Do Politicians Grant Favors to Donors? A Field Experiment on Campaign Contributions and Access to Congressional Officials*

Joshua L. Kalla†

Advisor: Professor Alan S. Gerber

May 22, 2014

Undergraduate Senior Thesis

Please do not cite without permission of author.

*I thank David Broockman and Becky Bond at CREDO Action for sharing data, Eden Pang for research assistance at CREDO Action, my advisor, Alan Gerber, and Baobao Zhang for helpful feedback, and seminar participants in the Yale Political Science Senior Colloquium and the ISPS Experiments Workshop for comments and suggestions. Data and replication code for the primary analysis are available at `https://sites.google.com/site/joshualkalla/replication-archive`. All remaining errors are my own.

†BA/MA Student, Department of Political Science, Yale University, `joshua.kalla@yale.edu`

Abstract

Scholars, jurists, and citizens have long been concerned with the question of whether donations lead to an undue distortion of lawmakers' behavior. After decades of research, methodological shortcomings have prevented a clearer understanding of the role of campaign donations on political outcomes from emerging. I begin with a methodological critique of observational research's attempts to answer causal questions and then report the first randomized field experiment to investigate the effect of campaign contributions on access to congressional officials. A political organization attempted to schedule meetings between 191 Members of Congress and groups of their constituents who had donated to political campaigns. However, the organization randomly assigned whether it informed legislators' offices that the requested meeting would be with previous donors. The revealed donors gained considerably more access to senior congressional officials than those thought to be only constituents. These findings have important implications for ongoing legal and legislative debates.

Contents

"Several states have recognized the public character of campaign contributions and expenditures by surrounding them with wholesome legal restrictions and prohibitions . . . [In Colorado] contribution by any other person or corporation to or for any party committee or any candidate for such offices, and also the acceptance of any such contribution, is made a felony punishable by imprisonment" (Aylsworth 1909).

"We need not ascribe evil motives to the men who in the recent campaign contributed great sums to the Democratic, Progressive and Republican parties, and we may as freely acknowledge the probability that these gifts were as honorably received as offered" (Weyl 1913).

1 Introduction

The role of money in politics has been a long-standing concern in the study of modern American political science. Since the discipline's earliest days, scholars have grappled with what, if any, influence money has and how best to curb that influence. Talk of contribution limits and bans, public financing, and the amplification of grassroots donations to address the potential for corruption from reliance on large donations is nearly as old as the modern political campaign (e.g., Aylsworth 1909; Weyl 1913). Since the early 1970s, research on campaign finance has received extensive coverage in the major political science journals, as seen in Figure 1.[1] The interest in campaign finance has not been limited to scholars of American politics. Congress passed three major bills regulating money in politics (Federal Election Campaign Act in 1971, major amendments to FECA in 1974 and the Bipartisan Campaign Reform Act in 2002) and the Supreme Court has heard 24 cases since the 1970s, everything from *Cort v. Ash* in 1975 to *McCutcheon v. FEC* in 2014 (Barnes 2012).

Discussion and study of campaign contributions has been extensive.

[1]Using the Web of Science Citation Report, I conducted a search in the top political science journals of all recent articles on campaign finance. The search criteria were: TOPIC: (campaign finance) OR TOPIC: (campaign contribution) OR TOPIC: (campaign donation) Refined by: SOURCE TITLES: (JOURNAL OF POLITICS OR AMERICAN JOURNAL OF POLITICAL SCIENCE OR AMERICAN POLITICAL SCIENCE REVIEW) Timespan: 1970-2014.

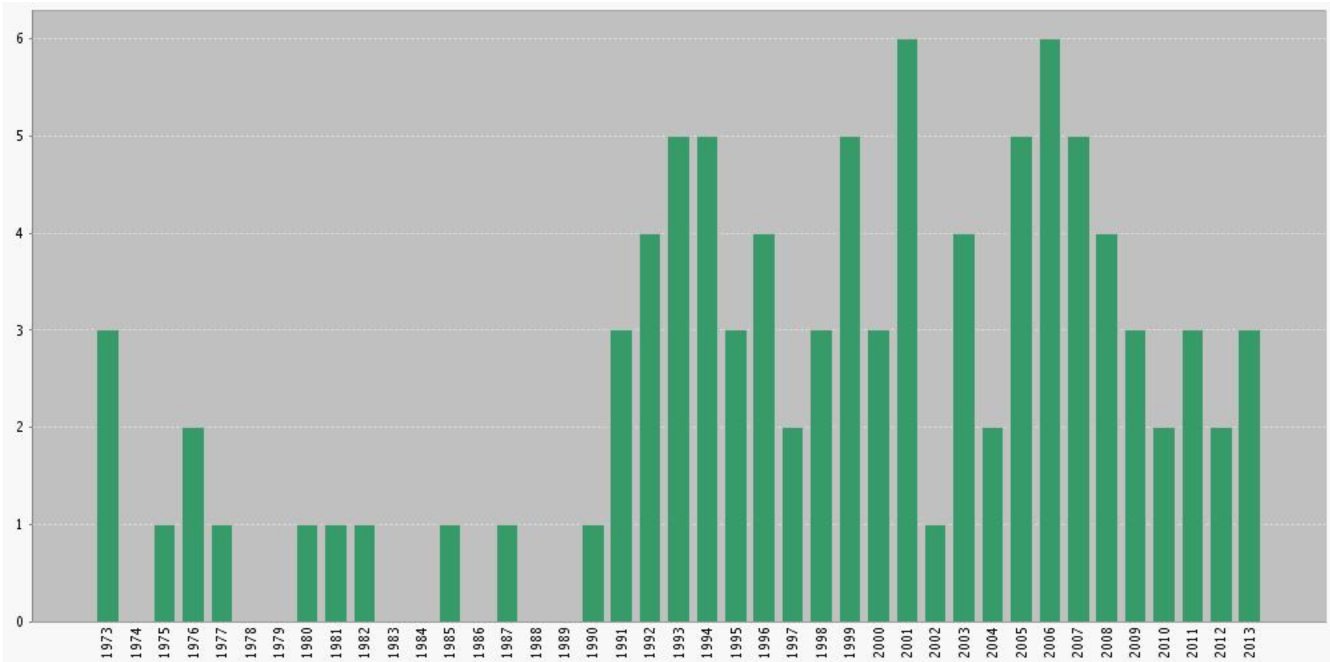

Figure 1: Count of campaign finance articles in each year at *APSR*, *AJPS*, or *JOP*. Graph created using the Web of Science Citation Report

Yet, after over one hundred years of scholarship, no consensus has been reached on what exactly money does within the political system. In the 2010 Supreme Court case striking down limitations on independent expenditures by corporations and labor unions, *Citizens United v. FEC*, Justice Kennedy, writing for the majority, concluded that "there is only scant evidence that independent expenditures even ingratiate" (588 U.S. 205 2010, at 45). Yet, there is no lack of empirical social scientific research attempting to establish such a link between contributions and legislative behavior (for a review, see Ansolabehere et al. 2003). In this paper, I review the literature on money in politics, explain how, because of the methodological limitations of observational research, the literature's findings are inconclusive, and then present new, causally valid field experimental evidence showing that being a campaign contributor increases access to senior congressional officials.

Decades of research have attempted to quantify the relationship between campaign contributions and legislative behavior, with a primary focus on whether donations influence congressional roll-call voting. This literature has mainly taken the form first used by

Silberman and Durden (1976): regress roll-call votes on campaign contributions from organized interests like labor unions or business groups and determine if the coefficient is statistically significant in the expected direction (e.g., a large number of labor contributions increases support for pro-labor policies like the minimum wage). The literature has differed in the roll-call votes investigated, the interest groups included, the control variables considered, and the statistical assumptions made in a model. These decisions have manifested themselves in over thirty years of research, dozens of papers, and many contradictory conclusions.

In a review of 36 prior studies, Ansolabehere et al. (2003) find no consistent evidence that campaign contributions cause changes in roll-call voting. In a subsequent reanalysis of Ansolabehere et al., Stratmann (2005) conducts a more formal meta-analysis in which he weights each study according to its coefficient size, direction, and significance level. Using this methodology, Stratmann finds that campaign contributions do in fact have an effect on legislative voting behavior. Thus, two different reviews of the same published literature arrive at two distinct conclusions as to the effect of money in politics. This divide in the empirical literature lends support to Justice Kennedy's claim of "scant evidence." In this paper, I present experimental evidence to help overcome this impasse in the literature.

2 Critique of Observational Evidence

As mentioned above, the extant literature on money in politics relies primarily on econometric techniques to investigate the role of campaign donations on congressional roll-call votes.[2] Specifically, this literature broadly tends to employ one (or more) of the following three approaches when making causal claims from empirical data of the effect of campaign donations in the legislative process: multivariate regression with control

[2]Grimmer and Powell (2013) avoid these problems by exploiting the quasi-natural experiment of involuntary removal of committee members after party losses in an election. I discuss this method in greater detail below.

variables, instrumental variables, or panel data.[3] In this section, I explain how the reliance on model-dependent observational investigations of the role of money in politics has led to the contradictory findings noted in Ansolabehere et al. (2003) and Stratmann (2005).

The study of money in politics is generally a causal question: what effect do campaign donations from interest groups have on legislative outcomes concerning those groups? In an ideal world, we would observe how a given legislator behaves both after receiving a campaign donation and in the counterfactual world in which she does not receive the donation. Following the notation of Gerber and Green (2012), I denote two *potential outcomes* for each legislator: one in which she receives the donation, $Y_i(1)$, and one in which she does not, $Y_i(0)$. In this hypothetical world, for each legislator, I could estimate the causal effect of the treatment by taking the difference between the two potential outcomes: $\tau_i = Y_i(1) - Y_i(0)$. That is, the treatment effect is the difference in outcomes between the two states of the world in which the legislator does and does not receive the contribution.

