

# Congressional Officials Grant Access to Individuals Because They Have Contributed to Campaigns: A Randomized Field Experiment<sup>\*</sup>

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## Abstract

Concern that donations to political campaigns secure preferential treatment from lawmakers has long occupied judges, scholars, and the public. However, the effects of political donations on legislators' behavior have proven notoriously difficult to assess. We first discuss reasons campaign donations may secure preferential treatment from lawmakers that may evade detection by traditional empirical approaches. We then present the first randomized field experiment on the effects of campaign contributions on access to policymakers. In the experiment, a political organization attempted to schedule meetings between 191 Congressional offices and active campaign donors in their districts. However, the organization randomly assigned whether it revealed to Congressional offices that prospective attendees had contributed to campaigns. When informed prospective attendees were political donors, senior policymakers made themselves available between three and four times more often ( $p < 0.01$ ). These findings underscore concerns about the Supreme Court's recent decisions deregulating campaign finance.

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Congressional campaigns spent \$3.7 billion in the 2012 election cycle (Center for Responsive Politics 2013) and, as Lewis et al. (1998) wryly note, such sums are not raised at bake sales. Rather, the lion's share of funds raised by political campaigns come from the wealthy, who typically have different preferences and priorities than most Americans (e.g., Page et al. 2013).

To many casual observers and the public at large, it seems obvious that this state of affairs secures the wealthy special influence with legislators (Lessig 2011; Gilens 2012). Indeed, the overwhelming majority of Americans who disapprove of Congress name "corruption" of legislators' decisions by campaign contributors as their main grievance (Grimmer and Powell 2013).

The reasons to suspect campaign contributions allow the wealthy to secure special treatment from legislators are straightforward enough. Money can buy expensive campaign advertising that increases legislators' chances of re-election (Green and Gerber 2008; Gerber et al. 2011). Legislators appear aware that money can affect whether they are re-elected as they choose to spend several hours each day raising it (Grim and Siddiqui 2013).<sup>1</sup> It is thus natural to suspect that, when making policy decisions, legislators sometimes face tradeoffs between satisfying voters and satisfying donors that they resolve in donors' favor (Stratmann 1991).

Despite the intuitive appeal of this line of reasoning, the influence of campaign donations on legislators' behavior is by no means obvious. In fact, what little agreement there is in the literature is that readily available evidence is insufficient for assessing the impacts of contributions (e.g., Baumgartner et al. 2009; Fox and Rothenberg 2011; Langbein 1986). For example, legislators may happen to support policies their contributor base approves of merely

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<sup>1</sup> Votes on campaign finance reform proposals also appear to reflect legislators' own understandings of how it will affect their own campaigns' bottom line (Bender 1988).

because contributors choose to give to legislators with positions they support, even if legislators never alter their behavior to please contributors (Grenzke 1989). Likewise, legislators might meet with contributors more frequently merely because they tend to meet with allies and their allies tend to donate to them, even if legislators would have met with their allies anyway if their allies had not made contributions. If legislators did favor contributors' interests, they may also confine such favoritism to activities that are difficult to observe (Hall and Wayman 1990).

Perhaps reflecting these challenges, efforts to empirically assess the effects of contributions on politicians' behavior are notorious for yielding uneven results (for reviews see Persily and Lammie 2004, fn. 53; Stratmann 2005). On the one hand, a constellation of evidence is consistent with the idea that contributions influence politicians' decisions. Most of all, contributors themselves appear to behave in ways consistent with a belief that their contributions influence policy (e.g., Gordon et al. 2007; Fournaies and Hall 2014b; Stratmann 1992). For example, when legislators lose committee seats, industries their former committees regulate often dramatically decrease their contributions, suggestive of a spot market for influence (Grimmer and Powell 2013). Legislators who occupy powerful positions within legislatures also tend to raise more, consistent with the notion that contributors see special value in gaining access to powerful decision-makers (Powell 2012). A great deal of work also links campaign contributions to legislators' decisions in many particular issue areas (e.g., Guttman 1978; Moore et al. 2013; Mian et al. 2013; Miler 2009; Stratmann 2002) and places them centrally in theories of policymaking (e.g., Stigler 1971; Abrams and Settle 1978; Kau et al. 1982).

However, the sizable literature on campaign contributions is largely characterized by skepticism that campaign contributions affect policymaking (see Stratmann 2005 for review). The empirical literature is replete with null findings and cause for skepticism (e.g., Bronars and

Lott 1997; Chin et al. 2000; McCarty and Rothenberg 1996; Wawro 2000; Wright 1989, 1990). Indeed, Ansolabehere et al. (2003)'s survey of the empirical literature finds that over 75% of research is insignificant or has the wrong sign (although see Stratmann 2005).

Moreover, many note that if donations could affect policy, donors ought to give much *more* than they currently do given policy's enormous economic implications (Tullock 1972; Ansolabehere et al. 2003). As Hall and Deardorff (2006) skeptically remark, were typically sized campaign donations able to meaningfully facilitate political influence, even the impecunious readers of academic journals should be able to buy a few votes in Congress from time to time.<sup>2</sup>

### **Donor Influence With and Without Quid Pro Quos: Theoretical Expectations and Empirical Challenges**

Existing empirical and theoretical work has typically considered quid pro quo relationships between donors and legislators. In this view, campaign contributions represent an implicit contract: donors give to legislators with the understanding that legislators will grant them access or support for particular policies in exchange (see Hall et al. 2009 for review).

However, many scholars doubt that quid pro quo arrangements could persist between donors and legislators, as there is no enforcement mechanism for these 'contracts' (McCarty and Rothenberg 1996).

