Campaign Contributions Facilitate Access to Congressional Officials: A Randomized Field Experiment

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Concern that donations to political campaigns secure preferential treatment from policy makers has long occupied judges, scholars, and the public. However, the effects of contributions on policy makers' behavior are notoriously difficult to assess. We present the first randomized field experiment on the topic. The experiment focuses on whether contributions facilitate access to influential policy makers. In the experiment, a political organization attempted to schedule meetings between 191 congressional offices and the organization's members in their districts who were campaign donors. 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 policy makers made themselves available between three and four times more often. These findings underscore concerns about the Supreme Court's recent decisions deregulating campaign finance.

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 spent to support and oppose candidates for office comes from the wealthy and from interest groups, who typically have different preferences and priorities than most Americans (e.g., Bawn et al. 2012; Page, Bartels, and Seawright 2013). Concerning many, the role of these organized interests and wealthy donors in American elections has continued to grow in the wake of the Supreme Court's decision in *Citizens United*, which allowed independent groups to spend unlimited sums influencing elections. Such groups spent over $1 billion in 2012, more than doubling since 2008 (Center for Responsive Politics 2014).

The potential that contributions to such efforts facilitate access to influential policy makers has long garnered special concern. Such concerns arise for at least two reasons. First, access to powerful officials is often necessary for influencing policy, even if it is not sufficient (Hansen 1991). In order to make one's case to a policy maker, one typically needs to secure her attention first (Hall and Wayman 1990; Hasen 2012; Lewis et al. 1998; Wright 1990). Second, when senior policy makers grant access, they expend a scarce legislative resource. Policy makers' time is finite, so when they decide to spend time hearing the concerns of some individuals, they have less time to hear others' (Hall 1996; Hall and Deardorff 2006). A chief concern voiced by critics of the American system of campaign finance has thus been that it encourages legislators to spend time attending to the concerns of donors and groups who represent their interests, giving those who can afford to contribute an advantage in the policy process (e.g., Barber 2014; Fouirnaies and Hall 2014a, b; Hall and Deardorff 2006; Hall and Wayman 1990; Lessig 2011; Miler 2009, 2010; Page et al. 2013; Powell 2012, 2013).

To many casual observers and the public at large, it seems obvious that contributions facilitate this sort of special treatment (Gilens 2012; Lessig 2011). Indeed, the overwhelming majority of Americans who disapprove of Congress name "corruption" of the political

We thank Chris Cotton, Bo Cowgill, Alan Gerber, Don Green, Jacob Hacker, Greg Huber, Gabe Lenz, Eric Schickler, Rob Van Houweling, the three anonymous referees, and seminar participants at Yale and Berkeley for helpful feedback. All remaining errors are our own. David Broockman acknowledges the National Science Foundation Graduate Research Fellowship Program for support. Replication data and code are available at the AJPS Data Archive on Dataverse (http://dvn.iq.harvard.edu/dvn/dv/ajps).

American Journal of Political Science, Vol. 00, No. 0, January 2015, Pp. 1–14

 DOI: 10.1111/ajps.12180

process by campaign contributions as their main grievance (Grimmer and Powell 2013). Political actors themselves also appear to believe contributions facilitate access to senior policy makers. For example, Fouirnaies and Hall (2014a) find that firms with higher exposure to regulation give more to incumbents, suggestive that they believe their contributions facilitate access. Likewise, when legislators lose committee seats, companies that these legislators' former committees regulate tend to decrease their contributions to them, suggestive of a spot market for access (Grimmer and Powell 2013). Groups also tend to give more to legislators who occupy powerful positions within legislatures, further consistent with the notion that contributors see special value in gaining access to powerful decision makers (Fouirnaies and Hall 2014c; Powell 2012).

