contributions within the corresponding decile of Share Own-Party (see the notes to fig. 2 for estimation details). The results are consistent with the prediction of the conformity channel: when the majority of a subject's neighbors support her party, higher visibility increases the probability that she will make a contribution; when supporters are evenly split between the two parties, higher visibility has no effect on the likelihood of contributing; and when most of a subject's neighbors support the opposite party, higher visibility decreases the probability of a subject making a contribution. Moreover, this partial regression plot suggests that the effect of Higher Visibility in-

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creases roughly linearly with Share Own-Party, which validates the linear specification used in the rest of the analysis.[18]

Table 2 presents the baseline results in regression form, along with a number of robustness checks. The results in column 1 present the effects of the higher visibility treatment on the amount contributed during the posttreatment period. The negative coefficient on Higher Visibility indicates that the treatment reduces participation in areas where everyone supports the party that the recipient opposes, while the significant positive coefficient on the interaction between Higher Visibility and Share Own-Party indicates that the effect of the higher visibility treatment is more positive (or less negative) when Share Own-Party is higher. These coefficients can be used to estimate the effects of the higher visibility treatment in areas with different values of Share Own-Party. For instance, consider a ZIP-3 in which 75 percent of contributors support the majority party. Our results indicate that, in such areas, higher visibility would reduce by about $53 the amount contributed by supporters of the minority party and increase by about $19 the amount contributed by supporters of the majority party. These effects are not only statistically but also economically significant. The effects of $53 and $19 constitute, respectively, 9.0 percent and 3.2 percent of the average amount contributed by subjects who made further contributions during the posttreatment period ($587).

Column 7 in table 2 reproduces the results from column 1 but with the probability of making at least one posttreatment contribution, rather than the amount contributed, as the dependent variable. The sign and statistical significance of the coefficients are consistent with the results from column 1. In areas where 75 percent of neighbors support the majority party, the higher visibility treatment reduces the probability of contributing by 1.86 percentage points among supporters of the minority party and increases the probability of contributing by 1.47 percentage points among supporters of the majority party. These effects of 1.86 and 1.47 percentage points represent, respectively, 3.3 percent and 2.6 percent of the baseline contribution rate of 55.7 percent.

A simple and straightforward way to check the randomness of treatment assignment is to compute the "effects" of our experiment on pretreatment, rather than posttreatment, contributions. Column 8 in table 2 presents the results from this falsification test. As expected, the higher visibility treatment had no "effect" on pretreatment contributions: the estimates of the coefficients on Higher Visibility and on its interaction with Share Own-Party are very close to zero, are not statistically significant, and are precisely estimated.

[18] Formally, we test the joint hypothesis that all of the binned scatter points from fig. 2 are equal to the corresponding points in the regression line (p-value = .996).

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TABLE 2
EXPERIMENTAL EVIDENCE ON THE CONFORMITY CHANNEL

	POSTTREATMENT CONTRIBUTIONS							PRETREATMENT
	$ (1)	$ (2)	$ (3)	$ (4)	$ (5)	$ (6)	$P(\$ > 0)$ (7)	$ (8)
Higher visibility (Website-Neighbors − Website-Self)	−89.07**	−88.48**	−89.62**		−84.38***	−35.40	−3.53**	−3.32
	(37.22)	(36.11)	(37.49)		(27.72)	(31.03)	(1.61)	(21.77)
Interaction with:								
Share own-party in ZIP-3	143.50**	142.09**	144.43**		149.04***	40.84	6.67**	3.37
	(60.30)	(59.12)	(60.78)		(44.81)	(50.03)	(2.71)	(34.16)
Share own-race in ZIP-3		−29.09						
		(48.02)						
Share low-income in ZIP-3			26.61					
			(130.63)					
Placebo (Placebo – No-Letter)				8.58				
				(29.00)				
Interaction with:								
Share own-party in ZIP-3				−8.99				
				(46.75)				
Regression method	Interval	Interval	Interval	Interval	Interval	Interval	OLS	OLS
Subperiod					≤ Sept. 1	> Sept. 1		
Mean outcome	$329	$329	$329	$329	$163	$166	56%	$522
Observations	32,070	32,070	32,070	103,367	32,070	32,070	32,070	32,070

NOTE.—Heteroskedasticity-robust standard errors are in parentheses. Observations are from subjects assigned to Website letters. The dependent variable in cols. 1–4 is the amount contributed posttreatment and in cols. 5 and 6 the amount contributed posttreatment before and after September 1, 2012, respectively; in col. 7, the value is 100 if the individual made at least one posttreatment contribution and zero otherwise; col. 8 presents the dollar amount contributed pretreatment. Higher Visibility takes the value one if the subject received a Website-Neighbors rather than a Website-Self letter. Share Own-Party stands for the share of own-party contributors to presidential campaigns in the ZIP-3 during the three previous presidential election cycles. Share Own-Race refers to the share of individuals of the same race (white or nonwhite) as the recipient in the same ZIP-3. Share Low-Income refers to the share of income-earning adults with an annual income below $30,000 (US census, 2010). Share Own-Race (Share Low-Income) is normalized to have zero mean so that the coefficient on Higher Visibility corresponds to individuals with Share Own-Party = 0 and average Share Own-Race (Share Low-Income). All the regressions except the one in col. 8 include as controls the levels of all variables that are interacted with Higher Visibility, the time it took for delivery of the mail piece, and a set of variables with pretreatment contributions to each candidate. Mean outcome denotes the mean of the outcome variable.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