Quid pro quo arrangements are one way donations may affect legislators' decisions, but they are not the only way. Recent Supreme Court decisions have raised a different possibility, largely neglected in academic literature but intriguing and potentially significant: do legislators grant preferential treatment to individuals because they have donated to *other* campaigns? For

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<sup>2</sup> Anecdotes and case studies yield conflicting accounts as well. While some Washington insiders depict quid pro quo politics as common among Congresspeople (e.g., Abramoff 2011), others strongly deny that donations yield influence (e.g., Drutman 2010). More systematic qualitative investigations have likewise found little evidence that campaign contributions are central to special interests' influence strategies (e.g., Drutman and Hopkins 2013).

example, would a legislator grant preferential treatment to an individual because they have given to a SuperPAC (*Citizens United v. FEC*) or to *other* legislators' campaigns (*McCutcheon v. FEC*)? In both the *Citizens United* and *McCutcheon* decisions, the Court reasoned that donations to independent political efforts would not yield influence with legislators. We see at least three reasons to expect that they would.

First, legislators might privilege an individual's concerns to the extent that individual *might* donate to their campaign, not only to the extent that person has already given. In particular, legislators may see donations to other campaigns as a signal that an individual might give to their own. Legislators likely recognize that some citizens have more interest in and ability to contribute to campaigns than others, regardless of why particular individuals choose to contribute (be it to gain access, exert influence, express their views, or extract vanity rents). That an individual has given to another campaign is an excellent signal that she has interest in contributing to campaigns and the means to do so. An analogy to firms illustrates the point. Previous donors to legislators' own campaigns may represent 'satisfied customers' whose 'business' legislators work hard to keep; but, donors to other campaigns may also represent '*potential* customers' whose 'business' legislators have an especially good chance to acquire.

Second, legislators may not only seek to facilitate donations for their own campaigns; they may also seek to forestall donations to their opponents'. For example, a legislator may work to maintain support from someone who has previously contributed to her opponent in order to discourage him from contributing to her opponent again. This may be especially likely for legislators concerned about facing opposition in primaries, as shared ideology is no guarantee that a potential donor will not find reason to support an opponent.

What unites these first two possibilities is that legislators may privilege individuals'

concerns in anticipation of *future* donations supporting *or* opposing their re-election, not only in response to donations to their own campaign that have already been made (e.g., Fox and Rothenberg 2011). When politicians believe someone *might* contribute to their campaign or their opponent's, they may have good reason to attend to their concerns even if no money has yet to change hands.

Finally, legislators may also view contributions to other legislators as a signal of shared ideology or concerns (e.g., Hall et al. 2009). Compared to an individual who has given no money to other legislators, legislators may view an individual who has given money to other similar legislators as likelier to provide useful policy information. Even as this explanation does not turn on legislators' desire to raise money per se, it still has troubling implications for political equality insofar as citizens of means may be more able to send such signals.

From an empirical point of view, it is particularly alarming that legislators may privilege *potential* donors' concerns because readily available data is ill-equipped to detect legislators doing so. If a donor can gain access to many legislators by contributing to only a few legislators, comparisons between legislators to whom a donor gives and does not give may not capture the full effect of the donor's contributions. If that donor had not given at all, she may not have gained access to any legislators. However, if a donor does give, she may have 'treated' all legislators with the knowledge that she is interested in contributing merely through contributions to a few legislators. Comparing the behavior of legislators to whom a donor did and did not donate may thus dramatically understate the total effects of their contribution activity.

To appreciate this point, consider the scenarios outlined in Table 1. In the hypothetical world we consider, legislators grant access to all individuals who have contributed to *any* legislator, as these individuals represent potential future donors. All individuals who have not

contributed to any legislator, such as Individual A, are denied access because there is little indication they may donate. Now, consider an empirical analyst studying the behavior of legislators towards Individual B to assess the effects of her contribution to Legislator 1. Although Individual B gave only to Legislator 1, she gains access to both Legislator 1 and Legislator 2 as a result. As a result, comparing the access Legislator 1 granted to Individual B and the access Legislator 2 granted to Individual B would be misleading. This comparison might suggest there was no effect of B's contribution to Legislator 1, as Legislator 2 also granted B access despite that B did not donate to her. This pattern is indeed inconsistent with the notion that legislators grant access as a *quid pro quo*. However, the scope for donor influence does not end here. To understand whether legislators grant access to those who have contributed to others, the proper comparison is how legislators treat donors and non-donors—here, individuals A and B—not how individuals who donated are treated by the legislators they have and have not contributed to.<sup>3</sup>

**Table 1. Scenarios Illustrating Failure to Identify Overall Effects of Contributions**

| Individual | Donated to<br>Legislator 1? | Access to<br>Legislator 1? | Donated to<br>Legislator 2? | Access to<br>Legislator 2? |
|------------|-----------------------------|----------------------------|-----------------------------|----------------------------|
| A          | No                          | No                         | No                          | No                         |
| B          | Yes                         | Yes                        | No                          | <b>Yes</b>                 |

Like a police force concluding guns are insignificant enablers of robbery by only focusing on cases when guns are fired, scholars run the risk of dramatically underestimating the role of money in politics by focusing only on the money that is spent (Fox and Rothenberg 2011; see also Simon 1953). Unfortunately, the existence of such equilibrium behavior is difficult to identify in readily available data, as whether or not legislators are aware individuals are donors is not generally randomly assigned.

<sup>3</sup> Readers may recognize this logic as that illustrating how SUTVA violations can bias estimated effects toward zero (Aronow and Samii 2013).

In this paper we report evidence uniquely capable of assessing how legislators react when they learn an individual is an active campaign donor, overcoming both the barriers to inference the literature has identified and that we discussed above. This evidence—the first randomized field experiment in the study of campaign finance—is simple, but unique and revealing. The next section describes the experimental design.