Evidence that the public, donors, and organized interests *believe* contributions facilitate access to policy makers has continued to build, yet evidence establishing the causal link between contributions and policy makers' actual behavior has been less forthcoming. Indeed, one of the few points of agreement in the literature on campaign finance is that the available evidence is insufficient for assessing the causal impacts of contributions, both in general (e.g., Baumgartner et al. 2009; Fox and Rothenberg 2011) and with regard to access decisions specifically (Austen-Smith 1995; Langbein 1986). The concern is endogeneity. For example, correlations between money and access may be spurious, simply reflecting shared ideology. If legislators tend to meet with allied interest groups and interest groups tend to give to their legislative allies, a correlation between contributions and access may persist even if legislators never grant access due to contributions (Grenzke 1989); legislators might prefer meeting with their allies even if their allies had not contributed to them. In addition, as we discuss at greater length below, if legislators attempt to court donations from donors who have not yet given to them, comparisons between the treatment donors receive from legislators to whom they have and have not given may substantially understate the impacts of contributions. 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).

In this article, we present a unique field experiment that helps overcome these methodological challenges. Our experiment considers whether political organizations can gain superior access to policy makers because their members have contributed to campaigns.[1] In the experiment, a political organization attempted to schedule meetings between 191 congressional offices and its members who were 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 policy makers made themselves available considerably more often ($p < .01$). Because whether congressional offices knew the prospective attendees were donors was randomly assigned, we can be confident that these differences reflect a causal effect of this information on their access decisions. These results have significant consequences for our understanding of how campaign contributions can facilitate political interest groups' prominence in the policymaking process (e.g., Baumgartner 2009; Gilens and Page 2014).

In the pages that follow, we first discuss why existing research on this subject has struggled with endogeneity. We then describe our experimental design and how it overcomes these issues. After presenting our results, we conclude with a discussion of their significance for ongoing academic and public debates.

Campaign Contributions and Access to Policy Makers: Theoretical Expectations and Empirical Challenges

To many members of the public, it seems obvious that campaign contributions facilitate preferential treatment from lawmakers (Gilens 2012; Lessig 2011). Political actors themselves also appear to share this view, as their behavior seems consistent with the belief that their contributions facilitate access to powerful policy makers (e.g., Fouirnaies and Hall 2014a, b; Grimmer and Powell 2013; Powell 2012).

The reasons to suspect campaign contributions help political actors secure special attention from policy makers are natural enough. First, money can buy expensive campaign advertising that increases legislators' chances of reelection (Gerber et al. 2011; Green and Gerber 2008). Legislators appear aware that money can affect whether they are reelected, as they choose to spend several hours each day raising it (Grim and Siddiqui

[1] As we discuss, the question we seek to answer is whether political organizations can gain superior access to policy makers because their members have contributed to campaigns. However, rather than randomly assigning whether an organization has donors among its membership, a thorny task, we instead randomly assigned whether an organization labeled its members as donors, which only organizations with donors among their membership can do. We discuss the advantages and disadvantages of this approach at greater length below.

2013).[2] It is thus natural to suspect that, when making decisions about whose policy concerns to consider, legislators sometimes face trade-offs between satisfying voters and satisfying contributors that they resolve in contributors' favor (Stratmann 1991). In addition, policy makers may also view contributions as a signal of shared ideology, shared concerns, preference intensity, or expertise (e.g., Hall, Beckmann, and van Houweling 2009). Legislators thus may view individuals or groups who have contributed to them as likelier to provide useful information about how they can make good public policy or about who cares about a policy.

We would add one more class of mechanisms to this list, which helped inform our experimental design (although our design does not definitively establish it). Existing work has typically considered potential 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 other favors in exchange (see Hall, Beckmann, and van Houweling 2009 for a review). 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? We see several reasons to expect that legislators may give special treatment, such as access, to donors to *other* campaigns as well. First, legislators might privilege an individual's concerns to the extent that that individual *might* donate to their campaign, not only to the extent that that person has already given. If legislators see donations to other campaigns as a signal that an individual might give to their own, contributions to one legislator may facilitate access to others. 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. Finally, legislators may likewise view contributions to other legislators as signals of their ideology or expertise.

In the case of both quid pro quo and these more indirect mechanisms, many explanations for why legislators might privilege contributors' concerns do not turn on legislators' desire to raise money per se. However, they still have troubling implications for political equality insofar as they suggest some political actors can more easily afford to secure access with policy makers than others.