But the fundamental problem of causal inference is that researchers are only able to observe one potential outcome at a given time and not both (Holland 1986). Thus causal inference requires tools to overcome this problem of missing data and a set of assumptions as to how a legislator would behave had we observed the other potential outcome.

One proposed solution is to use an experiment and random assignment of a treatment to overcome the fundamental problem of causal inference. Random assignment allows for an unbiased estimate of a treatment effect (Gerber and Green 2012). This is because under random assignment of a treatment, such as campaign donations, the set of legislators who receive the treatment are expected to have the same potential outcomes as legislators who

[3]Another approach has been to rely on qualitative reports of the effects of money in politics. Like the quantitative observation analysis, these accounts also have conflicting findings. Some from former lobbyists (e.g., Abramoff 2011) claim that donations are essential to purchasing access and support, while other accounts from political scientists (e.g., Drutman 2010) deny the possibility of *quid pro quo* transactions. Although anecdotal cases of corruption, or the lack thereof, are perhaps more easily identifiable and prosecutable, social scientists, legislators, and jurists are generally more interested in whether the system of campaign finance leads to systematic corruption in government (Persily and Lammie 2004).

do not. More formally, treatment assignment is statistically independent of a legislator's potential outcomes and her background characteristics, $\mathbf{X}_i : Y_i(0), Y_i(1), \mathbf{X}_i \perp\!\!\!\perp D_i$, where D_i is treatment assignment. $D_i = 1$ in the treatment group when the legislator receives a campaign donation and $D_i = 0$ in the control group when she does not receive the donation.

Though it is not possible to observe both $Y_i(0)$ and $Y_i(1)$ for each legislator, because random assignment is independent of a legislator's potential outcomes, we can conclude that the expected $Y_i(1)$ potential outcome among the treated legislators is, in expectation, equivalent to the expected $Y_i(1)$ potential outcome for the entire set of legislators. Formally this means that for any given random assignment of the entire set of potential assignments:

$$E[Y_i(1)|D_i = 1] = E[Y_i(1)]. \tag{1}$$

But the same property of independence that allows us to draw the above conclusion among the treated legislators also allows us to conclude that the expected $Y_i(1)$ in the control group is, in expectation, equal to the expected $Y_i(1)$ for the entire set of legislators. Even though we do not observe the $Y_i(1)$ in the control group, because random assignment to the control condition was independent of potential outcomes, we can say that *had* these control legislators received the treatment, their expected $Y_i(1)$ potential outcome would be the same as the expected $Y_i(1)$ potential outcome for the entire set:

$$E[Y_i(1)|D_i = 0] = E[Y_i(1)]. \tag{2}$$

By combining Equations (1) and (2), we can see that random assignment suggests that the treatment and control groups have the same expected $Y_i(1)$ potential outcome, even though we only observe that outcome for the treatment group:

$$E[Y_i(1)|D_i = 1] = E[Y_i(1)|D_i = 0] = E[Y_i(1)]. \tag{3}$$

The same logic applies to $Y_i(0)$. Even though we only observe this potential outcome for the control group, because of random assignment, we can nevertheless conclude, in expectation, that control legislators have the same expected potential outcome $Y_i(0)$ that treated legislators would have had if they had been untreated:

$$E[Y_i(0)|D_i = 0] = E[Y_i(0)|D_i = 1] = E[Y_i(0)]. \tag{4}$$

As mentioned above, the individual level treatment effect is the difference in outcomes between the two potential states of the world for each legislator: $\tau_i = Y_i(1) - Y_i(0)$. An average treatment effect can then be defined as:

$$\begin{aligned} ATE &= \frac{1}{N}\sum_{i=1}^{N}\tau_i & (5)\\ &= \frac{1}{N}\sum_{i=1}^{N}Y_i(1) - Y_i(0) & (6)\\ &= \frac{1}{N}\sum_{i=1}^{N}Y_i(1) - \frac{1}{N}\sum_{i=1}^{N}Y_i(0). & (7) \end{aligned}$$

Because $Y_i(1)$ and $Y_i(0)$ are random variables, using the property of expectations, we can rewrite the ATE as

$$ATE = E[Y_i(1)] - E[Y_i(0)]. \tag{8}$$

But $Y_i(1)$ and $Y_i(0)$ cannot simultaneously be observed for each legislator. Then substituting using Equations (3) and (4), we can state that

$$ATE = E[Y_i(1)|D_i = 1] - E[Y_i(0)|D_i = 0]. \tag{9}$$

This can then be rewritten as:

$$ATE = \frac{1}{m}\sum_{i=1}^{m}[Y_i(1)|D_i = 1] - \frac{1}{N-m}\sum_{i=m+1}^{N}[Y_i(0)|D_i = 0]. \quad (10)$$

where of the N legislators, the first m are assigned to receive the contribution and the remaining $N - m$ are assigned to a control group that does not receive a donation. In other words, we can estimate the average treatment effect by taking the difference in the averages of the observed outcomes in the units assigned to treatment and control groups - the *difference-in-means* estimator. This overcomes the fundamental problem of causal inference because we can observe both of those quantities of interests, which lead to an estimate for the average treatment effect. But as noted above, with the exception of the research presented in this paper, the extant literature on money in politics has not relied on random assignment of a treatment to estimate a causal effect. In the remainder of this section I review and critique three common techniques to conduct causal inference and demonstrate how they lead to biases that are not present in experiments.

The most common approach, the use of multivariate regression with control variables, typically involves regressing roll-call votes on political contributions and a set of control variables meant to account for confounding covariates such as the political strength of the donating group or the partisanship of the legislator. In a review of 36 articles published in economics and political science from 1976-2002, Ansolabehere et al. (2003, see Table 1) find that two-thirds rely primarily on multivariate regression. The problem with this approach is that in order for the estimated effect of donations on roll-call votes to be unbiased, all potential confounders must be accounted for (Angrist and Pischke 2009, Chapter 3). In an experiment with random assignment, we are able to assume that $Y_i(0), Y_i(1), \mathbf{X}_i \perp\!\!\!\perp D_i$. Multivariate regression attempts to recover this property by assuming that by conditioning on control variables, it is as if the treatment had been randomly assigned. We can formally denote this conditional independence assumption as: $Y_i(0), Y_i(1) \perp\!\!\!\perp D_i | \mathbf{X}_i$. This is a strong assumption which cannot be empirically verified and, when violated, can lead to a biased estimate of the causal effect.

To demonstrate this bias, assume a "true" regression model that has two explanatory variables and an error term (the following proof comes from Wooldridge 2009, chapter 3):

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + u \tag{11}$$

Because this is the "true" model, we can assume that the conditional independence assumption, along with the additional OLS assumptions, holds and can interpret β_1 as an unbiased causal effect. Imagine if we were forced to exclude x_2, perhaps because of missing data. We would thus estimate the model:

$$y = \widetilde{\beta}_0 + \widetilde{\beta}_1 x_1 \tag{12}$$

Wooldridge notes that $\widetilde{\beta}_1 = \widehat{\beta}_1 + \widehat{\beta}_2 \widetilde{\delta}_1$, where $\widehat{\beta}_1$ and $\widehat{\beta}_2$ are the estimators from the "true" multivariate regression and $\widetilde{\delta}_1$ is the estimator from the regression of x_2 on x_1 for the sample. Thus $\widetilde{\delta}_1$ is a fixed, nonrandom value. To calculate the bias from omitting x_2, we take the expected value of $\widetilde{\beta}_1$:

$$E(\widetilde{\beta}_1) = E(\widehat{\beta}_1 + \widehat{\beta}_2 \widetilde{\delta}_1) \tag{13}$$

$$= E(\widehat{\beta}_1) + E(\widehat{\beta}_2)\widetilde{\delta}_1 \tag{14}$$

$$= \beta_1 + \beta_2 \delta_1 \tag{15}$$

This implies that $\widetilde{\beta}_1$ is biased by $\beta_2 \delta_1$. In this simple case, it is possible to speculate as to the sign and direction of the bias by reasoning whether $\text{Corr}(x_1, x_2)$ and β_2 are positive or negative (Wooldridge 2009, Table 3.2). More generally however, this is not possible. In most observational studies, when there are more than two explanatory variables, we are unable to determine the magnitude or the direction of the bias. This leads to what Gerber et al. (2014) call the "Illusion of Observational Learning Theorem." When one is unsure of the bias in an observational study, classical standard errors understate the true level of uncertainty in an estimate. Gerber et al. (2014, p. 15) go so far as to argue that "If one is entirely uncertain about the biases of observational research, the accumulation of observational findings sheds no light on the causal parameter of interest." Because the extant literature on money in politics has relied overwhelmingly on observational research in which there is great uncertainty around the biases, it is unclear what, if any, causal effect may exist.