### **Experimental Design**

The experiment considers whether legislators and high-level staffers grant individuals access to individuals because they have contributed to campaigns. In the experiment, a political organization attempted to arrange meetings between high-level Congressional officials and individuals residing in their districts who had previously given to political campaigns. However, the organization randomly assigned whether it revealed that the prospective attendees were active campaign donors. Randomly assigning whether Congressional offices *knew* the prospective meeting attendees were campaign donors permits similar conclusions as would randomly assigning whether individuals contribute but without the potential logistical and legal challenges of doing so (Ludwig et al. 2011).

In contrast to existing research, a field experiment allows us to draw rigorous causal conclusions about political actors' real-world behavior (Findley et al. 2013; Grose 2014). This research design also allows us to overcome longstanding concerns that associations between donor's contributions and policymakers' behavior are driven by donor's propensity to give to existing allies and do not reflect policymakers' greater attention to individuals *because* they have donated.

#### *The Significance of Access*

Whether contributors can gain special access to influential policymakers is significant for



at least two reasons.

First, access to powerful officials is often necessary for influencing policy, even if it is not sufficient (Hansen 1992). In order to make one's case to a policymaker that one's preferred policy is superior, one typically needs to secure her attention first (Lewis et al. 1998; Hasen 2012; Wright 1990; Hall and Wayman 1990). Prominent theories of interest group influence thus suggest that making persuasive arguments to legislators is one of lobbyists' primary tools for influencing policy (e.g., Rotemberg 2003).

Second, access is considered a good measure of whose preferences legislators prioritize. Legislators' time is finite, so when they decide to meet with individuals who ask, they relinquish time they could spend pursuing other activities (Hall 1996). For example, a legislator may face a choice between meeting with a voter whose vote she could win or meeting with a donor whose contributions might buy advertising that convinces many voters to support her. Alternatively, policy-motivated legislators might take donations as a signal of policy expertise and value donors' counsel over typical constituents'. Regardless of what motivates legislators, a simple revealed preference argument suggests that if legislators choose to meet with individuals more often because they have contributed to campaigns, they place unique value on donors' concerns.

Consistent with both notions of why access is significant, a chief concern voiced by critics of the American system of campaign finance has been that it encourages legislators to spend time attending to the special concerns of donors (e.g., Barber 2014; Fournaies and Hall 2014a, b; Hall and Wayman 1990; Hall and Deardorff 2006; Lessig 2011; Miler 2009; Page et al. 2013; Powell 2013).

### *Context and Population*

The experiment was embedded in a political organization's effort to build support for a

bill before Congress to ban a chemical. The organization, CREDO Action, is a US liberal political organization with around 3.5 million members.

The sample for the experiment included every United States Representative of a particular political party who had not already cosponsored the bill. This led to a sample of 191 representatives.<sup>4</sup>

For the experiment, in each of 191 Congressional districts, the organization first secured agreement from around a dozen organization members who had previously donated to political campaigns to attend a meeting with their Members of Congress' office.

#### *Random Assignment*

Before the organization attempted to arrange the meetings between these campaign donors and their Member of Congress' offices, the offices were randomly assigned to one of two experimental conditions, a Constituent condition and a Revealed Donor condition.

To maximize the limited statistical power inherent in a study of members of the finitely-sized United States House of Representatives, legislators were blocked into triplets with the other legislators who were most similar to them on the following covariates: a score of environmental voting compiled by a third party (ProgressivePunch 2013), whether the legislator cosponsored the bill in a previous Congress, the number of years the legislator had served in Congress, the legislator's ideology (Clinton et al. 2004; Jackman 2013), the number of members of the political organization that resided within 40 miles of the district office where the meeting was to be held, and Barack Obama's share of the 2012 two-party presidential vote in the district. Note that using

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<sup>4</sup> While meeting invitations were being sent to legislators and well in advance of any outcome measures being collected, an employee accidentally emailed one of the legislators (in the Constituent condition) a meeting request addressed to a different Member of Congress (also in the Constituent condition). A third staffer not knowledgeable about the treatment condition of this legislator decided that this legislator would be removed from the study and a follow-up e-mail was sent to this legislator immediately apologizing for sending the request to the wrong office and asking them to discard the request. This reduced the sample size from 192 to 191 legislators.

covariates to conduct blocking does not affect estimation directly and the covariates need not be measured without error in order to improve statistical efficiency; using covariates to conduct block random assignment ex ante merely increases statistical power to the extent the covariates ultimately prove prognostic of outcomes. Legislative offices were allocated to blocks using `blockTools` in R (Moore and Schnakenberg 2013).

Legislative offices were randomly assigned to treatment conditions within each of the 64 blocks. Table S2 in the Supplementary Materials presents randomization checks and reports the expected covariate balance across the treatment conditions.

### *Procedure*

The organization then sent every Congressional office a meeting request. For offices randomized to the Constituent condition, the meeting request described the prospective attendees as “local constituents”; in the Revealed Donor condition, the request revealed that the attendees were “local campaign donors.” Except for revealing that the prospective attendees were “local campaign donors,” no other details about the meeting requests, delivered via email, were changed.

The email addresses for the schedulers were obtained from the *National Journals’ Almanac of American Politics*. If a scheduler could not be identified in the *Almanac*, the organization found the scheduler’s name using LegiStorm. If more than one scheduler was identified, such as a scheduler in the district and one in D.C., both were emailed. If a scheduler did not appear in either source, the organization collected the email address of the staffer most likely to have the duties of a scheduler (e.g., office manager, personal assistant, or district manager). The organization requested a meeting in the congressperson’s district office listed in the *Almanac*, and if multiple offices were listed, the organization chose the office in the city with

the greatest number of staffers.