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One potential concern with our findings is that the heterogeneous effects of higher visibility by partisan alignment may reflect heterogeneity in other area characteristics that are correlated with partisan alignment. Columns 2 and 3 of table 2 present some robustness checks. Column 2 includes the interaction between Higher Visibility and the share of individuals of the same race as the recipient's in her ZIP-3 (based on the categories white and nonwhite). The results suggest that there is no significant heterogeneity in the effect of Higher Visibility with respect to Share Own-Race: the coefficient on the interaction (−29.09, standard error [SE] 48.02) is close to zero, is not statistically significant, and is more precisely estimated than (and statistically significantly different from) the interaction with Share Own-Party.[19] In turn, column 3 includes an interaction between Higher Visibility and Share Low-Income, defined as the share of low-income households in the recipient's area. As expected, the coefficient on the interaction with Share Low-Income is close to zero and is not statistically significant. Furthermore, we find similar results when we include interactions with other characteristics of the recipient's area of residence, such as the share of college graduates and married individuals (results reported in app. C).

The share of Democrats (or Republicans) in a given ZIP-3 is significantly correlated to a number of characteristics of the area's population such as average income, race, and education. If our analysis focused on the interaction between Higher Visibility and Share Democrat, there would then be a potential omitted variable bias from these other ZIP-3 characteristics. However, our analysis is based on the alignment of the subject with respect to the political composition of her area. This difference is subtle but important, because the same ZIP-3 characteristics that are correlated to Share Democrat (or Share Republican) are only weakly correlated with the alignment variable, Share Own-Party, leaving less room for potential omitted variable biases. For instance, the correlation between Share Democrat and Share Low-Income is .275 (p-value < .01), whereas the correlation between Share Own-Party and Share Low-Income is only .033 (p-value < .01).

As an additional falsification test, column 4 of table 2 presents the results from a specification in which we replace the Higher Visibility with the Placebo treatment indicator. As expected, there is no significant heterogeneity with respect to Share Own-Party in the response to the Placebo letter: the coefficient on the interaction between Placebo and Share Own-Party (−8.99, SE 46.75) is close to zero, is not statistically significant, is pre-

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

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cisely estimated, and also is statistically different from the corresponding coefficient from column 1 on the interaction between Higher Visibility and Share Own-Party (143.50, SE 60.30).

We can also explore the timing of the effects of higher visibility. Columns 5 and 6 of table 2 show the effects of higher visibility on the posttreatment amount contributed before and after September 1, 2012, respectively. The effects of the higher visibility treatment faded over time: the coefficient on the interaction between Higher Visibility and Share Own-Party decreases by 73 percent (i.e., from 149.04 to 40.84) from the first to the second half of the posttreatment period, and this difference is marginally statistically significant (p-value $< .10$).

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

While the impact of our higher visibility treatment is consistent with the presence of conformity effects, some alternative interpretations of our results are worthy of discussion. The first is that individuals may use campaign contributions to send signals about characteristics that are not of a partisan nature—for instance, to signal wealth, generosity, or level of civic engagement. The second is that individuals may give more if they feel observed by neighbors because they believe that others will follow their lead and, in turn, contribute more themselves. However, neither of these accounts can explain our finding of a negative effect of higher visibility on contributions for supporters of the local minority party.

The results provide estimates of the effect of having been mailed a letter with certain information, which we denominate the intention to treat (ITT) effect. To assess the importance of higher visibility would require estimating the effect of reading the letter, which we denominate the treatment on the treated (TOT) effect. The ITT effects can be scaled up to TOT effects using the inverse of the reading rate r (i.e., the proportion of recipients who actually read the letters we sent): $\text{TOT} = (1/r)\text{ITT}$. A substantial share of experimental subjects—probably a majority—may not have read the letters we sent them: our mailing was sent in the middle of the presidential campaign, when potential voters, especially those who had made contributions before, were being flooded by large amounts of mailings soliciting campaign contributions and providing information about the candidates and the election. While we attempted to make our mail piece stand out, so did the candidates' campaign committees. In particular, for cost reasons, our mail piece consisted of a folded flyer, which tend to stand out less than regular envelopes.

For our statistical power calculations, we consulted mass-marketing experts who provided us with estimates for our mail piece's expected reading rate. These estimates ranged from 10 percent to 25 percent, which

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explains why we used such a large sample for conducting the experiment. Indeed, this range of estimates is between the conservative lower and upper bounds given by statistics about unsolicited mail and the number of visits to the project's website.[20] To get a more precise and objective measurement of the reading rate, we turn to our postelection mail-in survey.

Figure 3 compares the distribution of beliefs about the public nature of contribution records between survey respondents who were selected not to receive any letter (the No-Letter group) and recipients of our Website letters. As expected, respondents who had received a Website letter were significantly less likely to report being unsure about the public nature of contribution records. More precisely, the share of respondents who were unsure about the public nature of contribution declined from 19.2 percentage points in the No-Letter group to 15.8 percentage points in the Website group. Assuming that a subject who had read the letter would always report certainty about the disclosure policy, that difference of 21.5 percent implies a reading rate of $r = 0.215$ (with a 90 percent confidence interval of 0.146–0.284). Reassuringly, this estimate of the reading rate is within the range of estimates provided by our mass-mailing experts.[21]