Inspired by these concerns, a large literature has examined the role of contributions in shaping legislators' decisions. Yet, contrary to the expectations outlined above, this literature is famously replete with null findings and cause for skepticism (e.g., Bronars and Lott 1997; McCarty and Rothenberg 1996; Wawro 2001; Wright 1989, 1990; for reviews, see Persily and Lammie 2004, fn. 53; Stratmann 2005), including on the question of whether contributions facilitate access (e.g., Austen-Smith 1995; Chin 2005; Chin, Bond, and Geva 2000; Langbein 1986). In fact, null findings are more common than significant ones in this literature: Ansolabehere, de Figueirdo, and Snyder's (2003) survey of the empirical literature on campaign finance more generally finds that over 75% of empirical research is inconclusive or reaches opposite conclusions (although see Stratmann 2005).

Despite the abundance of empirical research on how contributions affect legislators' behavior, nearly all this research shares a common and well-recognized shortcoming. Indeed, one of the few points of agreement in the empirical literature on campaign finance is that the available evidence is insufficient for assessing the impacts of contributions, both in general (e.g., Baumgartner et al. 2009; Fox and Rothenberg 2011) and with regard to access specifically (Austen-Smith 1995; Langbein 1986). The concern is endogeneity.

To appreciate this concern, suppose interest groups choose to contribute to politicians who they believe share their preferences in order to help them gain reelection and continue advancing their shared priorities. Moreover, suppose legislators prefer to meet with interest groups whose preferences they share in order to gain useful policy knowledge (Hall and Deardorff 2006). Both of these propositions seem likely, yet they would result in a substantial association between campaign contributions and access even if contributions do not themselves secure this access. Correlations between money and access thus may reflect significant omitted variable bias due to shared ideology between contributors and legislators. If legislators tend to meet with allied interest groups and interest groups tend to give to their legislative allies, a correlation between contributions and access may persist even if legislators never grant access due to contributions (Grenzke 1989); legislators might prefer meeting with their allies even if their allies had not made contributions.

On the other hand, the null observational results found in much of the literature could also mask significant causal effects. Suppose contributors tend to give most to legislators whose support is in doubt but that their contributions do succeed in plying them to support their cause. Moreover, suppose contributors neglect

[2]Votes on campaign finance reform proposals also appear to reflect legislators' own understandings of how it will affect their campaigns' bottom line (Bender 1988).

giving to legislators whose support is already ensured, seeing no reason to squander funds where they are unneeded. In this state of affairs, we might observe no or even a negative relationship between contributions and legislative outcomes because contributors have chosen their levels of contributions on the basis of how *unlikely* a legislator is to support their cause otherwise (e.g., Baumgartner et al. 2009).

The possibilities we raised regarding how legislators may react to observing that an individual has given to *other* legislators also present further methodological challenges. Suppose a donor can gain access to many legislators by showing that he or she is willing to contribute through donations to only a few legislators. In this case, comparisons between legislators to whom a donor gives and does not give may underestimate the full effect of the contributions, as legislators who have received no donations (yet) may still respond strongly to the possibility of courting (or preventing) future donations. 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). To understand whether legislators grant access to those who have contributed, the better comparison is thus how legislators treat donors and nondonors, not how individuals who donated are treated by the legislators to whom they have and have not contributed.[3]

In summary, the common weaknesses of existing studies can be summarized as their inability to observe the following counterfactual: How would the legislators to whom political actors contribute have behaved if these actors had not contributed? Because political actors strategically choose to which legislators they give, and legislators respond strategically to this activity, comparisons between legislators to whom political actors do and do not contribute may simply reflect other unobserved differences. Likewise, the kinds of individuals who tend to give to campaigns may gain more access in general for other reasons, such as their other political activities or by virtue of influential positions they hold.

In this article, we present a field experiment that helps address this persistent challenge. The experiment considers whether political organizations can secure superior access to policy makers because their members have given to campaigns (we discuss the precise counterfactual we identify more in a moment). As discussed, existing research has difficulty answering this question because the legislators to whom interest group members contribute and do not contribute—and which interest groups have members who do and do not contribute—are likely to attain differing levels of access to policy makers for other reasons as well. In the experiment, however, we randomly assigned whether legislators were made aware that a political organization's members had contributed and compared the level of access its members obtained when this information was and was not revealed. This random assignment ensures that other factors that may lead policy makers to grant access to certain groups and not others are held constant by design. In the next section, we describe the experiment in greater detail.