This formal notation of how an omitted variable or misspecified regression leads to a biased estimate is understood most intuitively in Figure 2. This figure, a directed acyclical or causal graph, denotes the causal relationship between variables (see Pearl 2000; Morgan and Winship 2007). Here I apply the general reasoning of multivariate regression to the specific case of attempting to obtain unbiased estimates of the effect of campaign donations. In this figure, T is the donation or treatment variable, Y is the roll-call vote or outcome, C1-8 are potential *observed* associations between T and Y, and U are all potential *unobserved* associations between T and Y. In order to eliminate confounding and obtain an unbiased estimate of T on Y, all paths of association linking T and Y, other than the direct causal effect of T on Y, must be eliminated by using control variables in multivariate regression. The problem with this approach is that there are nearly always going to be other variables that are not controlled for. For example, even after controlling for the ideology of the donor, the partisanship of the legislator, the electoral competitiveness of the district, etc., additional unobserved associations still exist - higher political aspirations of the legislator, the ideology of the opposition candidate, strategies internal to the donor, the state of the economy, etc. The key fact is that the U term, the unobserved confounds, always remains. No dataset or model can fully measure and account for all of these variables. For every variable controlled for, a reasonable story can be told about another association between T and Y that was not included in the model.

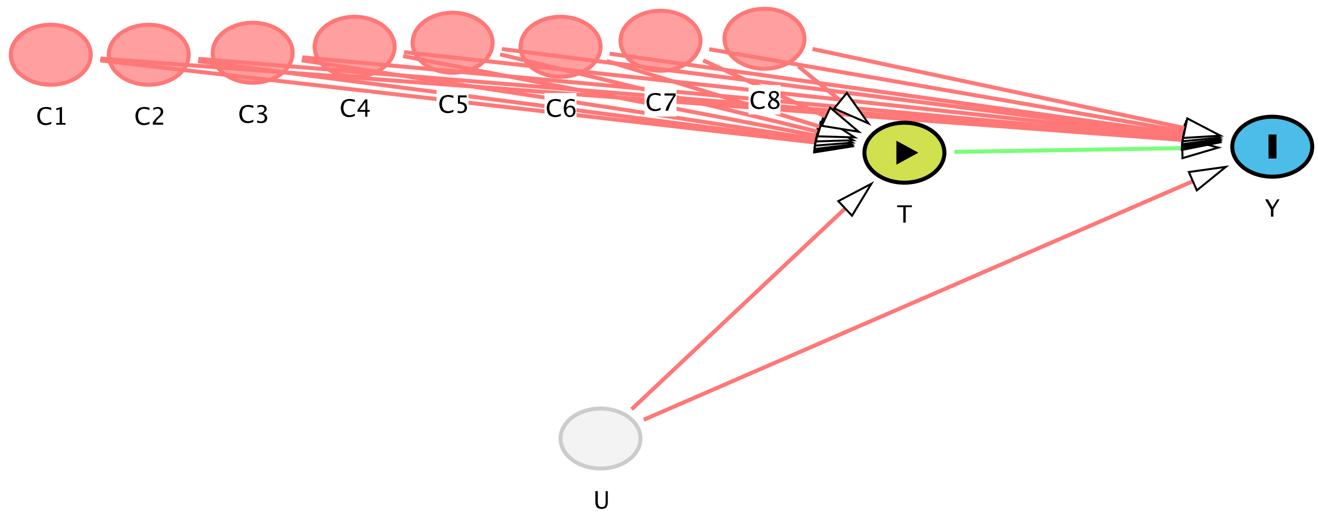

Figure 2: Access to congressional officials by treatment condition: multivariate regression.

In addition to multivariate regression, a second approach to attempt to obtain an unbiased estimate of a treatment effect is the use of instrumental variables. The goal of instrumental variables is to find some as-if randomly assigned factor to estimate the effect of T on Y. Instrumental variables is a powerful tool in causal inference that overcomes many of the problems of multivariate regression, but requires two strong assumptions (Angrist and Pischke 2009): the instrument is unconfounded with the outcome (independence) and the effect of the instrument on the outcome only goes through T and there is no relationship between the instrument and the outcome (exclusion restriction).

Formally, assume we want to estimate the effect of campaign donations on legislative voting. We can represent this as a regression: $y = \beta_0 + \beta_1 x + u$. A valid instrument ($z$) needs to satisfy two assumptions: (1) z is uncorrelated with u, such that $Cov(z, u) = 0$, and (2) z is correlated with x, such that $Cov(z, x) \neq 0$. The second assumption can be empirically confirmed, but because u is unobserved, we can never be certain that $Cov(z, u) = 0$. When $Cov(z, u) \neq 0$, this leads to a biased estimate of β_1. We can see that be rewriting the regression equation in terms of covariances: $Cov(z, y) = \beta_1 Cov(z, x) + Cov(z, u)$. Under the instrumental variables assumptions,

$\beta_1 = \frac{Cov(z,y)}{Cov(z,x)}$, but when the exclusion restriction does not hold, $\widehat{\beta_1}$ is biased: $\widehat{\beta_1} = \frac{Cov(z,y)-Cov(z,u)}{Cov(z,x)}$. Because u is unobserved, we cannot estimate the sign or magnitude of $Cov(z,u)$ and thus we return to the "Illusion of Observational Learning Theorem" (Gerber et al. 2014).

These two assumptions of instrumental variables can be viewed graphically in Figure 3. In this figure, the instrument Z is a valid instrument for the treatment T if and only if Z affects the outcome Y only through T. If a relationship exists through some backchannel U, then the instrumental variable assumptions no longer hold (Zhang 2013).

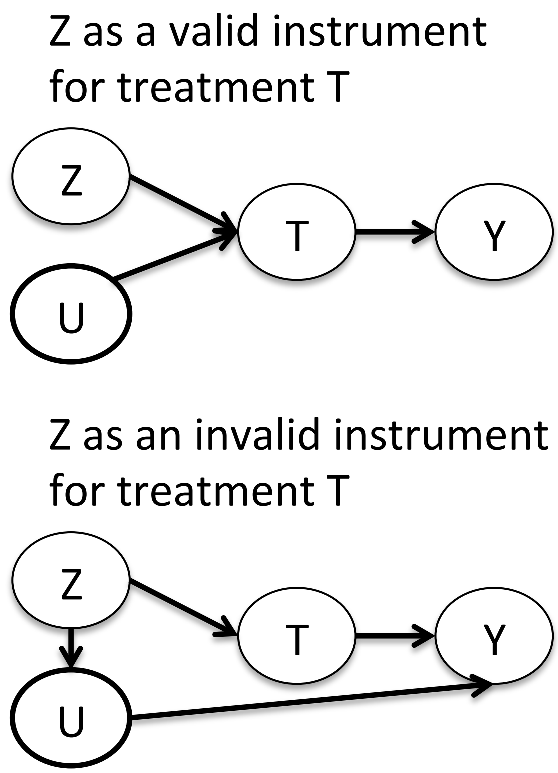

Figure 3: Access to congressional officials by treatment condition: instrumental variables.

Ansolabehere et al. (2003) use two types of instruments in their analysis: the degree of electoral competition and a legislator's measure of "power" in the House of Representatives. To operationalize these instruments, the authors use 6 specific variables: total campaign spending by the opponent, the absolute value of vote-share minus 0.5, a dummy variable if a member ran unopposed, a dummy variable for if the member is a party leader, a dummy variable if the member is a committee chair, and a dummy variable if the member served on the Ways and Means or Energy and Commerce committees. When an analyst chooses to include multiple instruments, *each* instrument must satisfy the exclusion restriction and independence assumptions (Wooldridge 2009, Appendix 15A: Assumptions for Two Stage Least Squares). Like with the case of multivariate regression, it is unlikely that one, let alone six instruments, satisfy these two fundamental assumptions. More specifically, it seems unlikely that these instruments all meet the independence and exclusion restriction assumptions. The researcher wants these variables to be related to contribution levels, but otherwise unrelated to roll-call votes. For example, substantial empirical evidence has found that party leaders are ideologically more extreme than the median legislators in their party, even prior to being elected to leadership positions (Heberlig et al. 2006; Jessee and Malhotra 2010). This suggests that being a party leader (and therefore being more ideologically extreme) is likely to effect roll-call voting in ways other than through T, calling into question the exclusion restriction. Similar stories could be told about how the other instruments might violate the independence assumption or exclusion restriction. Thus instrumental variables is unlikely to be a viable methodological approach when trying to untangle the causal effects of campaign donations.

The third approach is the use of panel data. The intuitive appeal of panel data is that it allows for the inclusion of legislator-specific effects which could account for an individual legislator's predisposition to support a bill. But like multivariate regression and instrumental variables, the assumptions underlying analyses of panel data are often unjustified, leading to unknown biases in the estimates of causal effects. Imagine a case in which we observe one pre-treatment time period (t) and one post-treatment period ($t + 1$) (adapted from Morgan and Winship 2007, chapter 9). In order to estimate the effect of a treatment, D, we can either model a change score:

$$Y_{i,t+1} - Y_{i,t} = a + D_i c + e_i \tag{16}$$

or an analysis of covariance:

$$Y_{i,t+1} = a + Y_{i,t} b + D_i c + e_i \tag{17}$$

Both of those models come with a set of strong assumptions. The change score model assumes that $E[Y_{i,t}(0)|D_i = 1]$ and $E[Y_{i,t}(0)|D_i = 0]$ differ by the same constant k for all time periods t. The analysis of covariance model assumes that the difference between $E[Y_{i,t}(0)|D_i = 1]$ and $E[Y_{i,t}(0)|D_i = 0]$ declines by the same factor r for all time periods, such that by time period $t = \infty$, $E[Y_{i,t=\infty}(0)|D_i = 1] = E[Y_{i,t=\infty}(0)|D_i = 0]$. Morgan and Winship (2007) note that these two models and their assumptions are both potentially reasonable, but the functional form chosen by a researcher is often consequential when estimating a treatment effect. Through simulations, Morgan and Winship (2007, Table 9.2) demonstrate that these assumptions can lead to estimates of a causal effect that are either too large or too small. Without knowing the "true" functional form, a researcher cannot be sure which direction her bias points.