We can use the estimated reading rate to scale up the magnitude of the effects of higher visibility. Our results indicated that, in an area where 75 percent of the population supports one party, the higher visibility treatment induced a drop in the amount contributed posttreatment of 9.0 percent of the mean amount for recipients supporting the local minority party and an increase of 3.2 percent for recipients supporting the local majority. The scale-up factor of 4.6 (i.e., $1/0.215$) implied by the reading rate, however, indicates that the TOT effects were substantially larger: -41 percent (i.e., -9.0 percent $\times$ 4.6) and 15 percent (i.e., 3.2 percent $\times$ 4.6), respectively.[22] Even with a reading rate twice as high, the effects of higher visibility would still be very large. Appendix D.2 presents a formal discussion

[20] The US Environmental Protection Agency indicates that 44 percent of unsolicited mail is discarded before being opened, which provides a conservative upper bound on the reading rate (source: US Environmental Protection Agency, "EPA Junk Mail Reduction," June 28, 2006). On the other hand, visitors to our project's website account for 5 percent of the letters we sent, which in turn provides a conservative lower bound for the reading rate.

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

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

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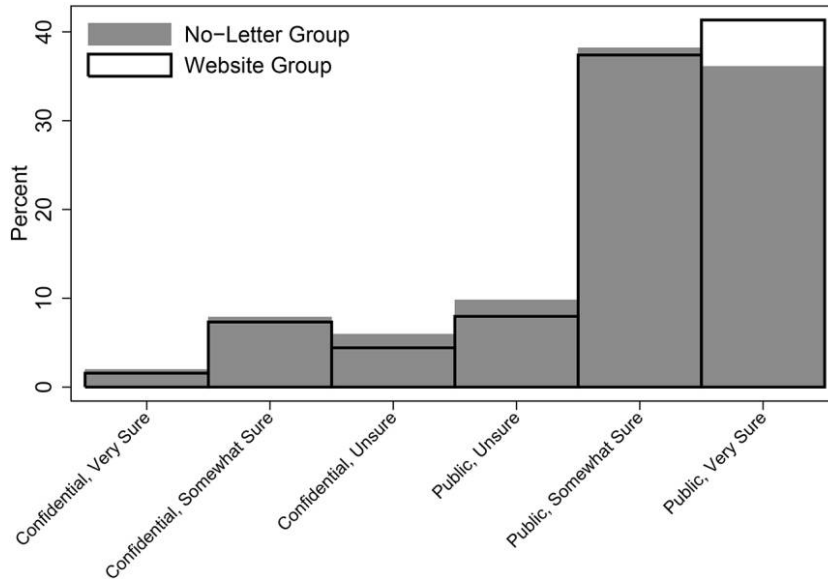

FIG. 3.—Effect of the website letter on the belief that contribution records are confidential/public (postelection mail-in survey); $N = 9{,}414$. Histograms of responses to the postelection mail-in survey. No-Letter corresponds to respondents who did not receive any letter during the experimental stage, while the Website group corresponds to respondents who received a Website-Self or a Website-Neighbors letter. This measure of perception of the public nature of contribution records combines the answer to a first question about disclosure policy and the answer to a second question about the respondent's confidence in that first answer (questions 4 and 5 from the questionnaire in app. A.7, respectively).

of the implicit assumptions in this exercise, and it discusses some qualifications, robustness checks, and alternative estimators.

B. The Comparison Channel

The evidence about the comparison channel is based on the sample of subjects who were sent letters of the List type. The estimation is given by equation (3), which consists of regressing the posttreatment contributions on the characteristics of the table of contributors included in the letter: the average amount contributed to the recipient's own party ($\bar{c}_{\text{own}}$) and to the opposite party ($\bar{c}_{\text{opp}}$), as well as the number of contributors to the recipient's party on the list (N_{own}).

Figure 4 presents the evidence in graphical form, based on the same type of partial regression plots used in the previous section. The dots in the two figures correspond to binned scatter plot representations of the partial regression plots. Each dot corresponds to one quintile of the distribution of the horizontal axis variable, with its position in the horizontal

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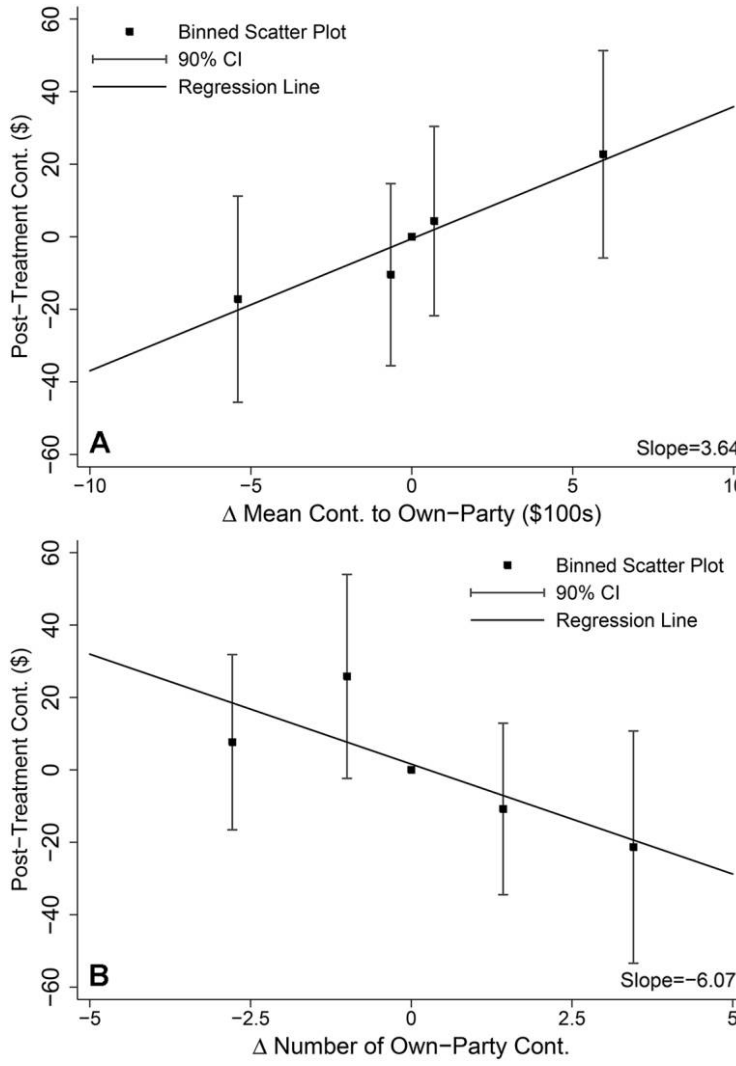