Experimental Design

The experiment considers whether a political organization can obtain superior access to policy makers because its members have contributed to campaigns. Previous natural experimental research suggests that organized groups *believe* such contributions help secure access (Fouirnaies and Hall 2014a, b, c; Grimmer and Powell 2013; Powell 2012). Here, we provide one of the starkest tests to date of whether such contributions actually do secure superior access.

In the experiment, an access-seeking political organization attempted to arrange meetings between high-level congressional officials and its members residing in their districts who had previously given to political campaigns. However, the organization randomly assigned whether it revealed that the prospective attendees were political donors. Randomly assigning whether congressional offices were *aware* the prospective meeting attendees were campaign donors helps us shed light on what would have occurred if we had randomly assigned whether these individuals actually contributed, but without the potential logistical and legal challenges of doing so (Ludwig, Kling, and Mullainathan 2011).[4]

In contrast to existing research, this field experimental approach 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 long-standing concerns that associations between political actors' contributions and policy makers'

[3]Readers may recognize this logic as that illustrating how stable unit treatment value assumption (SUTVA) violations can bias estimated effects toward zero (Aronow and Samii 2013).

[4]As with other "mechanism experiments" (see Ludwig, Kling, and Mullainathan 2011 for a definition), there are other mechanisms for the effect we uncover that do not reflect one of the broader theoretical issues at stake. For example, congressional offices could have seen the act of revealing an individual's donor status as a signal that the group itself cared more about the issue rather than have been reacting to the information that the individuals were donors. With this said, our results still have similar implications for political equality, as groups that do not have many donors among their memberships cannot employ this efficacious strategy.

behavior are driven by political actors' propensity to give to existing allies and do not reflect policy makers' greater attention to them *because* they have donated.

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 U.S. liberal political organization with around 3.5 million members. It attempts to arrange meetings between its members and their legislators from time to time, although prior to the experiment it had never discussed its members' donation history in these invitations.

The sample for the experiment included every U.S. representative of one political party who had not already cosponsored the bill—191 representatives in all.[5]

For the experiment, in each of these 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 legislator's office. Members of the political organization who had donated were recruited via e-mail and informed that their previous contributions might be revealed to the office of their members of Congress when the meetings were requested.

Random Assignment

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

First, to maximize the limited statistical power inherent in a study of members of the finitely sized U.S. 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 same bill in a previous congress, the number of years the legislator had served in Congress, the legislator's ideology (Clinton, Jackman, and Rivers 2004; Jackman 2013), the number of members of the political organization that resided within 40 miles of the district office where the meeting would be held, and Barack Obama's share of the 2012 two-party presidential vote in the district.[6] Legislative offices were allocated to these blocks using blockTools in R, which seeks to construct the blocking scheme that minimizes the differences between observations within each block as determined by the Mahalanobis distance (Moore and Schnakenberg 2013).

Legislative offices were then randomly assigned to treatment conditions within each of these 64 blocks, with one office in each block being randomly assigned to the Revealed Donor condition and the other two offices assigned to the Constituent condition. Treatment offices were selected by block—assigning each observation a random number with Stata 12's runiform() function and selecting the office in each block with the lowest random number for the treatment group.

Table S2 in the supporting information presents randomization checks and reports the expected covariate balance across the treatment conditions. As expected by random assignment, offices in the Revealed Donor condition were similar on values of all covariates to offices in the Constituent condition. An omnibus test for the existence of differences on these covariates was insignificant, as expected ($p = .92$, $x^2 = 1.44$, 5 *df*).

Implementation Procedure

After the random assignment of the 191 offices to the Constituent and Revealed Donor conditions was completed, the organization next sent the scheduler[7] in every congressional office a meeting request via e-mail, the typical medium by which this group attempts to arrange meetings between its members and policy makers. Schedulers' e-mail addresses were obtained from the National Journal's *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 DC, both were e-mailed at once. If a scheduler did not appear in either source, the organization collected the e-mail address of the staffer most likely to have the

[5]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 the legislator to discard the request. This reduced the sample size from 192 to 191 legislators.

[6]Note that using 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 (Gerber and Green 2012).