The email sent to legislators in the revealed donor condition was:

SUBJECT: Meeting with local campaign donors about cosponsoring bill to [BILL DETAILS]?

BODY:

Hi [SCHEDULER],

My name is [EMPLOYEE] and I am an Organizer with CREDO Action. Around a dozen of our members near [DISTRICT CITY] who are active political donors have expressed interest in meeting with the Congressman, in person or by phone from the [CITY] office.

These donors are extremely concerned by [DETAILS ON BILL] and would like to tell the Congressman why his base would like him to cosponsor H.R. [BILL DETAILS]. This legislation would [DETAILS ON BILL]. They very much hope that the Congressman will cosponsor the bill.

If the Congressman is not available, they'd like to arrange a meeting with the chief of staff, LA, or local district director, in person or by phone from your office.

Could we arrange such a call on [DATES]? Our members are looking for just 30 minutes to have their concerns and ideas heard.

Looking forward to hearing from you on what time might work well and who our members can expect to meet with.

Thanks in advance,

[EMPLOYEE]

The email sent to legislators in the Constituent condition was the same, with the “donor” language replaced by “constituent” language, as shown below:

SUBJECT: Meeting with local constituents about cosponsoring bill to [BILL DETAILS]?

BODY:

Hi [SCHEDULER],

My name is [EMPLOYEE] and I am an Organizer with CREDO Action. Around a dozen of our members near [DISTRICT CITY] who are concerned constituents have expressed interest in meeting with the Congressman, in person or by phone from the [CITY] office.

These members are extremely concerned by [DETAILS ON BILL] and would like to tell the Congressman why his base would like him to cosponsor H.R. [BILL DETAILS]. This legislation would [DETAILS ON

BILL]. They very much hope that the Congressman will cosponsor the bill.

If the Congressman is not available, they'd like to arrange a meeting with the chief of staff, LA, or local district director, in person or by phone from your office.

Could we arrange such a call on [DATES]? Our members are looking for just 30 minutes to have their concerns and ideas heard. Looking forward to hearing from you on what time might work well and who our members can expect to meet with.

Thanks in advance,  
[EMPLOYEE]

In neither condition did the organization supply the names or contribution histories of the attendees.

#### *Protocol for Communicating with Congressional Offices*

Successfully holding the meetings required several communications with Congressional offices. For example, organization staffers often needed to obtain information about the physical location of the office and security procedures for entry, answer questions about the number of attendees expected so a room of proper size could be reserved, etc. These interactions potentially represented an opportunity for organization employees to introduce bias by communicating to the treatment offices in a different manner. To guard against this possibility, organization employees carefully followed a detailed protocol when communicating with Congressional offices to ensure no differences in their interactions with the offices apart from the randomly assigned treatment. The organization thus pre-specified and standardized their responses to follow-up inquiries prior to sending any invitations so as to ensure all correspondence would be identical with the offices regardless of treatment condition. In the rare event that the organization received an email that required a response that had not been pre-specified, the situation was described to a different employee blind to that office's experimental condition who composed

the response and added it to the list of standard responses. Table S1 in the Supporting Information lists all the responses verbatim.

For example, if the organization did not receive a reply within three business days, the organization sent the below follow-up email as a reply to the original email on the morning of the fourth day.

Hi [SCHEDULER],

My name is [EMPLOYEE] and I am an Organizer with CREDO Action. I am following up on this meeting request I sent you last week.

We're attempting to hold these meetings on [BILL] with Members of Congress from across the country. Please let me know if we could schedule this meeting. We are hoping for sometime around noon on [DATES].

Thanks, and hope to hear from you soon.

Best,

[EMPLOYEE]

The organization did not further pursue the meeting after two request emails were sent. If the scheduler offered scheduling for a date the organization did not originally request, the organization reiterated their request to hold the meeting on the originally specified dates.

If a meeting was scheduled, the organization invited members of the sponsoring political organization who self-identified as political donors and lived near the congressperson's district office. The organization provided talking points to the meeting attendees and called or emailed every attendee to answer questions about the meeting logistics or talking points.

A team of organization employees implemented this procedure in June, July, and August of 2013, identifying organization members who had previously donated to campaigns, recruiting them for the meetings, contacting Congressional offices, arranging meeting details, preparing information about the bill at hand for the attendees, and ensuring the meetings went smoothly.

This experimental design sets aside many ethical concerns that often arise with field experiments on elites. Although legislators and meeting attendees were not aware of the experiment itself, no deception was involved: all the attendees were previous donors and all the meetings were real, a part of the organization's efforts to build support for a bill before Congress. Attendees were aware that their donation history may have been disclosed to their legislators. And, finally, the requests did not ask legislators to engage in any illegal behavior; the requests merely noted that the attendees were campaign donors.

### *The Benefits of Random Assignment*

Random assignment of Congressional offices to experimental conditions ensures that significant differences in the access they provided across the Constituent and Revealed Donor conditions can only be attributed to the randomly assigned treatment: whether Congressional officials were informed that meeting attendees were campaign donors. Existing studies establishing correlations between donations and legislators' behavior have consistently had difficulty ruling out the hypothesis that legislators do not alter their behavior to favor donors but that donors merely give more to legislators whose choices they support (Baumgartner et al. 2009; Grenzke 1989; Hall and Wayman 1990). By contrast, our experimental design is capable of assessing the causal effect of knowledge that an individual has donated on how legislators treat that individual, without the need to control for factors that lead certain individuals to donate.