FIG. 4.—Effect of information from the List letter on posttreatment contribution amount; $N = 36{,}795$. *A*, Effect of mean contributed amount own-party. *B*, Effect of number of own-party contributions. Binned scatter plots of effects of information on the posttreatment contribution amount. Regressions include the usual set of individual-level controls (see the notes to table 2). Additionally, the regression from panel A controls for $\bar{c}_{\text{opp}}$ and N_{own}, while the regression from panel B controls for $\bar{c}_{\text{own}}$ and $\bar{c}_{\text{opp}}$. Confidence intervals are based on heteroskedasticity-robust standard errors.

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axis corresponding to the mean value of the variable in that quintile. For each dot, the position in the vertical axis corresponds to the average effect of the intervention on the posttreatment contribution amount. The regression line corresponds to the linear relationship between the two variables. The binned scatter plot was estimated from an interval regression of the posttreatment amounts contributed on a set of dummies corresponding to the quintiles of the variables on the *x*-axis, with the coefficients for the middle categories normalized to zero—more specifically, corresponding to the difference between the value of this variable computed with the list sent to the recipient and the corresponding value computed in the baseline list. Figure 4*A* corresponds to the effect of $\bar{c}_{own}$, holding constant $\bar{c}_{opp}$ and N_{own}. The evidence suggests that recipients contribute more the higher the mean of contributions from supporters of their own party. Figure 4*B* corresponds to the effect of N_{own}, holding constant $\bar{c}_{own}$ and $\bar{c}_{opp}$. This evidence suggests that individuals contribute less when the list we mailed had a larger number of contributors to their own party. Furthermore, these results suggest that a linear specification is a good approximation to the relationship between contributions and both $\bar{c}_{own}$ and N_{own}.[23]

Table 3 presents further regression results. The specification in column 1 includes as independent variables the average amount contributed to the recipient's own party ($\bar{c}_{own}$) and the average amount contributed to her opposite party ($\bar{c}_{opp}$). These independent variables were defined in hundreds of dollars. The coefficient on $\bar{c}_{own}$ indicates that for each \$100 increase in this variable, there is a statistically significant increase in the recipient's own contributions of about \$2.95 (p-value < .05). While we cannot discard other interpretations, this evidence is consistent with models of social norms, which predict that individuals contribute more if they perceive that similar individuals (i.e., geographically close individuals supporting the same party) contributed, on average, higher amounts. In contrast, the coefficient on the contributions of neighbors who support the opposite party indicates that an increase of \$100 in $\bar{c}_{opp}$ has an effect on the subject's contribution of just \$0.06 (p-value > .10), which is both statistically and economically insignificant. Moreover, the difference between the two coefficients is marginally statistically different from zero (p-value = .096). This finding is also consistent with identity theories (Akerlof and Kranton 2000), according to which individuals do not follow the behavior of peers with whom they do not identify.

Like conformity effects, these comparison effects are ITT estimates, since we do not know which recipients actually read the letters. According to the

[23] Formally, we test the joint hypothesis that all of the binned scatter points from fig. 4*A* (4*B*) are equal to the corresponding points in the regression line: p-value = .967 (p-value = .634).

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TABLE 3
EVIDENCE ON THE COMPARISON CHANNEL

	Posttreatment Contributions					Pretreatment
	$ (1)	$ (2)	$ (3)	$ (4)	$P(\$ > 0)$ (5)	$ (6)
Average amount contributed by own-party contributors ($\bar{c}_{own}$)	2.95** (1.47)	3.20** (1.47)	1.75 (1.22)	2.48** (1.17)	.05 (.06)	.90 (.91)
Average amount contributed by opposite-party contributors ($\bar{c}_{opp}$)	.06 (.92)	−.40 (.96)	−.01 (.74)	−.41 (.78)	−.05 (.04)	.06 (.58)
Number of own-party contributors (N_{own})		−5.44* (2.86)	−6.19*** (2.24)	−.47 (2.30)	−.16 (.13)	1.59 (1.75)
Regression method	Interval	Interval	Interval	Interval	OLS	OLS
Subperiod			≤ Sept. 1	> Sept. 1		
Mean outcome	$338	$338	$168	$171	56%	$527