[7]It is the job of congressional schedulers to "make recommendations on proposed meetings" (Petersen 2010, 12).

duties of a scheduler (e.g., office manager, personal assistant, or district manager). In the e-mails, the organization requested a meeting in the congressperson's district office listed in the *Almanac*. If multiple offices were listed, the organization chose the office with the greatest number of staffers.

For offices randomized to the Constituent condition, the meeting request described the prospective attendees as "local constituents," making no reference to their contribution history. However, 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 were changed. The e-mail sent to legislators is shown below, with the text in boldface being assigned based on the legislators' experimental condition (this text was not boldfaced in the actual e-mails):

> SUBJECT: Meeting with local [**campaign donors/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 [**active political donors/concerned constituents**] have expressed interest in meeting with the Congressman, in person or by phone from the [CITY] office.
>
> These [**donors/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]

Note that neither version of the invitation suggested that the prospective attendees had given to the particular legislator in question; rather, the invitation simply revealed that they had contributed to campaigns in the past in general. This choice allowed the appearance of a quid pro quo arrangement to be avoided, although congressional staff may have interpreted the message as indicating that group members had given to this legislator. (Moreover, in neither condition did the organization supply the names or contribution histories of the attendees in advance of the meeting.) In this sense, this treatment is more subtle than many other ways contributions may influence policy makers. The manipulation here consisted of changing only a few words in an e-mailed meeting request. By contrast, large campaign donors often tender generous checks directly to legislators and form personal relationships in the process. Any preferential treatment these kinds of interaction help facilitate seems unlikely to be smaller than that garnered by the manipulation we consider here.[8]

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, and so on. As the organization employees could not be fully blind to the treatment condition of each office given the differing subject lines of the e-mail exchanges with schedulers, these interactions potentially represented an opportunity for organization employees to introduce bias by communicating with 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 prespecified and standardized its 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

[8]Describing the attendees as campaign donors in general instead of as donors to the particular legislator in question also allows the results to speak to one of the questions raised by the Supreme Court in the *Citizens United* decision—namely, whether lawmakers would grant preferential treatment because an individual or a group had donated in general, even in the absence of a quid pro quo arrangement (which, to be clear, was not present here).

e-mail that required a response that had not been prespecified, 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 e-mail as a reply to the original e-mail 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 e-mails were sent. If the scheduler offered scheduling for a date the organization did not originally request, the organization reiterated its request to hold the meeting on the originally specified dates.

If a meeting was scheduled, the organization invited the 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 e-mailed 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 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.

An advantage of this experimental design is that it does not present many of the ethical concerns that often arise with field experiments on elites (Grose 2014). 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 when they agreed to attend the meetings that the fact they had donated may be 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 associations 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 interest groups 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 revealing that members of an interest group have donated on legislators' access decisions, without the need to control for factors that lead certain interest groups or their members to donate.[9]

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.

Outcome Measurement and Ex Ante Ranking of Access Quality

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.

[9]Random assignment of the knowledge that the prospective meeting attendees had donated also potentially 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 may 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.

Table 1 Results: 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 = .07
Chief of Staff	0.0%	4.7%	2.4%	12.5%	p = .006
Legislative Director or Deputy Chief of Staff	3.1%	6.2%	5.5%	18.8%	p = .005
DC-Based Legislative Assistant or Local District Director	25.2%	18.8%	30.7%	37.5%	p = .17
Other District-Based Staffer	12.6%	10.9%	43.3%	48.4%	p = .26
No Meeting	56.7%	51.6%	100%	100%	—

To test our hypothesis concerning legislative access, 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 the supporting information):

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, such as 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.[10]

[10]This decision rule was set in advance of the experiment.