Random assignment of the knowledge that an individual has donated also allows us to uncover an effect of contributions that previous studies may have missed. In particular, contributors may also gain access to legislators simply by virtue of being donors in general. Legislators to whom a donor has given and not given might both be more likely to meet with a donor – for example, one legislator may hope to secure a first donation, and the other my hope to

secure a second. Comparisons between legislators to whom a donor gives and does not give may thus understate the total effects of the contributions.

Random assignment also holds constant across conditions other factors that may lead some Congressional offices to grant greater access regardless, such as chance unavailability of certain officials during the study period.

#### *Ex Ante Ranking of Access Quality and Outcome Measurement*

In all cases, Congressional offices specified more than 24 hours in advance which representative from the office would be attending the meeting and this was recorded as the outcome variable of the experiment. After the meeting, the organization contacted the attendees to confirm that the meeting occurred and that the promised staffer attended. This was confirmed in all cases.

To test the hypothesis that Congressional offices make more senior staffers available for meetings with individuals because they have donated, prior to examining any results from the experiment, we developed a scheme to rank Congressional officials in order of seniority and influence. This ranking mirrored the group's request itself, which noted the attendees' desire to meet with the most senior officials available (see Supplementary Materials):

1. Member of Congress (most desirable outcome)
2. Chief of Staff [most senior staffer in Congressional offices]
3. Legislative Director or Deputy Chief of Staff [second most senior staffers in Congressional offices]
4. Legislative Assistant or District Director [policy-focused staffers, but less senior than above]
5. Other District-Based Staffer, e.g., Constituent Services Representative [these staffers



rarely have policy responsibilities]

6. No Meeting (least desirable outcome)

Prior to examining results, two coders blind to treatment assignment categorized each Congressional office into the above categories according to whether a meeting was scheduled and who attended. Initial disagreements about how to code staffers in two offices were easily resolved. If the office did not respond within three weeks of the initial request, the office was coded as declining the meeting.<sup>5</sup>

## Results

Descriptive statistics for the share of offices that provided access to officials at each level of seniority are presented in Table 2. The first two columns of Table 2 record how often offices in the Revealed Donor Condition and Constituent Condition made officials available for meetings. The next two columns show the cumulative probability that offices in each condition made officials available at or above each rank. We discuss the calculation of  $p$ -values below.

**Table 2. Results: Level of access gained in Constituent and Revealed Donor conditions**

| Level of Official Group Met                   | Constituent Condition Frequency | Revealed Donor Condition Frequency | Constituent Condition Cumulative Probability | Revealed Donor Condition Cumulative Probability | $p$ -value: Revealed Donors more likely to gain access at or above this rank |
|---|---------------------------------|------------------------------------|--|---|--|
| Member of Congress                            | 2.4%                            | 7.8%                               | 2.4%   | 7.8%  | $p = 0.07$   |
| Chief of Staff                                | 0.0%                            | 4.7%                               | 2.4%   | 12.5%   | $p = 0.006$  |
| Legislative Director or Deputy Chief of Staff | 3.2%                            | 6.3%                               | 5.6%   | 18.8%   | $p = 0.005$  |

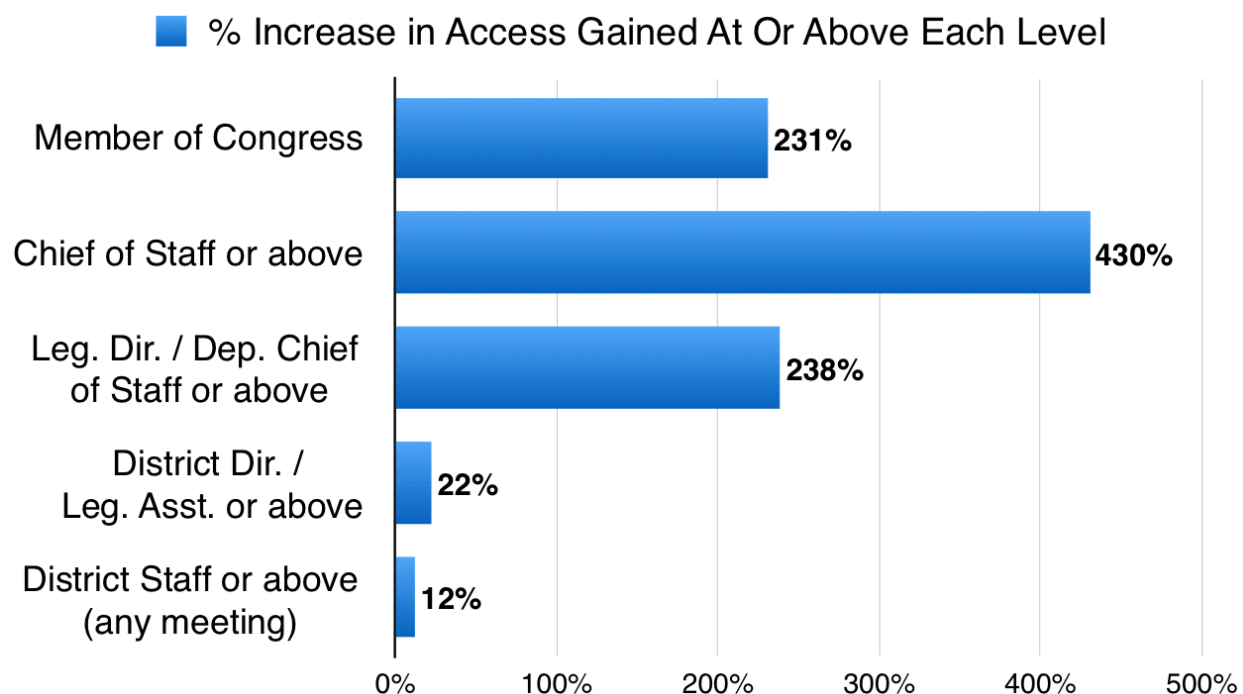
<sup>5</sup> This decision rule was set in advance of the experiment.