NOTE.—N = 31,996. Heteroskedasticity-robust standard errors are in parentheses. Observations are from subjects assigned to the List letter. The dependent variable in cols. 1 and 2 is the amount contributed during the posttreatment period. The dependent variable in col. 3 takes the value of 100 if the individual made at least one posttreatment contribution and zero otherwise. The dependent variable in col. 4 is the dollar amount contributed during the pretreatment period. All the independent variables except N_{own} are expressed in hundreds of dollars (i.e., the estimates correspond to effects of $100 changes in the independent variables). The term $\bar{c}_{own}$ (conversely, $\bar{c}_{opp}$) corresponds to the average contribution of all the individuals in the letter's table who contributed to the recipient's own (conversely, other) party. The term N_{own} is the number of individuals in the table who contributed to the recipient's party. These independent variables are included in the regression as the difference between the value computed with the list sent to the recipient and the corresponding value computed in the baseline list. The outcome variables in cols. 3 and 4 correspond to two disjoint sets of the posttreatment period: before and after September 1, 2012. See online table B.4 for descriptive statistics for all these independent variables. All the regressions except for the one in col. 6 include the usual control variables: the time it took for delivery of the mail piece and a set of variables with pretreatment contributions to each candidate. Mean outcome corresponds to the average of the outcome variable.

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

calculations in Section IV.A, the TOT effect may be 4.6 times greater than the ITT effect. This would imply that for each $100 increase in $\bar{c}_{own}$, the recipients who actually read the letter increased their contributions by $13.57 (i.e., 2.95×4.6).

To provide complementary evidence on the economic significance of social norms, we included a question in the postelection survey intended to quantify the respondent's perception of the contribution norm. This question asked how much an individual earning an average income should contribute to a presidential campaign (question 8 in the survey's

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questionnaire, presented in app. A.7). By matching the responses to this question to the FEC records, we can measure the relationship between the perceived social norm and the actual contributions made by respondents during the 2012 presidential campaign. Figure 5 depicts this relationship for responses from individuals in the No-Letter group. As expected, there is a significant positive relationship between the perceived contribution norm and actual contributions: a $100 increase in the perceived norm is associated with an extra $11.21 ($p$-value < .01) in contributions. Of course, this observational evidence is subject to a number of identification challenges. With that caveat in mind, this auxiliary result suggests that the effects of social norms in a nonexperimental setting are of the same order of magnitude as those suggested by the experimental evidence.

Column 2 of table 3 presents the results from a specification that includes as an independent variable the number of individuals on the list

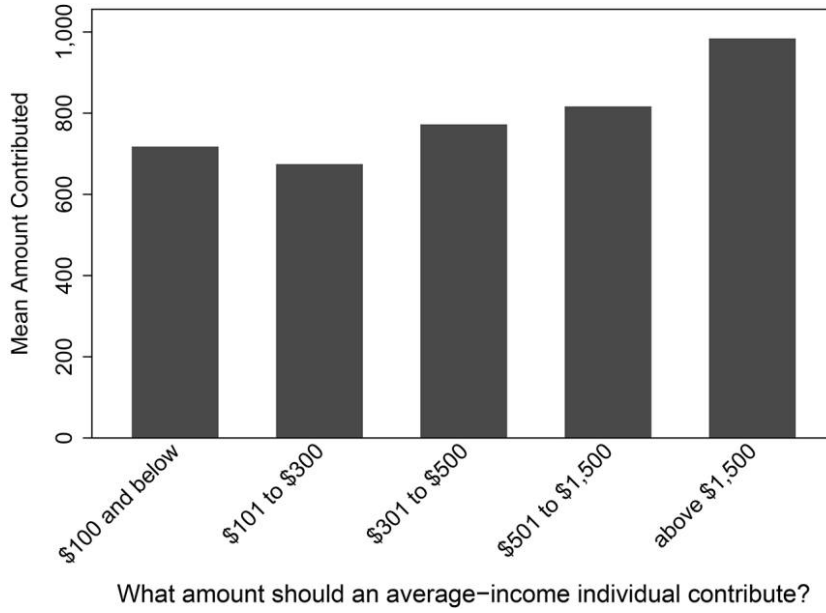

FIG. 5.—Relationship between self-reported contribution norm and actual amount contributed (postelection mail-in survey); $N = 3{,}018$. The figure is based on a combination of responses to the postelection mail-in survey from subjects in the No-Letter group and data on those respondents' contributions during the entire 2012 presidential campaign cycle (from FEC records). The horizontal axis represents the quintiles of the distribution of responses to the survey question about how much individuals "should" contribute to a presidential campaign (question 8 from the questionnaire in app. A.7). The vertical axis represents the average amount contributed by respondents during the presidential election cycle.

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who contributed to the recipient's party (N_{own}), in addition to the average amounts contributed to the recipient's own and to the opposite party, $\bar{c}_{own}$ and $\bar{c}_{opp}$. Social norms theory predicts that a higher value of N_{own} should increase the recipient's contribution because individuals feel pressured to behave like the majority. The negative and statistically significant coefficient on N_{own} in column 2 suggests that, to the contrary, the effect operates in the opposite direction, which could be interpreted as a form of free riding. For each additional individual supporting the same party on the list, the recipient reduced the amount of her contribution by \$5.44 (p-value < .10). The magnitude of this effect is equivalent to the effect of decreasing the mean contribution of own-party neighbors by \$170.