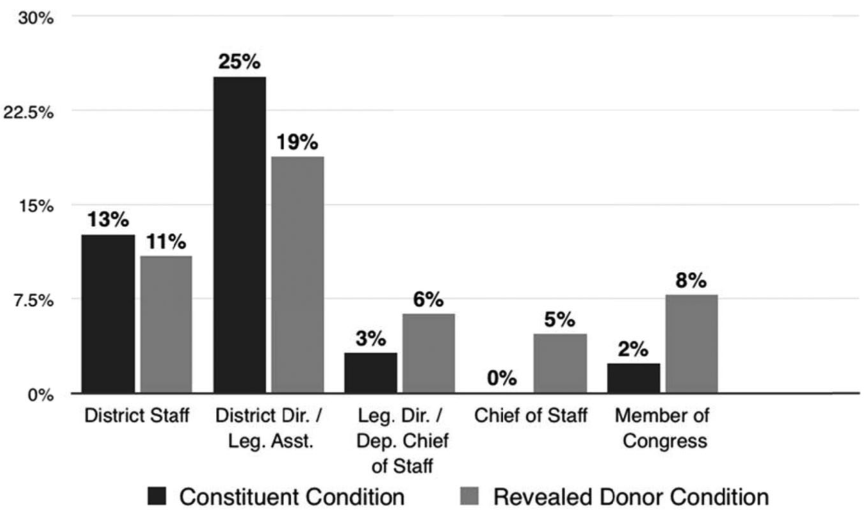
Results

Overall and irrespective of the experimental condition, about half of the congressional offices granted meetings in response to the request. We expected that many offices would not grant meetings at all, given the substantial time constraints faced by members of Congress and their staff (Fitch and Goldschmidt 2005; Goldschmidt 2011). Moreover, in order to minimize the risk of deviations from the experimental protocol, the protocol was designed to limit the number of interactions between organization staffers and congressional offices. For example, if the organization had made telephone calls to schedulers when requesting the meetings or made an appearance in congressional offices to do so, it is likely that more meetings would have been secured. However, these additional interactions would also have presented the opportunity for staffers to deviate from the experimental protocol. Confining the requests to two e-mails ensured that interactions with offices in each condition could be kept entirely similar except for the randomly assigned treatment.

Descriptive statistics for the share of offices that provided access to officials at each level are presented numerically in Table 1 and graphically in Figure 1. The first two columns of Table 1 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 1 shows that senior policy makers attended the meetings considerably more frequently when

FIGURE 1 Access Gained to Congressional Staffers, by Experimental Condition



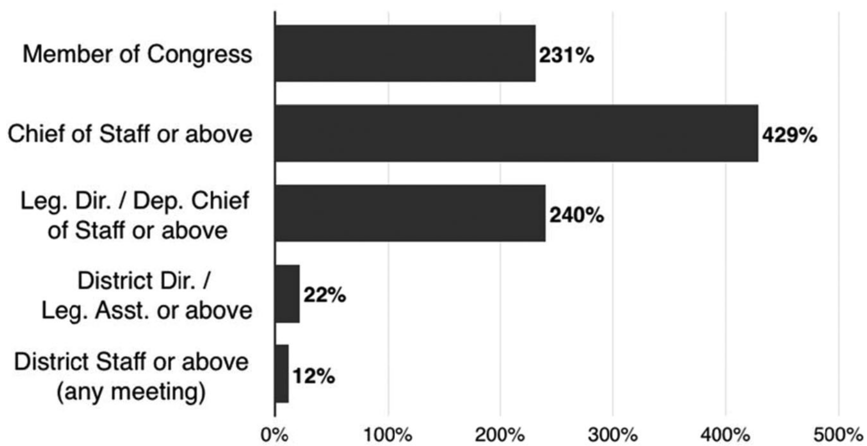
congressional offices were informed that the meeting attendees were donors. Only 2.4% of offices arranged meetings with a member of Congress or chief of staff when they were told the attendees were merely constituents, but 12.5% did so when the attendees were revealed to be donors. In addition, 18.8% of the groups revealed to be donors met with any senior staffer, whereas only 5.5% of the groups described as constituents gained access to a senior staffer, a more than threefold increase.[11] To compare, 56.7% of the offices in the Constituent condition scheduled any meeting, as opposed to 51.6% in the Revealed Donor condition.

Figure 2 helps one to appreciate the magnitude of these differences in percentage terms. The figure displays the percentage increase in access to offices in the Revealed Donor condition granted relative to offices in the Constituent condition. These percentages can also be obtained by comparing the fourth column of Table 1 to the third. 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 informed only 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. Although these percentage differences are associated with considerable uncertainty, they help illustrate that, despite the low baseline level of access to the most senior staffers, the treatment effect of revealing the attendees had contributed was considerable.