|   |       |       |       |       |            |
|---|-------|-------|-------|-------|------------|
| DC-Based Legislative Assistant or Local District Director | 25.2% | 18.8% | 30.8% | 37.6% | $p = 0.17$ |
| Other District-Based Staffer                              | 12.6% | 10.9% | 43.4% | 48.5% | $p = 0.26$ |
| No Meeting  | 56.7% | 51.6% | 100%  | 100%  | -          |

Table 2 shows that senior policymakers attended the meetings considerably more frequently when Congressional offices were informed that the meeting attendees were donors. Only 2.4% of offices arranged meetings with a Member of Congress or Chief of Staff when they believed the attendees were merely constituents, but 12.5% did so when the attendees were revealed to be donors. In addition, 18.8% of the groups revealed to be donors met with any senior staffer, while only 5.5% of the groups described as constituents gained access to a senior staffer, a more than three-fold increase.

To help appreciate the magnitude of these effects, Figure 1 displays the percentage increase in access offices in the Revealed Donor Condition granted relative to offices in the Constituent Condition. Members of Congress were more than three times as likely to meet with individuals when their offices were informed the attendees were donors, an over 200% increase in access. Putative donors were likewise more than 400% as likely to meet with either a Member of Congress or a Chief of Staff. Strikingly, nearly all the meetings with Chiefs of Staff and Members of Congress occurred in the Revealed Donor condition. When Congressional offices were only informed that the attendees were their constituents, attendees very rarely gained access to officials at this level. Offices in the Revealed Donor Condition were also 200% more likely to make any senior officials available for the meetings.

**Figure 1. Percentage Increase in Access Revealed Donors Gained, At Or Above Each Level**



To assess how likely these differences in access would have arisen by chance, each row in the final column of Table 2 displays the exact  $p$ -value (obtained using randomization inference, a procedure that yields exact  $p$ -values for experiments even in small samples [Keele et al. 2012]) that differences as large as the observed differences would have been observed if informing the offices that the attendees were donors did not influence access decisions. The exact procedures and code are given in the Supplementary Materials and available in the replication materials.

It is highly unlikely that the greater number of meetings arranged with officials at the rank of Chief of Staff and above or Legislative Director and above would have occurred in the donor condition by chance ( $ps < 0.01$ ), even when accounting for strict multiple testing corrections ( $ps < 0.05$ ). It is also unlikely that Members of Congress themselves would have met with groups in the Revealed Donor condition more often by chance ( $p = 0.07$ ).

As an alternative way to analyze our data, an ordered probit tested the overall hypothesis that revealing the attendees were donors caused offices to arrange meetings with more senior

officials, with exact  $p$ -values again obtained using randomization inference (see Supplementary Materials for details). This test yielded a  $p$ -value of 0.05, indicating a low probability that offices in the Revealed Donor condition would have provided attendees the superior access they did if knowledge that the attendees had donated did not affect the level of access they granted.

## **Discussion**

The effects of campaign contributions on legislators' behavior have famously evaded rigorous quantification. Existing literature has had difficulty ruling out the possibility that policymakers tend to act in ways contributors approve merely because contributors give to legislators whose actions they support. Moreover, existing literature has largely focused on the potential for quid pro quo relationships between donors and legislators, neglecting the possibility that legislators give special attention to donors in general, even to other campaigns. Our experiment is the first to clearly document that policymakers grant preferential treatment to individuals because they have donated to campaigns. When a political organization randomly assigned some Congressional offices to be informed that individuals seeking a meeting were donors, those individuals had much greater success securing meetings with senior policymakers.

The superior access we observed congressional offices granting to putative donors was not only statistically significant, it was substantively large: senior policymakers attended the meetings between three and five times as often when offices were informed the attendees were donors. We find it particularly striking that we detected such large differences in how Congressional offices behaved across the experimental conditions because the treatment was extraordinarily subtle. It consisted of merely replacing the phrase "local constituents" with "local campaign donors" in two locations in a meeting request. By contrast, large campaign donors often tender generous checks directly to legislators. The differences uncovered here seem

unlikely to be smaller in such situations.

These findings have particular importance in light of recent Supreme Court decisions that have loosened many American campaign finance regulations. These decisions have crucially turned on whether legislators might grant preferential treatment to individuals because they have donated to campaigns in general. The justices speculated that political contributions beyond quid pro quo arrangements have limited potential to cultivate influence with lawmakers. However, this possibility that legislators grant preferential treatment to individuals who have given to *other* campaigns has generally evaded the literature's focus on quid pro quo arrangements. This possibility also poses a particularly difficult problem to empirically investigate because legislators to whom an individual has not donated may be affected by observing an individual's donations to other legislators, potentially leading comparisons across legislators to underestimate the full effects of contributions. In this paper, we outlined reasons why legislators may pay special attention to donors in general. The results of our experiment are consistent with this notion, as attendees gained superior access by being identified as "active campaign donors" in general. Cognizant of ethical and legal considerations, our experiment did not randomly assign whether individuals actually donated to a particular legislator, leaving open the possibility that congressional officials may have granted superior access to those in the Revealed Donor condition because they inferred these individuals had given to their own campaign. However, our experiment highlights the multiple reasons why legislators might attend to donors' concerns and suggests future work should take a broad view of the ways donations may affect legislators' behavior.