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

We can quantify the effects of the information about contribution patterns on the extensive margin of contributions as well. The specification in column 5 of table 3 is the same as in column 2, with the only difference that the dependent variable is the probability of making at least one posttreatment contribution. The relevant coefficients from column 5 are small in magnitude and not statistically significant, indicating no statistical evidence of comparison effects on the extensive margin.

Columns 3 and 4 in table 3 consider the effects on the amount contributed during the two posttreatment subperiods: before and after September 1, 2012. The estimates suggest that $\bar{c}_{own}$ had a lasting effect: the coefficients are similar for the two subperiods (1.75 and 2.48), and their difference is not statistically significant. The effect of N_{own}, however, lasted only for the first half of the posttreatment period: the coefficient on N_{own} is statistically significant during the first subperiod (−6.19, p-value < .01) but close to zero and not statistically significant during the second half of the posttreatment period (−0.47, p-value > .10), and the difference between the two periods is statistically significant. One potential explanation for this finding is that, as the election neared, the recipients obtained new information about the total contributions to the two presidential campaigns that overrode the information provided in our letter. Indeed, information about the total contributions raised by both campaigns was periodically reported on and discussed in the media during the election cycle.

Appendix C presents a number of additional robustness and specification checks, such as showing that the results are very similar with alterna-

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tive clustering of standard errors and when using Tobit or Poisson regressions instead of the interval regressions.

V. Counterfactual Analysis of the Conformity and Comparison Effects on Geographic Polarization

A. *Implications of the Conformity Channel*

In this section, we present some back-of-the-envelope calculations of the counterfactual distribution of contributions in a scenario with no conformity effects, based on the experimental estimates.

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

$$P_z^k = \tilde{P}_z^k \left[1 + \gamma_1 \cdot v \cdot \left(\frac{\tilde{P}_z^k}{\sum_k \tilde{P}_z^k} - \gamma_0 \right) \right] \quad \forall\ i, z, k, \tag{4}$$

where the parameter $\gamma_1 > 0$ represents the intensity of conformity effects, and the parameter $\gamma_0 \in [0, 1]$ represents their degree of symmetry.[24] The symmetric case, $\gamma_0 = \frac{1}{2}$, corresponds to the patterns we observe in the data: when neighbors are evenly split between the two parties, the conformity channel does not affect contributions; but in areas with more uneven distributions of political preferences, the conformity channel increases contributions to the majority party and decreases contributions to the minority party.

Let P and $\tilde{P}$ denote the vectors with all P_z^k's and $\tilde{P}_z^k$'s. Given $\{P, \gamma_0, \gamma_1 \cdot v\}$, we obtain the counterfactual probabilities $\tilde{P}$ by numerically solving the system of nonlinear equations given by (4). Thus, this counterfactual analysis simply requires estimates for $\{P, \gamma_0, \gamma_1 \cdot v\}$ (note that we need only the product $\gamma_1 \cdot v$, and not each of these parameters separately).

We use the vector of observed shares of contributors in each ZIP-3 during the 2012 presidential election as the estimate of P, and we estimate the values of γ_0 and $\gamma_1 \cdot v$ using the experimental findings. The value of γ_0 corresponds to the value of Share Own-Party for which an increase in

[24] More precisely, we should be using $\tilde{P}_{-i,z}^k$, the probability of contributions excluding i's own, instead of $\tilde{P}_z^k$. This approximation error is virtually zero because there are hundreds of thousands of individuals in each ZIP-3 area. We are also implicitly assuming that the parameters are bounded such that the resulting probabilities are between zero and one.

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visibility would not affect the probability of contributing.[25] The estimates from column 7 of table 2 imply an estimate of $\gamma_0 = 0.53$ (i.e., $-3.53 + 6.67 \cdot 0.53 = 0$). In turn, the parameter γ_1 corresponds to the effect of visibility on contributions between the extreme cases of areas with all own-party neighbors and all opposite-party neighbors. However, our experimental results correspond to the effects of an intervention that increased visibility by some unknown degree, Δv, rather than from zero to one. Thus, we need to take the coefficient on the interaction between Higher Visibility and Share Own-Party and scale it up by dividing it by Δv. The results from column 7 of table 2 imply

$$\gamma_1 = \frac{6.67}{56 \cdot 0.215 \cdot \Delta v} = \frac{0.55}{\Delta v}.$$

In turn, this implies $\gamma_1 \cdot v = 0.55 \cdot (v/\Delta v)$. Thus, the only remaining unknown to complete the counterfactual analysis is $\Delta v/v$, that is, the proportional increase in visibility that resulted from our higher visibility treatment. We do not have a direct measure of $\Delta v/v$ nor of either of its components. Instead, we present some baseline results based on the case of $\Delta v/v = 1$ (i.e., our intervention doubled the visibility of contributions among neighbors) and discuss the results under alternative assumptions.[26]

Figure 6*A* presents the actual and counterfactual distributions of contributors as a function of the share of contributors to the Democratic Party. The solid bars represent the histogram of the actual shares of Democrat contributors across ZIP-3s. The dispersion of this variable is intrinsically related to the degree of geographic clustering of contributors (i.e., the extent to which active Democrats are located near other active Democrats), which is also known as geographic polarization.[27] The main driver of this geographic polarization of contributors is the sorting of individuals into areas with a higher share of like-minded peers. Additionally, once individuals are sorted into geographical areas, the conformity effects can exacerbate polarization by increasing the participation

[25] Note that our experimental estimates correspond to the probability of making a posttreatment contribution conditional on having contributed during the first half of the election (the pretreatment period). Instead, this counterfactual analysis is based on the unconditional probability of making a contribution. This exercise thus relies on the assumption that, in proportional terms, the magnitude of the conformity effects is similar between these conditional and unconditional probabilities.