To assess how likely these differences in access would have arisen by chance, each row in the final column of Table 1 displays the exact p-value (obtained using randomization inference, a procedure that yields exact p-values for experiments even in small samples; Fisher 1935; Keele, McConnaughy, and White 2012; Pitman 1937) 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. Randomization inference calculates p-values under the sharp null hypothesis of no treatment effect for all observations. This procedure first assumes the sharp null hypothesis: that the observed outcome for the Revealed Donor condition would have been the same had each office been

[11] As discussed, out of concern for designing an experimental protocol that staffers could reliably follow, the organization's campaign for this bill was much less forceful and elaborate than it might have been had it been only interested in securing cosponsors. Perhaps as a result, the number of additional cosponsors the bill received was very low: Only nine legislators cosponsored the bill during the study period, with the same share in the treatment and control groups. Because the baseline is so low, however, this estimate is associated with such considerable uncertainty that it says little about the effect of access on cosponorships. Moreover, as several congressional offices that never replied to the meeting requests at all ultimately did cosponsor the bill, it is likely that many of these cosponsorships would have occurred anyway and were not due to the meetings. (There was also never a vote held on this bill.) These results thus tell us little about the effects of contributions on legislative outcomes per se. Efforts to affect legislators' behavior more forcefully would provide a fruitful focus for future experiments (e.g., Bergan 2009).

FIGURE 2 Percentage Increase in Access Revealed Donors Gained, at or above Each Level.



Note: Each bar shows the percent increase in the share of meetings that occurred at or above each level in the Revealed Donor condition relative to the Constituent condition. These can be obtained by comparing the fourth column of 1 to the third column.

assigned to the Constituent condition instead, and vice versa. Assuming this sharp null hypothesis allows us to compute a complete schedule of potential outcomes, from which we can simulate randomizations under the blocked assignment procedure to estimate the sampling distribution under the sharp null hypothesis of no effect. We then calculate a p-value by calculating the probability of obtaining an average treatment effect as large as the one observed in the experiment at each level of access that is granted. The code for this procedure is given in the supporting information and is 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 Revealed Donor condition by chance ($ps < .01$). These differences are statistically significant even when accounting for strict multiple testing using a Bonferroni correction with a significance level of .05 (because we conduct five comparisons and these are significant at the .01 level).[12] It is also unlikely that members of Congress themselves would have met with groups in the Revealed Donor condition more often by chance ($p = .07$).

As an alternative way to analyze our data and address the potential for multiple testing bias, 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 the supporting information for details). This test yielded a p-value of .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.

[12]Because these tests are correlated, this conservative correction overstates our uncertainty. We nevertheless present this very conservative correction because it makes extremely minimal assumptions and demonstrates the robustness of our results.

Discussion

The causal effects of campaign contributions on legislators' behavior have famously evaded rigorous quantification. The public and contributors themselves appear to believe that contributions facilitate special access to lawmakers, yet a clear demonstration that lawmakers provide such special access has been elusive.

Existing literature has struggled to identify the causal effects of contributions because it has had difficulty ruling out the possibility that policy makers tend to grant access to contributors merely because contributors give to legislators whose actions they support (e.g., Langbein 1986). Our experiment helps overcome these notorious barriers to inference and clearly documented policy makers granting preferential access to interest group members because they have contributed to campaigns. When a political organization randomly assigned some congressional offices to be informed that its members seeking a meeting were donors, the group had much greater success securing meetings with senior policy makers.

These results shed new light on how contributions can help organized interest groups obtain access to influential policy makers. By virtue of having members who had given to political campaigns, the organization in this study was able to obtain far superior access to influential policy makers. But not all organizations or individuals can be described as campaign contributors, as many Americans cannot afford to contribute to campaigns. The difference between how congressional offices reacted to the meeting requests when they were and were not aware that organization members had donated thus provides a window into the reception organized groups that contribute to campaigns receive in Washington, shedding light on how they succeed in influencing politics (e.g., Gilens and Page 2014) and suggesting troubling implications for political equality.