We hasten to note several limitations of our study. First, one experiment cannot definitively establish *why* senior officials more readily avail themselves to putative donors

(Bullock et al. 2010). Our experiment was designed to capture a broader variety of reasons legislators may privilege contributors. However, as in many experiments, this stronger external validity also leaves open some questions regarding mechanisms. Legislators may anticipate that meeting with donors increases their likelihood of donating again, but legislators may also expect donors to have greater policy expertise than constituents or be more prognostic of other party activists' preferences. Different mechanisms imply different policy remedies and different legal implications, so we would welcome further research on the matter. Moreover, that legislators privilege requests from individuals because they have donated has the same social consequences regardless of why they do so. Nevertheless, the question of mechanisms is ripe for future research.

This experiment also invites replication with other politicians, actors, and groups. We see little reason to expect legislators would be sensitive to knowledge that members of a grassroots liberal organization give to campaigns but not to knowledge that corporate executives do so; in fact, to the extent concerns about raising money were at play, the effects seem likely to be larger for organizations whose members are wealthy. However, successful replications in other contexts would no doubt further strengthen the credibility of the findings.

Our findings also leave open the question of whether or not increased access translates into tangible influence. Certainly, professional lobbyists behave as though securing access with legislators is a tremendously important catalyst for influence (e.g., Hansen 1992), and reformers have focused on disparities in access in part because the assumption that access can facilitate influence seems so natural. That Washington actors place such value on access makes it worthy of study in its own right. However, future work should build on the present research and examine whether the increased access contributions appear to enable can be used to secure policy

outcomes.

Assuming the Court does not revisit its decisions in the *Citizens United* and *McCutcheon* cases, our findings raise troubling concerns about their implications for political equality. Access gives individuals the opportunity to make arguments and requests to powerful decision-makers. Our experiment suggests that individuals who can afford to donate to political campaigns are especially likely to attain such access, and the Court's recent decisions have the potential to worsen this disparity. For example, by increasing the amount of money wealthy individuals can legally donate to candidates, the *McCutcheon* decision may encourage legislators to work to earn donations from a broader variety of donors to other campaigns.

The implications of our findings for political equality bear emphasis. In recent years, scholars have become increasingly concerned with the ways rising economic inequality may translate into political inequality (e.g., Bartels 2008). The hypothesis that individuals can command greater attention from influential policymakers by contributing to campaigns has been among the most contested explanations for how financial resources translate into political power (Gilens 2012; Gilens and Page 2014; Hacker and Pierson 2010; Hall and Wayman 1990; Lessig 2011). The simple but revealing experiment presented here elevates this hypothesis from extensively contested to experimentally supported: the organization members who wanted to express their concerns to senior policymakers in our experiment were rarely able to do so when policymakers were not informed they had donated. Our results thus suggest that the vast majority of Americans who have not donated to campaigns are at a disadvantage when attempting to express their concerns to policymakers.

Few Americans can afford to contribute to campaigns, while those who can afford to do so have markedly different preferences and priorities than the broader public (Page et al. 2013;

Page and Seawright 2014). To the extent that who policymakers hear from is determined by individuals' ability to afford to donate to campaigns, policymakers' worldviews may be significantly distorted in ways that benefit the wealthy (Lessig 2011; Hall and Deardorf 2006). Schattschneider (1960) famously noted that those who tend to participate in politics are more likely to speak with an "upper class accent." Our findings suggest policymakers play an active role in inviting the wealthiest voices to fill the halls of Congress.



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## Supplementary Materials for “Campaign Donations Facilitate Access to Congressional Officials: A Field Experiment”

Joshua Kalla and David Broockman

### Randomization Inference Procedure

As described in the text, after scheduling the meetings, the organization recorded the name and title of the staffer who would be attending the meeting. After the experiment was finished, two coders who were blind to treatment assignment sorted the staffers into one of six ranks:

1. Member of Congress (best outcome)
2. Chief of Staff [most senior staffer in Congressional offices]
3. Legislative Director or Deputy Chief of Staff [second most senior staffers in Congressional offices]
4. Legislative Assistant or District Director [policy-focused staffers, but less senior than above]
5. Other District-Based Staffer [these staffers rarely have policy responsibilities]
6. No Meeting (worst outcome)

Two initial coding disagreements between the two coders were easily resolved.

The statistical methodology for each of the tests described in the text are as follows.

To calculate all  $p$ -values we conducted randomization inference (Keele et al. 2012), which yields exact  $p$ -values under the sharp null of no effect. To do so, this procedure first creates a matrix of 5,000,000 possible treatment assignment vectors given the blocked randomization procedure, with R code as follows (the outcome variable is sorted by block):

```
perms <- replicate(5000000,
  c(
    as.vector(replicate(63,sample(c(0,0,1),3))), #block random
assignment
    sample(c(0,0,1),2) #last block has two members
  )
)
```

To calculate the  $p$ -values reported in Table 1, the following R code computes the average treatment effect under each possible pattern of random assignment with an indicator as the outcome variable. The  $p$ -value is the share of possible random assignments that would yield average treatment effects as large or larger than the average treatment effect observed in the experiment were the sharp null hypothesis true. Intuitively, this corresponds to the likelihood of seeing an average treatment effect as or more extreme if the treatment had no effect for every unit.<sup>15</sup>

```
#RI Functions for ATE estimation
est.ate <- function(treat, outcome){
  return(mean(outcome[treat==1]) - mean(outcome[treat==0]))
}
```

```

ri <- function(outcome, treat, perms){
  ate <- est.ate(treat, outcome)
  ate.dist.under.sharp.null <- apply(perms, 2, est.ate, outcome)
  p.value <- mean(ate <= ate.dist.under.sharp.null)
  return(list(ate = ate, p.value = p.value))
}
#Test hypotheses that donor condition met more at or above each rank
for(i in 5:1){
  print(i)
  met.this.high <- as.numeric(data$staffrank >= i)
  print(ri(met.this.high, data$treat, perms))
}