Wawro (2001) reports the effects of labor and corporate PAC contributions on voting behavior for AFL-CIO and the U.S. Chamber of Commerce roll-call votes using panel probit models from both sessions of the 102nd-104th Congresses. Wawro finds inconsistent results ranging from statistically significant in the predicted direction (e.g., labor PAC contributions increase AFL-CIO roll-call votes), to no statistical significance, to statistically significant in the wrong direction (e.g., labor PAC contributions decrease AFL-CIO roll-call votes). This lack of consistency suggests that the models are particularly sensitive to the exact specification, consistent with the critique of panel data presented in Morgan and Winship (2007). Wawro's specific model, which is a correlated random effects estimator, assumes that legislator fixed effects (which are time-invariant) are correlated with all the leads and lags of the explanatory variable, which Wawro specifies as the congressional district's monthly unemployment rate. In addition to the complex, and likely unfounded, functional form assumption, monthly unemployment rates are often measured with substantial error (Feng and Hu 2013). Non-random measurement error in an explanatory variable is likely to lead to a biased estimate of the treatment effect in an unknowable direction (Wiley and Wiley 1970). Furthermore, panel models assume that the treatment (campaign contributions) are as-if randomly assigned conditional on the legislator-specific effects (Angrist and Pischke 2009, p.222). But this assumption is unlikely to hold. Even if a legislator remains the same, her predisposition to vote a certain way may change over time: she may switch parties, gain a leadership position, be redistricted, or simply learn new facts which change the way she views the world. With these critiques in mind, we essentially return to the same problems of bias in causal estimates from observational research.

The existing methodological approaches to studying the effect of money in politics have generally been unable to justify the necessary assumptions to claim unbiased estimates. The key problem of the extant literature estimating the effect of campaign donations on roll-call votes is one of endogeneity. None of these econometric techniques can entirely account for confounding variables which bias the causal estimates in unknowable directions (Green and Gerber 2003). Political donors are strategic in who they choose to support. Depending on how political donors operate, the same underlying potential outcomes can be used to show large positive, negative or no effects of campaign donations on roll-call votes.

Assume for simplicity that a PAC is considering which of five incumbent legislators to support in their reelection bids. The PAC knows how the legislators have voted previously and therefore can predict how the legislators would vote in the absence of campaign donations from the PAC. But the PAC is not sure who an opposing PAC might support and what effect money from the opposing PAC might have on how the legislators might vote. Therefore, we can imagine that the PAC could consider one of four donation strategies: (a) donate to nobody; (b) donate to everybody; (c) donate to supporters in the previous legislative cycle; or (d) donate to opponents in the previous legislative cycle. If strategies (a) or (b) are chosen, we are unable to estimate a causal effect of campaign donations because there is no comparison group. Strategies (c) and (d) can be represented in the below Table 1 of potential outcomes.

This table presents the hypothetical schedule of potential outcomes for five legislators. $Y_{i,t-1}$ is how the legislator voted in the previous legislative session, $Y_{i,t}(0)$ is how the legislator would vote in the current session if she receives no PAC donations, $Y_{i,t}(1)$ is how the legislator would vote in the current session if she does receive PAC donations, and τ_i is the individual-level effect between receiving a PAC donation and receiving no PAC donation.

Depending on our modeling assumptions, we can reach biased estimates of the effect of campaign donations that are either too large or too small. Assuming that PACs choose to randomly assign their campaign donations would be overly strong in light of their compelling strategic interests in target donations. If we assume that the PAC only donates to supporters in the previous legislative cycle (when $Y_{i,t-1} = 1$), then we observe $Y_{i,t}(1)$ for legislators 1-3 and $Y_{i,t}(0)$ for legislators 4-6. Among the legislators receiving the PAC donation, the average outcome is $\frac{1+0+1}{3} = 0.67$, and among the legislators who do not receive a PAC donation, the average outcome is $\frac{0+0+1}{3} = 0.33$. This results in an average treatment effect of $0.67 - 0.33 = 0.34$ (Gerber and Green 2012). This is an upwardly biased estimate of the true average treatment effect of 0.17.

Alternatively, if the PAC only donates to opponents in the previous legislative cycle, we observe $Y_{i,t}(0)$ for legislators 1-3 and $Y_{i,t}(1)$ for legislators 4-6. Among the legislators not receiving the PAC donation, the average outcome is $\frac{1+0+1}{3} = 0.67$, and among the legislators who do not receive a PAC donation, the average outcome is $\frac{1+0+1}{3} = 0.67$. This results in an average treatment effect of $0.67 - 0.67 = 0$. Under this scenario, we would

Table 1: Illustration of potential outcomes for legislative voting depending on whether legislators receive PAC donations. Entries are whether the legislator votes with the PAC (1) or not (0).

Legislator$_i$	$\boldsymbol{Y_{i,t-1}}$ Vote in previous session	Strategy (c)	Strategy (d)	$\boldsymbol{Y_{i,t}(0)}$ Vote in current session if no PAC donation	$\boldsymbol{Y_{i,t}(1)}$ Vote in current session if given PAC donation	$\boldsymbol{\tau_i}$ Treatment effect
Legislator$_\mathbf{1}$	1	Donate	No Donation	1	1	0
Legislator$_\mathbf{2}$	1	Donate	No Donation	0	0	0
Legislator$_\mathbf{3}$	1	Donate	No Donation	1	1	0
Legislator$_\mathbf{4}$	0	No Donation	Donate	0	1	1
Legislator$_\mathbf{5}$	0	No Donation	Donate	0	0	0
Legislator$_\mathbf{6}$	0	No Donation	Donate	1	1	0
Average				0.5	0.67	0.17

conclude that PAC donations have no effect on legislative voting, even though the *true* effect is 0.17. As this simple example demonstrates, a researcher's modeling assumptions vastly change the estimated effect of donations on legislative voting.

In order to overcome the problems associated with estimating a causal effect in the presence of confounding variables, in the present study, I report the results of the first randomized field experiment on campaign donations. The ability of random assignment to lead to an unbiased estimate of the causal effect can be demonstrated from Table 1 (Gerber and Green 2012). Suppose that instead of using a deterministic strategy to make campaign contributions, the PAC randomly assigned 3 of the 6 legislators to receive the money. This leads to:

$$\binom{N}{m} = \frac{N!}{m!(N-m)!} = \frac{6!}{3!3!} = 20$$

possible randomizations. Empirically only one of these possible randomizations would be observed, but using the schedule of potential outcomes from Table 1, it is possible to generate the hypothetical experimental results from each of these 20 possible randomizations. Table 2 presents the sampling distribution of the estimated ATEs generated when three of six legislators listed in Table 1 are randomly assigned to receive a campaign contribution. These are done by calculating the average vote in the donation (treatment) and non-donation (control) groups and calculating the difference-in-means for each possible randomization.

The average estimated ATE is 0.17, which is the true ATE presented in Table 1. This demonstrates numerically what the formal notation above explained: given random assignment, the average estimated ATE recovers the true ATE. Hence, in a randomized experiment, the difference-in-means estimate is an unbiased estimate of the ATE.

In the experiment presented below, random assignment of a treatment, in this case knowledge of a political donation, ensures that treatment and control groups, in

Table 2: Sampling distribution of estimated ATEs generated when three of the six legislators listed in Table 1 are randomly assigned to receive a campaign donation

	Estimated ATE	Frequency with which an estimate occurs
	-0.67	1
	-0.33	3
	0.00	6
	0.33	6
	0.67	3
	1.00	1
Average	0.17	
Total		20

expectation, have the same expected outcome of supporting a bill (Fisher 1935; Gerber and Green 2012). The only difference between the two groups is the presence of the treatment. When random assignment is properly implemented, potentially confounding factors such as party leadership and electoral competition are evenly distributed across the conditions. With random assignment of a treatment, scholars are able to straightforwardly calculate an unbiased estimate of the average treatment effect by taking the difference-in-means between the experimental conditions.

2.1 Why Access?

Much of the literature on money in politics has focused primarily on the effect of campaign donations on roll-call votes. Unlike a legislative calendar for roll-call votes, legislators get to determine with whom they meet. In 2012, the average congressional candidate raised over $1.1 million (Federal Election Commission 2014), and as Lewis et. al (1998) note, such sums cannot be raised at bake sales alone. Legislators have devoted increasing amounts of time to campaign fundraising and consequently have less time to spend on their legislative functions (Alexander 2006; Hall 1996). Thus, as legislators become more discerning in how they spend their time, access becomes an increasingly good measure of how much value they place in a given meeting request (Bauer et al. 1963). In this paper, I present the results of the first randomized field experiment on the effect of campaign donations on *access* to congressional officials. There are several reasons why scholars should choose to focus their efforts on studying access to Congress.

Although the above cited research has focused primarily on roll-call votes as a dependent variable, this does not mean that scholars have neglected to study the role of access in congressional policymaking. The earliest studies in political science concerned themselves exclusively with access as a dependent variable. Herndon (1982, 997) notes that, "Since David Truman's *The Governmental Process* appeared in 1955, political scientists have argued that the chief instrumental goal of interest groups is to secure access to decision makers." Access continues to remain a meaningful subject of inquiry today, both among political scientists and among those who actually seek to influence policy. This sentiment is perhaps best summarized by renowned lobbyist Jack Abramoff, who stated that, "Access is vital in lobbying. If you can't get in your door, you can't make your case."[4]

Theoretically, access is an interesting outcome measure for several reasons. First, scholars have found that access is an important way to convey policy-relevant information. An important strand of the literature on lobbying has argued that lobbying should be modeled as a "legislative subsidy" in which policy information is provided to legislative allies to aid them in achieving their shared objectives (Hall and Deardorff 2006). Thus it is plausible that interest groups donate to legislators who share their interests so that they can gain access, share their expertise, and advance their causes.