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

[27] For a formal discussion of this relationship, see Perez-Truglia (forthcoming). For more information on polarization measurement, see Gentzkow, Shapiro, and Taddy (2015).

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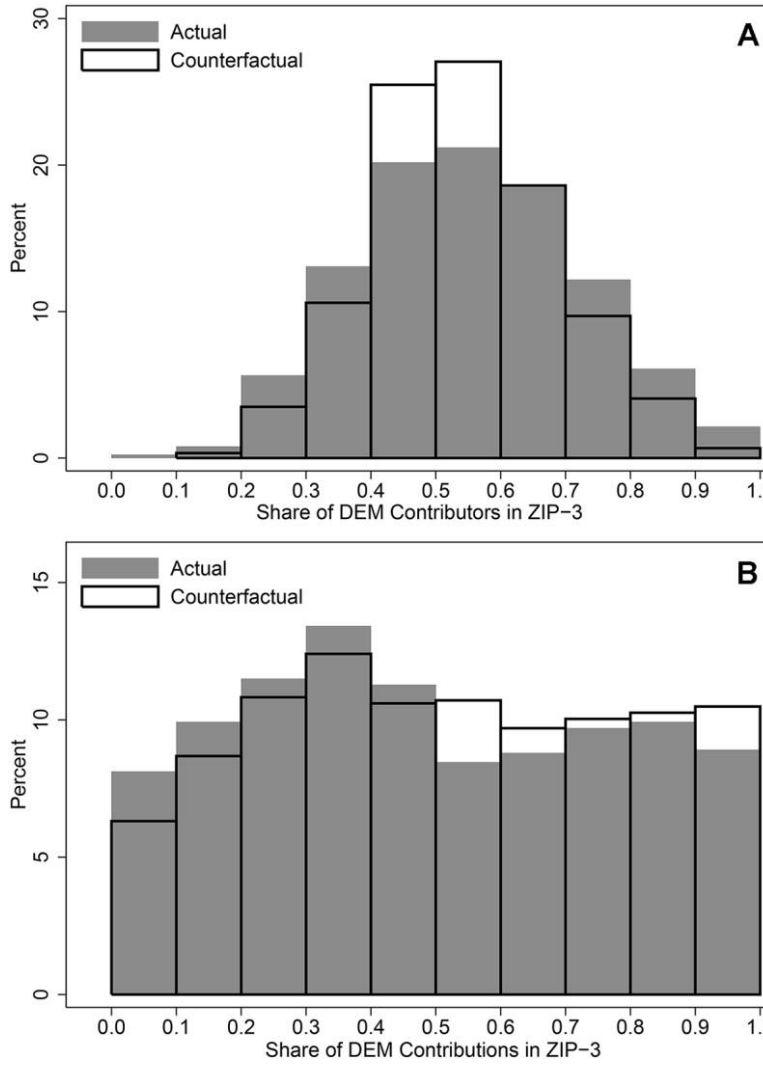

FIG. 6.—Counterfactual analysis of the comparison and conformity effects on geographic polarization. *A*, Conformity channel ($N = 31{,}996$). *B*, Comparison channel ($N = 32{,}070$). In panel A, the solid bars correspond to the actual distribution of the share of Democratic contributors across ZIP-3 areas during the 2012 presidential campaign cycle, and the hollow bars correspond to the counterfactual distribution in a scenario with no conformity effects. In panel B, the solid bars correspond to the actual distribution of the share of Democratic contributions across ZIP-3 areas during the 2012 presidential campaign cycle, and the hollow bars correspond to the counterfactual distribution in a scenario with no comparison effects.

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of supporters of the local majority and reducing the participation of the local minority.

The hollow bars from figure 6*A* correspond to the distribution of contributors in the counterfactual scenario with no conformity effects, assuming $\Delta v/v = 1$. The mean of the distribution is roughly the same (0.55) in the factual and counterfactual scenarios. However, the dispersion of the actual distribution (standard deviation of 0.170) is significantly larger than that of the counterfactual scenario with no conformity effects (standard deviation of 0.142). In other words, the conformity channel exacerbates the geographic polarization by 20 percent. This effect of conformity is on the same order of magnitude as found with observational data: on the basis of an event study of geographically mobile contributors, Perez-Truglia (forthcoming) finds that conformity effects increase geographic polarization by 27 percent.

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

B. Implications of the Comparison Channel

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

Let $C_{i,z}^k$ be the contribution from individual i in ZIP-3 area $z \in \{1, 2, \ldots, Z\}$ to a presidential candidate of party $k \in \{\text{DEM, REP}\}$, and let $\tilde{C}_{i,z}^k$ be i's corresponding contribution in the counterfactual scenario with no comparison effects. Let N_z^k be the number of contributors from ZIP-3 area z to the presidential candidate of party k: on the basis of our experimental results, we assume that the number of contributors remains unchanged with or without comparison effects. We can parameterize the comparison effects by means of the following equation:

$$C_{i,z}^k = \tilde{C}_{i,z}^k + \delta_1 \frac{\sum_i \tilde{C}_{i,z}^k}{N_z^k} + \delta_2 \frac{N_z^k}{\sum_k N_z^k} \quad \forall\ i, z, k, \tag{5}$$

where we assume the parameter δ_1 to be positive, since our results indicated that individuals contribute higher amounts when other contributors