With regard to the implications of our experiment for how policy makers interact with the broader constellation of interest groups vying for their attention, we find it particularly striking that we detected significant differences in how congressional offices behaved across the experimental conditions in this experiment because the manipulation 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, representatives of interest groups and wealthy donors often tender generous checks directly to legislators. The differences uncovered here seem unlikely to be smaller in such situations.

We hasten to note several limitations of our study. First, one experiment cannot definitively establish *why* senior officials more readily avail themselves to individuals because they have donated (Bullock, Green, and Ha 2010). The experiment was designed to capture multiple reasons why legislators might attend to a request because those associated with it had donated and suggests future work should take a broad view of the ways donations may affect legislators' behavior. However, this design left open questions regarding mechanisms. Legislators may anticipate that meeting with donors increases their likelihood of donating to them, but legislators may also expect donors to have greater policy expertise than constituents, to have stronger preferences on the issues at hand, or to 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. On the other hand, that groups who have campaign donors among their membership can gain access to policy makers that their counterparts without campaign donors cannot has the same social consequences regardless of why this inequity arises. Nevertheless, the question of mechanisms is ripe for future research.

Our research also leaves open the relative importance of contributions. In our experiment, congressional offices always were informed the potential attendees were constituents, so we could not ascertain the relative importance of being a constituent and being a contributor (e.g., Chin 2005; Chin, Bond, and Geva 2000). Future research that considered both elements would help paint a fuller picture of the scope of contributor influence.

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 increased access translates into tangible influence. Certainly, professional lobbyists behave as though access to legislators is an important condition for influencing policy (e.g., Hansen 1991), and reformers have focused on disparities in access in part because the view that access facilitates influence is so widely accepted. 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.

In closing, we return to the implications of our findings for political equality. In recent years, scholars have become increasingly concerned with the ways rising economic inequality may translate into political inequality (e.g., Bartels 2009). The hypothesis that political actors and organized interest groups can command influential policy makers' attention 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: Many of the organization members who wanted to express their concerns to senior policy makers in our experiment would not have been able to do so if policy makers had not been informed that they had donated. Our results thus suggest that the vast majority of Americans who cannot afford to contribute to campaigns in meaningful amounts are at a disadvantage when attempting to express their concerns to policy makers.

The disadvantage that groups representing most Americans appear to face when it comes to gaining access to policy makers is crucial. Access to powerful officials is often necessary for influencing policy, even if it is not sufficient (Hansen 1991): In order to articulate one's concerns to legislators, one needs their attention first (Hall and Wayman 1990; Hasen 2012; Lewis et al. 1998; Wright 1990). Access also appears to bring concerns to the top of legislators' minds (Miler 2009, 2010). Yet legislators and senior officials have limited time and attention, so when they spend time hearing the concerns of some groups that ask to meet with them, they necessarily devote less to others' (Hall 1996; Hall and Deardorff 2006). And there is good reason to believe that contributors' concerns are systematically different. Few Americans can afford to contribute to campaigns, whereas those who can afford to do so have markedly different preferences and priorities than the broader public (Page, Bartels, and Seawright 2013; Page and Seawright 2014). To the extent that who policy makers hear from is determined by who can afford to donate, policy makers' worldviews may be significantly distorted in ways that benefit the wealthy (Hall and Deardorff 2006; Lessig 2011).

These findings have particular importance in light of recent Supreme Court decisions striking down significant campaign finance regulations, such as *Citizens United* and *McCutcheon v. FEC*. Assuming the Court does not revisit its decisions in these cases, our findings raise concerns about their implications for political equality. Those who seek to influence policy value access so highly because it allows them to make arguments and requests to powerful decision makers (Hansen 1991). Our experiment suggests that campaign finance rules allow those who can afford to donate to political campaigns a special advantage to obtain this coveted opportunity. By allowing significantly more spending in American elections, the Court's recent decisions have the potential to worsen this disparity.

Schattschneider (1960) famously noted that those who tend to participate in politics are more likely to speak with an "upper class accent." Our findings provide some of the clearest evidence to date that the American campaign finance system helps amplify their voices.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher’s website:

Randomization Inference Procedure
Table S1: Rules for responses to Congressional offices
Table S2: Relationship between Treatment Assignment and Covariates