In their book on the politics of foreign trade, Bauer et al. (1963) emphasize a Member of Congress's time as one of her most critical resources. At any given moment, a Member of Congress may have to consider dozens of issues, hundreds of bills, committee hearings, constituent services, and reelection efforts. But unlike other aspects of the congressional job, such as the voting and hearing schedules, a Member of Congress has near complete control over her schedule. Thus by focusing on how a Member of Congress allocates her

[4]Planet Money. 2011. Jack Abramoff on Lobbying. National Public Radio, December 20, 2011, http://www.npr.org/blogs/money/2011/12/20/144028899/the-tuesday-podcast-jack-abramoff-on-lobbying (accessed April 24, 2013).

time, scholars can determine how much a legislator cares about a given issue or constituency. When faced with time constraints, a legislator will prioritize meeting with the group that she values most.

Miler (2009) argues that legislators and their staff rely on heuristics when determining the interests of their district. In a series of interviews with congressional staffers, Miler finds that on a given issue, a staffer only recalls the preferences of a small subset of the relevant constituents that might be affected. Importantly, she finds that this recall is biased; legislative staff is more likely to recall information about constituents when those constituents donate money and when they contact the congressional office more frequently. This biased recall of information is then likely to influence which positions are supported in the policymaking process. In subsequent work, Miler (2010) finds that legislators' perceptions of their constituents, based on a biased recall of information, do influence legislators' cosponsorship and voting decisions.

Nevertheless, this literature similarly suffers from the same problems of the observational research on campaign donations and votes. Hall and Wayman (1990) examine the effect of campaign donations on a member's activity during bill drafting and committee markup. While the authors find campaign donations encourage legislative involvement in committee, they do so using two-stage least-squares, with party, a voting index, and the marginality of the district as instruments. Much like the Ansolabehere et al. (2003) model, it is unlikely that the necessary assumptions for valid instruments holds in this case. Other scholarship take similarly limited quantitative approaches.

One prior *survey* experiment was conducted by Chin et al. (2000) to attempt to overcome the problems of endogeneity noted throughout this review. In this experiment, 69 congressional schedulers were asked to put together a mock congressional schedule based on 16 meeting requests and eight free time slots. Three of these requests were randomly assigned to come from a PAC or from constituents. The schedulers needed to decide whether to favor donors or constituents in granting access. The authors find that meeting requests from PACs are not significantly more likely to be granted access than meeting requests from non-PACs.

Although this research is a novel way to overcome the problems of observational research, Chin et al. face several problems endemic to survey experiments. Even though Chin et al. use actual congressional staffers in their experiment, these staffers are nevertheless aware of the manufactured experimental setting (Gerber and Green 2012, ch. 1). Because these schedulers are under the observation of researchers, they might go out of their way to act particularly in line with congressional ethics norms, even though in their actual day-to-day work they do treat donors differently than constituents. These rules state that a "Member's obligations are to all constituents equally, considerations such as political support, party affiliation, or campaign contributions should not affect either the decision of a Member to provide assistance or the quality of the help that is given" (House Committee on Standards of Official Conduct. 2008, p. 308). The overriding ethical norm is to avoid actions which might lead to an "appearance of impropriety."

While no studies exist benchmarking how congressional schedulers respond differently to survey experiments and actual behavior, Findley et al. (2013) conduct parallel field and survey experiments on the availability of anonymous shell corporations, which can be used for money laundering and tax evasion.[5] The authors find that corporate service providers are nearly two-thirds more likely to agree to the anonymous incorporation of a shell corporation in the field experiment than in the survey experiment. Across multiple experimental conditions, the authors find different results depending on the research method. This suggests that when researchers are investigating potentially unethical actions, be it money laundering or the influence of money in politics, respondents may not be entirely forthright in a survey experimental setting. This questioning of the external

[5] Also see Barabas and Jerit (2010) for an attempt to benchmark three national survey experiments on Medicare and immigration to quasi-natural experiments on the same topics occurring at the same times. The authors find vastly different treatment effects in the survey experiment compared to the real-world setting of the quasi-natural experiment.

validity of survey experiments underscores the importance of conducting field experiments in which there is a high "authenticity of treatments, participants, contexts, and outcome measurements" (Gerber and Green 2012, p. 11). The experiment reported in this paper involved the real advocacy efforts of a real political organization seeking actual meetings from congressional offices.

Another approach to overcome the endogeneity problem is to exploit "committee exile," or the involuntary removal of legislators from committees after a wave election (Grimmer and Powell 2013). Grimmer and Powell find that because electoral losses are unevenly distributed across committees, some legislators who survive the wave election are nevertheless "exiled" from their committees in the most recent congressional session. Thus they compare PAC donations to those legislators of the same party who survived the wave election but either were exiled from the committee or managed to remain on the committee. They find that legislators exiled from influential committees see a large decrease in PAC donations from industries regulated by the committee relative to their co-partisans who remained on the committee. They conclude that this PAC behavior, coupled with the fact that the PACs then donate to members of the new majority while maintaining similar donation levels to the new minority, suggests that campaign donations are a way to obtain short-term access and policy influence from members on relevant committees.

Lastly, access is perhaps an easier outcome to measure than a roll-call vote. When planning an experiment, a researcher cannot be certain that a given vote will ever be held.[6] Furthermore, a roll-call vote may not necessarily represent a given legislator's preferences. When casting a vote, a legislator may be constrained by what is on the agenda (Cox and

[6]Butler and Nickerson (2011) conducted an experiment in New Mexico in which they randomly assigned state legislators to receive district-specific polling results to determine if learning constituency opinion affects how legislators vote. While they eventually found a bill for this research, it required several months of searching for legislation that would come up for a vote but with sufficient time to survey constituents and share these results. This required a special legislative session with a pre-announced agenda from the governor. Unfortunately, most roll-call votes do not follow this neat pattern, and even with the governor's agenda, one of the bills never received a vote.

McCubbins 2005), the policy options under consideration (Kingdon 1984), and partisan loyalties (Cox and Poole 2002). Members of Congress, on the other hand, have substantially more control over which meetings they take. A roll-call vote would not necessarily capture an interest group's use of campaign donations to promote an issue to the policy agenda, because even if donations and lobbying have an effect in raising the salience of an issue, this does not necessarily mean that the issue will ever receive a vote.

3 Present Experiment

I now report the results of a randomized field experiment measuring the effects of campaign contributions on access to senior congressional officials (reported elsewhere as Kalla and Broockman 2014). This research overcomes many of the critiques leveled at the extant literature on money in politics, first by using a randomly assigned treatment and second by being conducted in conjunction with an actual lobbying campaign.

3.1 Experimental Design

3.1.1 Setting

During the summer of 2013, staff from CREDO Action implemented a field experiment testing the effect of campaign donations on access to congressional officials. CREDO Action is a progressive political interest organization with over 3.5 million members across the United States.[7] CREDO Action has previous experience cooperating on field experiments with political scientists (e.g., Broockman 2013; Dale and Strauss 2009). This experiment was conducted in the midst of CREDO Action's efforts to pass a bill that would ban a toxic chemical.

[7]CREDO Action mobilizes its members to petition, call, and attend events pressuring political officials to support CREDO Action's progressive policies. See `http://credoaction.com/`.

3.1.2 Treatments

To advocate for this bill, CREDO Action sought meetings between its members and Members of Congress. CREDO Action hoped that through these meetings, Members of Congress would become more likely to support the toxic chemical ban. To arrange these meetings, CREDO Action sent an email to each congressional office requesting a meeting between CREDO Action's members in the congressional district and the Member of Congress.

The treatment was in how CREDO Action referred to their members. The emails varied whether the meeting request stated that the CREDO Action members were "local campaign donors" or "local constituents."[8] The text of the email is presented below. The only changes across the two experimental conditions concerns the language in color, where red represents the Revealed Donor condition and blue represents the Constituent condition.

SUBJECT: Meeting with local campaign donors / 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 active political donors / concerned citizens 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, theyd 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.

[8]Prior to conducting the advocacy effort, CREDO Action collected data on which of its members had previously donated to campaigns and informed them that this information might be used to advance its legislative priorities. In order to avoid deception, only those CREDO Action members who were donors would have attended the meetings with Members of Congress, regardless of treatment condition.

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]

It is important to note how subtle the treatment is. The treatment is simply the changing of a few words in the subject line and body of the email to modestly reflect that the individuals who would attend these meetings are donors. Nowhere in the email does CREDO Action reveal the names of who would be attending the meetings (preventing the office from searching donor databases), whether these so-called donors had donated to this Member of Congress compared to any other political cause, and how much these donors had donated. From the treatment, it is possible that the meeting request came from individuals who had donated $5 to a presidential candidate or $2,000 to the Member of Congress. The scheduler should not have been able to discern who these donors were or how "valuable" they might be to the Member. The only information provided is that these individuals are called "donors," a claim that could not be verified.[9] The meeting requests did not ask congressional officials to engage in illicit behavior and did not contain any explicit or implicit *quid pro quo* arrangements.

Prior to sending the initial emails, CREDO Action employees pre-specified and standardized their responses to follow-up inquiries from legislators so as to maintain the internal validity of the experiment by ensuring that all correspondences with the schedulers would be identical regardless of treatment condition. If 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 experimental condition who wrote the response and added it to the list of standard responses. Table 7 in the appendix lists all of the responses.