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from the same party and ZIP-3 contribute higher amounts. The parameter δ_2, in turn, is assumed to be negative, since our experimental results indicated that individuals contribute lower amounts if there is a higher share of own-party contributors in the same ZIP-3.[28] Let $\overline{C_z^k}$ and $\overline{\tilde{C}_z^k}$ denote average contributions in the actual and counterfactual scenarios.[29] We can average equation (5) over i and solve for $\overline{\tilde{C}_z^k}$:

$$\overline{\tilde{C}_z^k} = \frac{1}{1+\delta_1}\overline{C_z^k} - \frac{\delta_2}{1+\delta_1}\frac{N_z^k}{\sum_k N_z^k} \quad \forall\ z, k. \tag{6}$$

Let C, N, and $\tilde{C}$ denote the vectors with the average contributions and number of contributors in all ZIP-3s. We can obtain the counterfactual distribution of contributions $\tilde{C}$ by replacing estimates of $\{C, N, \delta_1, \delta_2\}$ into equation (6). Analogously to the previous section, C and N are estimated by means of the average contribution amounts and the number of contributors in each ZIP-3 during the 2012 presidential election. We can also obtain δ_1 and δ_2 from our experimental results. On the basis of the results presented in column 2 of table 3, we use

$$\delta_1 = \frac{3.20}{100} \cdot \frac{1}{0.215} = 0.149$$

and

$$\delta_2 = \frac{-5.44 \cdot 9}{0.215} = -2.811.$$

Figure 6*B* depicts the distribution of the actual share of Democratic contributions and the counterfactual distribution with no conformity effects. The results suggest that the comparison effects induce a slightly smaller share of Democratic contributions: the actual mean share of Democratic contributions is slightly lower (49.2 percent) than in the counterfactual scenario with no comparison effects (51.8 percent). This effect is due to a stronger free-riding effect for Democrats, given the higher average share of Democratic contributors in the United States. This exercise also indicates that the standard deviation of the share of Democrat contributions increases by just 1.1 percent in the counterfactual scenario

[28] In principle, our results cannot determine whether individuals care about the contribution patterns in some reference group, such as their neighbors, or whether they simply care about broader (e.g., nationwide) patterns.

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

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with no comparison effects relative to the actual figure (from 0.275 to 0.278).[30] This finding suggests that, unlike the conformity channel, the comparison channel has a very small effect on geographic polarization.

VI. Conclusions

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

We conclude by discussing some implications of our findings for the disclosure of contribution records. With the advent of the internet and the proliferation of online services provided by both the public and private sectors, the issues of information disclosure and privacy have become salient topics in the public debate.[31] Nonetheless, there is still limited evidence about those issues and their effects. The requirement that all campaign contributions be filed with a regulatory agency such as the FEC is key to preventing corruption and to enforcing other campaign regulations. The purpose of making detailed contribution records easily accessible online to the general public is less clear, however. The main justification of an open disclosure policy is that voters can use public records to learn about candidates (Gilbert 2013).[32] Our evidence suggests that

[30] The effect from the first term from the right-hand side of (6), which depends on δ_1, has the same impact on contributions from both parties, and thus it cannot affect the partisan geographic polarization of contributions. The second term, which depends on δ_2, can have a differential impact on individuals identified with the minority and the majority parties and can thus affect polarization.

[31] Just in the last few months of 2015, there were several cases that were widely covered in the media, such as the dissemination of voters' records ("Database of 191 Million U.S. Voters Exposed on Internet: Researcher," Reuters, December 29, 2015), the right to remove false information from online records compiled by private companies ("Justices Should Let an Online Privacy Case Proceed," *New York Times*, October 31, 2015), and the adoption of stringent data protection rules following the Edward Snowden scandal ("Europe Approves Tough New Data Protection Rules," *New York Times*, December 15, 2015).

[32] In the words of the Supreme Court, "Disclosure provides the electorate with information . . . in order to aid the voters in evaluating those who seek federal office. It allows voters to place each candidate in the political spectrum more precisely than is often possible solely on the basis of party labels and campaign speeches" (*Buckley v. Valeo*, 1976).

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public disclosure could have the unintended effect of facilitating social pressure. Interestingly, these unintended effects were mentioned by the Supreme Court decades before the contribution records became easily accessible online.[33] This view of stigmatization is also consistent with some anecdotal evidence, such as the use of FEC records to harass supporters of ballot Proposition 8 in California in 2008 and the alleged use of these records by the Internal Revenue Service to target supporters of the Tea Party (e.g., Briffault 2010; La Raja 2014).

Some simple modifications to the current policy could reduce the unintended uses of the FEC records without compromising some of the other goals of the regulation. If the goal of the FEC's disclosure policy is to allow voters to learn about candidates and to let journalists monitor corruption, specific identifying information about who makes each contribution (e.g., name, address, employer) should be largely irrelevant, possibly with the exception of large contributors. Thus, for smaller contributors the FEC could restrict the amount of identifying information reported online (e.g., using initials instead of full names). Alternatively, the FEC could impose a small pecuniary or nonpecuniary cost for accessing identifying information about contributors, which could discourage individuals from using the search tool to snoop on friends, relatives, and neighbors.[34]

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

[34] For instance, in Norway the use of the online search tool of tax records declined dramatically after the searches were de-anonymized, presumably because this tool stopped being used for finding out the incomes of social contacts (Perez-Truglia 2015).

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