```

Given the ordinal nature of the outcome variable, an ordered probit model is appropriate for testing the overall hypothesis. The following R code computes the log-likelihood of the model under each possible pattern of random assignment with the staff rank as the outcome variable. The  $p$ -value is the share of possible random assignments that yield log-likelihoods larger than the average treatment effect observed in the experiment.

```

#Ordered probit
library(MASS)
staffrankfactor <- ordered(data$staffrank)
get.ll <- function(treat) logLik(polr(staffrankfactor ~ treat, method =
"probit"))[1]
ll.actual <- get.ll(data$treat.donor.invite.stage)
ll.dist.sharp.null <- apply(perms, 2, get.ll)
mean(ll.actual <= ll.dist.sharp.null) #pvalue

```



**Table S1. Rules for responses to Congressional offices**

| <b>Email Received</b>  | <b>Response Rule</b>  |
|--|---|
| Email bounces or an automatic reply states that the intended recipient is no longer working in the office and there is only one intended recipient                   | Use LegiStorm to find the next contact for a scheduler or office manager  |
| When there are two or more intended recipients and one email bounces or an automatic reply states that one intended recipient is no longer working in the office and | No action required because there are additional recipients  |
| Scheduler asks where we would like to hold the meeting   | Reply with the name of the district office<br><br>Hi [SCHEDULER],<br>Thanks for checking. The [DISTRICT] office.<br>Best,<br>[EMPLOYEE]   |
| Autoreply with a link to an online scheduling form   | Fill out the online form and paste in body of original request. Take no further action  |
| Email thanking us for initial email but not asking further questions   | No reply  |
| Email asks about dates   | Reply with the originally requested dates but give flexibility on time<br><br>Hi [SCHEDULER],<br>Thanks for getting back to me. We are looking to schedule a meeting on one of those three days [ORIGINAL DATES]. Around noon is preferable, but we can probably do any time between 10am-3pm or so.<br>Thanks,<br>[EMPLOYEE] |
| Emails asks for contact information  | Reply with personal cell phone number   |
| Receive call from staff  | Maintain message of the original email.<br>Record date, time and subject of phone call  |
| Scheduler provides email of another staffer in the office  | Send original email to the new staffer  |
| Receive an email from another staffer  | Reply with original email   |
| Request list of attendees before scheduling the meeting  | State that all attendees will be from the Member's district but that we cannot release their personal information until we confirm the meeting with them. If the scheduler refuses twice, stop trying to schedule the meeting   |

|   |  |
|---|--|
|   | <p>Hi [SCHEDULER]</p> <p>I can send you a list of attendees and where they live in the [MC's] district once they are finalized. However, right now, everyone's schedule and availability is different, hence why I am helping to get the scheduling and logistics end of this done. But they are all constituents of [MC].</p> <p>Best,<br/>[EMPLOYEE]</p>   |
| Scheduler offers meeting during the August recess       | <p>Ask to meet with a staffer during one of the original dates</p> <p>Hi [SCHEDULER],</p> <p>We would like to hold a meeting sometime around [ORIGINAL DATES]. Since [MC] is not available, could we arrange a meeting with the chief of staff, LA, or local district director, in person or by phone from your district office?</p> <p>Thanks,<br/>[EMPLOYEE]</p>   |
| If they request more information on the bill            | <p>Reply with the factsheets</p> <p>Hi [STAFF],</p> <p>Thank you for taking the time to learn more about [BILL] ahead of meeting with [ORGANIZATION] on [DATE] in the [DISTRICT] office. [LINKS TO BACKGROUND INFORMATION AVAILABLE FROM TWO INDEPENDENT ORGANIZATIONS]. Our members will be able to provide more information, in addition to their personal stories, when they meet with you on [DATE].</p> <p>Let me know if you have any other questions.</p> <p>Thanks,<br/>[EMPLOYEE]</p> |
| If they request more information about the organization | <p>Share number of members, amount donated to non-profit groups, and provide a link to the organization's "About Us" page.</p>   |

**Table S2. Relationship between Treatment Assignment and Covariates**

|   | <b>Revealed Donor<br/>Condition</b> | <b>Constituent<br/>Condition</b> |
|---|-------------------------------------|----------------------------------|
|   | <b>Mean</b>                         | <b>Mean</b>                      |
| <b>Members within 40<br/>miles of district<br/>office</b>               | 7365.62<br>(5080.78)                | 7760.32<br>(5076.45)             |
| <b>Ideal Point<sup>6</sup></b>  | 1.00<br>(0.38)                      | 1.00<br>(0.41)                   |
| <b>2012 Presidential<br/>Vote Share in<br/>District<sup>7</sup></b>     | 64.88<br>(11.58)                    | 65.59<br>(12.32)                 |
| <b>Environment<br/>Score<sup>8</sup></b>                                | 88.82<br>(10.40)                    | 89.58<br>(10.13)                 |
| <b>2012 Total<br/>Campaign Receipts</b>                                 | \$1,538,232<br>(961,590)            | \$1,642,801<br>(1,016,656)       |
| <b>Multinomial<br/>Logistic Regression<br/><math>\chi^2</math> Test</b> | $p=0.92, \chi^2=1.44$ (5 d.f.)      |                                  |
| <b>N</b>  | 64                                  | 127                              |

*Notes: The rows report mean values with standard deviation of the mean in parentheses. The LR test reports the results from multinomial logistic regression of treatment assignment on the covariates, not including the block indicators.*

<sup>6</sup> Sign may be reversed to anonymize the political party of the legislators.

<sup>7</sup> This may reflect Obama or Romney vote share to maintain the anonymity of the political parties of the legislators.

<sup>8</sup> Higher score does not necessarily reflect a more pro-environment voting record.