[9] A CREDO Action employee accidentally emailed one of the legislators in the Constituent condition intended for a different legislator in the Revealed Donor condition. Upon review, a different employee blind to the treatment condition of this legislator decided that this legislator would be removed from the study and a follow-up e-mail was immediately sent apologizing for the request to the wrong office. This reduced the sample size from 192 to 191 legislators.

If CREDO Action did not receive a reply within three business days, a follow-up email was sent the morning of the following business day. This email used the same subject line as the initial email.

SUBJECT: Meeting with local campaign donors/local constituents about cosponsoring bill to [BILL DETAILS]?
BODY:
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.
Were 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]

If there was no response after this second email, no further action was taken and the legislator was coded as not agreeing to a meeting with CREDO Action.

Once a meeting was scheduled, CREDO Action invited its members in the corresponding congressional district, who had previously self-identified as political donors, to attend the meeting. CREDO Action provided talking points to the meeting attendees and called or emailed every attendee to answer questions about the meeting logistics and talking points. Over 1,200 CREDO Action members participated in these meetings. Twenty-four hours prior the meeting, CREDO Action employees confirmed with the legislative office which staffer would be attending. This was recorded as the outcome variable for congressional access. After the meeting, CREDO Action followed-up with the meeting attendees to confirm that the meeting occurred and that the proper staffer attended. This occurred in all cases.

3.1.3 Study Population

The sample for the experiment consisted of every United States Representative of a particular party who had not already cosponsored the bill according to `http://thomas.loc.gov/` on the date of random assignment.[10] CREDO Action collected email addresses of every eligible Representative's scheduler from the *National Journal's Almanac of American Politics*. When the *Almanac* did not contain a scheduler, *LegiStorm* was used to identify a scheduler or the staffer most likely to have a scheduler's duties (e.g., office manager, personal assistant, or district manager). This resulted in an experimental universe of 192 representatives.

Prior to conducting random assignment, we blocked the legislators into triplets of the most similar legislators based on: an environmental voting score created by *Progressive Punch* based on previous congressional votes (Progressive Punch 2013), whether the legislator had cosponsored this particular bill in a previous congressional session, the number of years the legislator had served in Congress, the legislator's ideal point (Jackman 2013), the number of CREDO Action members 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 congressional district. Blocking, by ensuring balance on observed covariates, increases the precision of treatment effect estimates and allows for an improved experimental design (Moore 2012). The blocking was conducted using *blockTools* in **R** (Moore and Schnakenberg 2013). Within each block of three legislators, we randomly assigned one legislator to the Revealed Donor condition and two legislators to the Constituent condition. Table 3 shows pre-treatment covariates for the two experimental conditions and confirms that there is no relationship between experimental assignment and these covariates. This point is confirmed statistically using a logistic regression to predict experimental assignment as a function of the covariates listed in Table 3. As expected from random

[10]Per our agreement with CREDO Action, we were able to publish the results of this experiment but had to agree to keep the lobbied political party anonymous in order to preserve CREDO Action's relationships with the Representatives.

assignment, a likelihood ratio test with 5 degrees of freedom is nonsignificant ($LR = 1.44$, $p = 0.92$), confirming that the experimental conditions are balanced in terms of these observable covariates.

Table 3: Relationship between treatment assignment and covariates.

	Revealed Donor Condition Mean *S.E.*	**Constituent Condition** Mean *S.E.*
Members within 40 miles of district office	7365.62 *5080.78*	7760.32 *5076.45*
Ideal Point	1.00 *0.38*	1.00 *0.41*
2012 Presidential Vote Share in District	64.88 *11.58*	65.59 *12.32*
Environment Score	88.82 *10.40*	89.58 *10.13*
2012 Total Campaign Receipts	$1,538,232 *961,590*	$1,642,801 *1,016,656*
N	64	127
Logistic Regression x^2 Test	$p = 0.92$, $x^2 = 1.44$ (5 d.f.)	

Note: The rows report mean values with standard error of the mean in italics. The LR test reports the results from multinomial logistic regression of treatment assignment on the covariates, not including the block indicators. Signs on the ideal point estimates, vote share, and environment score may be reversed to anonymize the political party of the legislators.

3.2 Hypotheses

The field experiment was meant to test two hypotheses about political donors and access to congressional officials. Consistent with earlier survey experimental work (Chin et al. 2000), I expect that both the Revealed Donor and Constituent conditions will have equal success in scheduling meetings.

H1: No difference in likelihood of scheduling meetings.

As of 2010, the average Member of Congress employed over 16 full-time paid staffers, with nearly half of them based in district offices (Ornstein et al. 2013). With such large staffs and with so many of them assigned to the district, it seems likely that nearly any reasonable meeting request would be honored. Thus I do not expect to find a difference in how often the two experimental conditions can schedule a meeting with *any* congressional staffer.

Nevertheless, I still expect that the Revealed Donor condition will be granted a different type of access than the Constituent condition. Though there are many staffers who could potentially hold a meeting with an advocacy organization, there are only certain staffers in a congressional office who exert great influence on the policymaking process. Thus I expect that the Revealed Donor condition will be granted higher quality access in their meetings.

H2: Donating to campaigns increases access to more senior Congressional officials.

Prior to conducting the experiment (and thus before any analyses), we developed a ranking of congressional officials in order of policy influence in a congressional office. This ranking was developed in conjunction with the experienced political staff at CREDO Action and echoed the email to congressional schedulers, which requested a meeting with the most senior official available:

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)

I examine these two hypotheses in the field experiment reported below.

3.3 Ethical Considerations

Butler and Broockman (2011) in their experiment on racial discrimination among state legislators describe three ethical considerations that should be taken into account prior to conducting an experiment on public officials: deception, harm, and undue burden. Several steps were taken to ensure that this experiment would minimize the harm from each of these considerations. First, in this experiment, there was no deception. All participants were actual campaign donors who truly wished to lobby in support of this bill and ended up attending actual meetings with their Member of Congress or staff. By working with a political organization to incorporate this experiment into its existing legislative campaign, it was possible to eliminate any need for deception.

Second, I minimize harm to the experimental subjects, Members of Congress and their staff, by maintaining their complete anonymity. No information has been released that might reveal their identities or partisan affiliations. Furthermore, because of the Fundamental Problem of Causal Inference, I do not observe the complete schedule of potential outcomes for the Members of Congress, thus it is impossible to know whether a given legislator responded differently because she received the Revealed Donor treatment. This experiment can only speak to averages.

Third, Butler and Broockman were concerned with placing an undue burden on legislators' time. Similar to the deception consideration, by embedding this experiment into an existing legislative campaign, no congressional time was wasted. This experiment asked Members of Congress to meet with their constituents who wished to lobby for a genuine bill. This experiment requested of Members of Congress what they otherwise would normally do (Fenno 1978).

3.4 Results

Prior to examining results, two coders blind to treatment assignment categorized each meeting using the 1-6 ranking outline above as *Hypothesis 2* (1=Member of Congress;

6=no meeting). Disagreements about how to code staffers from two congressional offices were easily resolved, resulting in perfect agreement across the two coders. If the congressional office did not respond within three weeks of the initial meeting request, the office was coded as not agreeing to a meeting.

I find that 48.4% of the Revealed Donor and 43.3% of the Constituent Condition were able to schedule meetings with anyone in the Member of Congress's office. To test *Hypothesis 1* that there is no difference in the overall likelihood of being able to schedule a meeting, I conduct randomization inference (also known as permutation inference) to calculate an exact p-value under the sharp null of no treatment effect (Keele et al. 2012; Gerber and Green 2012).

Under the sharp null hypothesis, I test whether the treatment effect is zero for all observations, such that $Y_i(0) = Y_i(1)$. In other words, I assume that the observed outcome for the Revealed Donor condition would have been the same had this unit been assigned to the Constituent condition, and vice-versa. This hypothesis states that each outcome would have been the same regardless of treatment assignment. This provides a complete schedule of potential outcomes, similar to Table 1. From this, I can simulate all possible randomizations to produce an exact sampling distribution of the estimated average treatment effect under the sharp null hypothesis.[11] I then calculate a p-value by calculating the probability of obtaining an estimated average treatment effect as large as the one observed in the experiment $(48.4 - 43.3 = 5.1)$.

Replication code for the randomization inference is presented in the Supplementary Materials. Using randomization inference, I find that difference of 5.1 percentage points in scheduling a meeting across the two experimental conditions is not statistically significant ($p = 0.25$). As expected, I find that at a surface level, donors and constituents are granted

[11]In practice, I do not actually simulate *all* possible randomizations. Given 191 Members of Congress with one-third assigned to the Revealed Donor condition, this generates $\frac{191!}{127!64!}$ or 7 x 10^{656} possible randomizations. Instead I approximate the sampling distribution by randomly sampling 100,000 of the possible randomizations.

the same quantity of access to congressional officials.

To test *Hypothesis 2* that meeting requests in the Revealed Donor condition will be granted access to more senior congressional officials than requests in the Constituent condition, I first present descriptive statistics for the percentage of offices that provided access to officials in each ranking category numerically in Table 4 and graphically in Figure 4. Nearly all of the meetings with senior congressional officials came in the Revealed Donor condition. 2.4% of the Constituent condition was able to meet with a chief of staff or the Member of Congress compared to 12.5% in the Revealed Donor condition. Only when district staff or policy assistants are considered can we see parity between the two conditions.

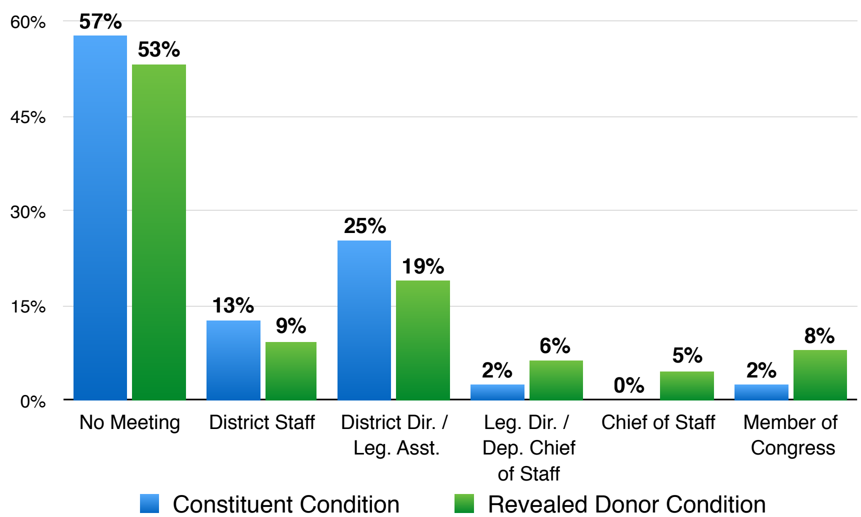

Figure 4: Access to congressional officials by treatment condition.

To quantify the uncertainty around these differences in the quality of access, each row in the far-right column of Table 4 displays the exact p-value, again using randomization inference, that differences as large as the observed differences would have been observed if the treatment had no effect. In other words, I find that, for example, it is highly unlikely that the observed difference of Revealed Donors gaining substantially more access to the Member of Congress or Chief of Staff can be explained by chance ($p = 0.006$).

In addition, I also conduct a Wilcoxon rank-sum test and an ordered probit analysis. Both

Table 4: Access to congressional officials by treatment condition.

Official Group Met	**Constituent Condition** (N=127)	**Revealed Donor Condition** (N=64)	***p*-value:** Revealed donors more likely to meet with officials above this rank
Member of Congress	2.4%	7.8%	
Chief of Staff	0.0%	4.7%	$p = 0.07$
Legislative Director or Deputy Chief of Staff	3.2%	6.3%	$p = 0.006$
Legislative Assistant or District Director	25.2%	18.8%	$p = 0.005$
Other District-Based Staff	12.6%	10.9%	$p = 0.17$
No Meeting	56.7%	51.6%	$p = 0.25$

of these are alternative measures to the above approach. The rank-sum test compares the distribution of outcomes for legislators in the Revealed Donor and Constituent conditions, where $\Delta = G(Y(0)) - F(Y(1))$, where F and G are distribution functions and the null hypothesis is: $H_0 : \Delta = 0$ (Keele et al. 2012, Supplementary Materials). Specifically, to test this hypothesis, the rank-sum test orders outcomes in both conditions from the least (no meeting) to the greatest (meeting with Member of Congress) to calculate W, the sum of the ranks of the Revealed Donor condition. Like with randomization inference of the difference-in-means estimator, this W is compared to all possible permutations (or a random sample of them) of the ranks to generate a distribution of the ranks. A p-value is then calculated by the probability of observing a W greater than that seen in the distribution. Under the rank-sum test, I calculate that it is highly unlikely that the observed difference in ranks (i.e., access to senior congressional officials) is due to chance ($p = 0.07$).

The ordered probit follows a similar logic. An ordered probit uses maximum likelihood to estimate a log-likelihood function of the probability of observing each distinct outcome category (Jackman 2000). Given N units, m potential categories (where m is the highest category, meeting with Member of Congress), and an indicator variable Z_{ij} which equals 1 if $y_i = j$ and 0 otherwise, the log-likelihood is:

$$ln\mathcal{L} = \sum_{i=1}^{N}\sum_{j=0}^{m} Z_{ij} ln[\phi_{ij} - \phi_{ij-1}] \tag{18}$$

where $\phi_{ij} = \phi[\mu_j - D_i\beta]$ or a function relating the randomly assigned treatment variable D_i to the probability of observing a given outcome. The observed data produces one log-likelihood which I then compare to the simulated log-likelihoods from a random sample of the randomizations under the sharp null hypothesis. I then calculate the p-value by seeing how many of the simulated log-likelihoods are greater than or equal to the observed log likelihood. This yields a p-value of 0.05. These three alternative ways to estimate the statistical uncertainty around the effect of changing the word "constituent" to "donor" all yield similar results suggesting that it is highly unlikely that the greater access granted in the Revealed Donor condition would have occurred by chance alone.

3.4.1 Exploratory Analysis

This experiment will hopefully serve as a guide for a larger research agenda that attempts to use experiments to unpack the questions of causality around money in politics. To that end, I present exploratory results that may aid in future hypothesis generation. Due to the growing concern in the social sciences over "fishing," or the post-hoc determination of researchers to search for interesting results in the data, I present these results as "exploratory analysis" (Gerber et al. 2001; Gerber and Malhotra 2008; Ioannidis 2005; Humphreys et al. 2013). While these results fail to reach conventional levels of statistical significance, I nevertheless choose to present all analyses conducted in the interests of full transparency.

First, I examined whether there is treatment effect heterogeneity across various characteristics of the members of Congress. The primary analysis presented above provides a causal estimate of the *average* treatment effect across the experimental universe, but it is also possible to estimate how this effect may vary across sub-populations (Feller and Holmes 2009; Green and Kern 2012). For example, members of Congress who raised more money in 2012 may be more liked to grant access to the Revealed Donor condition relative to a member of Congress who faced less fundraising pressure. Thus by only reporting the average treatment effect, we could be missing important variation in how Members of Congress with different characteristics respond to the treatment. In addition to fundraising pressures, we might expect that more liberal Members of Congress (measured using ideal point estimates), Members with more pro-environment voting records (measured using an environmental voting scorecard), or Members who faced more difficult reelection bids (predicted non-safe House seat)[12] may react different than their relatively more conservative or anti-environment colleagues. To do this, I conducted an ordered probit regression with interaction terms between four covariates of interest - 2012 fundraising totals, ideal point estimates, environment score, and 2014 competitiveness - and the donor treatment indicator. Table 5 presents the average marginal effect of these covariates and their interaction terms on seniority of access granted, using the same coding scheme as above. As can be seen, none of the interaction terms reach conventional levels of statistical significance, thus suggesting that treatment heterogeneity, at least across these covariates, was not present.

[12]Predictions come from the June 13, 2013 update of the University of Virginia Center for Politics House Update, available at `http://www.centerforpolitics.org/crystalball/articles/house-update-tiny-movement-to-republicans/`. These ratings were released prior to the implementation of the experiment and are thus pre-treatment covariates that might explain the congressional responses to the experiment.

Table 5: Average Marginal Effect of Treatment by Covariate Interactions

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Treatment	-0.112*	-0.191	0.0916	-0.00124	0.200	-0.219
	(0.0674)	(0.129)	(0.191)	(0.572)	(0.230)	(0.643)
2012 Fundraising		-6.64e-08*				-6.60e-08
		(3.77e-08)				(4.07e-08)
Ideal Point			-0.0690			0.0312
			(0.100)			(0.146)
Environment Score				0.00291		0.00209
				(0.00389)		(0.00584)
2014 Safe Seat					0.0777	-0.0106
					(0.128)	(0.163)
Fundraising X Treat		4.55e-08				-7.66e-09
		(6.96e-08)				(7.66e-08)
Ideal X Treat			0.203			0.224
			(0.177)			(0.252)
Environment X Treat				-0.00122		0.00658
				(0.00639)		(0.00861)
Safe X Treat					-0.342	-0.267
					(0.238)	(0.281)
Observations	191	191	191	191	191	191

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Second, I investigated whether the Revealed Donor treatment increases the likelihood that a Member of Congress not only grants access but actually follows-through and cosponsors the bill. The email requesting a meeting specifically stated that the purpose was "to tell the Congressman why his base would like him to cosponsor H.R. [BILL DETAILS]." Very few Members of Congress, in either condition, chose to cosponsor the legislation, perhaps because after the meetings, the political group was unable to follow-up with additional information or lobbying efforts. Table 6 presents the rate of cosponsorship across the two experimental conditions and, using randomization inference for the difference-in-means estimator, finds no statistically significant difference ($p = 0.66$). Additional research is necessary to determine whether donors are more likely to exert policy influence in addition to being granted greater access.

Table 6: Cosponsorship Rates by Experimental Condition

	Constituent	Revealed Donor
No Cosponsor	121	61
	(95.28%)	(95.31%)
Cosponsor	6	3
	(4.72%)	(4.69%)
Observations	127	64

Difference-in-means: $p = 0.66$

4 Discussion

The present study provides rigorous experimental evidence suggesting that congressional officials favor the requests of donors over regular constituents. Nevertheless, there are several limitations worth noting. First, this experiment was conducted with one political party, one interest group, and on one piece of legislation. Although the incentives to fundraise are similar across parties and members would suggest that these results ought generalize to other legislative settings, replication is certainly encouraged in order to establish the external validity of these findings. Second, although access is an important variable to measure, this experiment can only speak to one part of the policymaking process. This experiment does not fully investigate what happens after access is granted and whether it leads to changes in other activities, such as bill cosponsorship or a roll-call vote.

Third, this experiment cannot establish the mechanisms explaining *why* senior congressional officials meet more frequently with revealed donors than with constituents. Although congressional officials may meet with donors hoping to raise money, it is also possible that legislators assume something different when they see the word “donor” in a request for a meeting. For example, legislators and their schedulers may believe that donors are more committed to the issue, more likely to convey high-quality policy information because of an assumed education background, or may be more likely to convey relevant information from the district because they are perhaps more active and influential in community politics. This experiment is unable to provide evidence for or against these mechanisms.

Future experiments with multiple treatment arms could begin to examine these competing explanations. For example, researchers could investigate the "quality of information" mechanism by including a non-partisan academic expert condition and the "issue importance" mechanism by comparing campaign *volunteers* to campaign *donors.* Untangling these alternative explanations of the treatment effect observed in this experiment is important to understanding its potentially different welfare implications. For example, if donors are granted access to more senior congressional officials because legislators are "for sale," then policies will reflect the interests of those most able to pay - the affluent (Gilens 2014). But if donors are granted better access because legislators think they convey more relevant policy information, perhaps because of education or commitment to the issue, then it is less clear what the welfare implications may be. Members of Congress, given their time constraints, may seek to take meetings from the "most qualified" individuals on each side of an issue and donor status may serve as a proxy for quality. Thus access may reflect a genuine search for policy information rather than a *quid pro quo* deal exchanging a legislative outcome for a donation. In the case of a genuine search for information, the eventual policy outcome may reflect the consensus of experts rather than the affluent. Answering the *why* is necessary to knowing whether there is a normative shortcoming and what policies might be required to improve the state of American democracy.

Nevertheless, this experiment does begin to respond to recent Supreme Court rulings striking down campaign finance regulations. The majority opinions in both *Citizens United* and *McCutcheon v. FEC* point to the lack of evidence between campaign contributions and corruption. In the *Citizens United* ruling, Justice Kennedy argued that independent expenditures, because they are separate from campaigns, have not and will not lead to corrupting influences in Congress. On the other hand, Fox and Rothenberg (2011) produce a formal model showing that interest groups can influence a legislator's policy decisions without having to make a campaign contribution. In their model, an interest group that donates only to ideological allies is able to move an incumbent closer to its position with just the threat of contributing to a challenger. Thus the existence (and threat) of campaign contributions, regardless of the recipient of the donation, is sufficient to change political outcomes.

This experiment provides some empirical support for the model presented in Fox and Rothenberg (2011). The Revealed Donor condition did not state whether the donors had donated to the contacted Member of Congress. That information, as well as their names which could be matched to a donor database, was not included in any of the communications with congressional staff. Thus the decision to grant more senior access to the revealed donors was made without an explicit *quid pro quo* or an implied promise. Like in the Fox and Rothenberg model, the presence of campaign donations, and not the donations themselves, were sufficient to cause a distortion of political outcomes.

In the *McCutcheon* decision, the majority argued that "constituents support candidates who share their beliefs and interests, and candidates who are elected can be expected to be responsive to those concerns" (572 U.S. 2014, at 2). This sentiment reflects the pitfall of observation research and explains why the decades of such research has been unable to establish a convincing *quid pro quo* relationship between money and politics. Yet this experiment shows that an identical email from the same political organization, thus trying to keep beliefs and interests the same, in which individuals are labelled as either donors or constituents lead to differences in the access granted. Similarly from a normative perspective that seeks to understand the criteria for democratic accountability, this empirical finding does not bode well. As Mansbridge (2003) notes, unequal campaign contributions lead to unequal access that is neither proportionally fair (access is not proportional to the population size of competing interests) or deliberatively efficient (money does not guarantee significant representation of important perspectives).

This experiment also speaks to a larger concern in political science, that of the Tullock Paradox. Tullock (1972), and more recently Ansolabehere et al. (2003), noted that there is a major discrepancy between the amount of political contributions and the value of federal government spending at stake. For example, defense contracting firms and individuals donated $13.2 million to candidates and parties in the 2000 election, while the federal government spent $134 billion on defense procurements. Given these implied astronomical rates of returns, then more firms should enter the political marketplace to compete for government contracts. Ansolabehere et al., as noted above, answer this puzzle by arguing that campaign contributions do not buy votes and that donations should be considered a consumption good rather than a political investment. But the key phrase in framing the Tullock Paradox is the condition that the market between campaign contributions and political favors is competitive.

If we loosen the restriction on there being a competitive market, then it would be plausible for small donors to gain political favors without there being a paradox of economic intuition. In many policy domains, there are no opposing interests donating against one another to create a competitive market. As Mettler (2011) shows, in recent decades, much legislation has come in the form of the "submerged state," government policies that exist away from the public eye and primarily through incentives, subsidies, or payments within the tax code. By focusing on crafting compromises across interest groups and by keeping the process away from public scrutiny, the submerged state has created a market in which much legislation gets passed without competition. Thus, in this noncompetitive political market, donors do not need to oppose one another in order to gain access and favors. Because on many policy issues there are no competing sides with larger checks bidding up the rate for access to a legislator, it seems plausible that a Member of Congress would be sensitive to the requests of even the smallest donors, such as those in this experiment. Nevertheless, this explanation of the Tullock Paradox is not entirely satisfying. It is hard to imagine a world in which the political market is so entirely non-competitive that even the most modest of campaign contributions can purchase access and favors. Future experimental research, perhaps with the random assignment of varying levels of actual campaign donations, can attempt to estimate how much different political favors may cost, or if contributions even do allow one to purchase favors beyond access.

This experiment leaves many questions, namely around causal mechanisms, alternative outcome measures, and external validity, open to future experimentation. It is my hope that political scientists, after viewing an example of a field experiment to study the causal effect of money in politics, will embark on such a research agenda. As has been demonstrated in the critique of the extant observational research and elsewhere (e.g., Green and Gerber 2003; Gerber et al. 2014), observational studies with econometric models are unlikely to move the discipline any closer to a "true" causal estimate. Future scholars ought to consider how the random assignment of information or even of small-dollar donations can be used to answer pressing causal concerns.

5 Supplementary Materials

5.1 Rules for Responses to Congressional Offices

Table 7: Rules for Responses to Congressional Offices

Email Received	Response Rule
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	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: Hi [SCHEDULER], Thanks for checking. The [DISTRICT] office. Best, [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: Hi [SCHEDULER], 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. Thanks, [EMPLOYEE]
Emails asks for contact information	Reply with personal cell phone number
Receive call from staff	Maintain message of the original email. 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 Members 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: Hi [SCHEDULER], I can send you a list of attendees and where they live in the [MCs] 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]. Best, [EMPLOYEE]
Scheduler offers meeting during the August recess	Ask to meet with a staffer during one of the original dates Hi [SCHEDULER], 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? Thanks, [EMPLOYEE]

If they request more information on the bill	Reply with the factsheets: Hi [STAFF], 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]. Let me know if you have any other questions. Thanks, [EMPLOYEE]
If they request more information about the organization	Share number of members, amount donated to non-profit groups, and provide a link to the organizations About Us page.

5.2 R Code for Analysis

```
library(foreign)
setwd("/Users/JLK/Desktop/Research/Senior_Thesis/Donor/R_Code")

genperm <- function(){
  return(c(as.vector(replicate(63,sample(c(0,0,1),3))),sample(c
     (0,0,1),2)))
}

genperms <- function(n.perms){
  return(
    replicate(n.perms,
      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
      )
    )
  )
}

#RI Function 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)
  plot(density(ate.dist.under.sharp.null))
  abline(v = ate)
  return(list(ate = ate, p.value = p.value))
}


#Read in overall data
data <- read.dta("Kalla-Broockman-Donor2013-ANON.dta", convert.
   underscore = TRUE)
data <- data[order(data$block),] #put data in order of blocks
   to make RI easier
data <- data[c(1:135,138:191,136,137),] #put block 46 at end
   since it only has two in it
head(data)


############################
###### Access findings
############################

#Generate permutations for randomization inference
n.perms  <- 100000
perms <- genperms(n.perms)
```

```
data$meeting.scheduled <- ifelse(data$staffrank>0,1,0)

#Had a meeting?
print(ri(data$meeting.scheduled, data$treat.donor, perms))

#Met people at each rank
table(data$treat.donor, data$staffrank)
for(i in 5:1){
  print(i)
  met.this.high <- as.numeric(data$staffrank >= i)
  print(ri(met.this.high, data$treat.donor, perms))
}

#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)
ll.dist.sharp.null <- replicate(100000, get.ll(genperm()))
mean(ll.actual <= ll.dist.sharp.null) #pvalue

#Wilcox W
w.actual <- wilcox.test(data$staffrank ~ data$treat.donor)$
   statistic
```

```
w.dist.sharp.null <- apply(perms, 2, function(perm) wilcox.test(data$staffrank ~ perm)$statistic)
mean(w.actual >= w.dist.sharp.null) #pvalue

#EXPLORATORY ANALYSIS
############################
###### Bill Cosponsorship
############################
table(data$treat.donor, data$cosponsored)
print(ri(data$cosponsored, data$treat.donor, perms))

################################
###### Treatment Heterogeneity
################################
#See STATA Code
```

5.3 Stata Code for Analysis

```
cd /Users/JLK/Desktop/Research/Senior_Thesis/Donor/R_Code
use "Kalla-Broockman-Donor2013-ANON.dta" , clear

label var staffrank "Staff Rank"
label var total_receipts "2012 Fundraising"
label var idealpoint "Ideal Point"
label var enviro_lifetime_overall_pp "Environment Score"
label var treat_donor "Donor Treatment"

```

```
gen tXt = total_receipts*treat_donor
label var tXt "Fundraising X Treat"
gen iXt = idealpoint*treat_donor
label var iXt "Ideal X Treat"
gen eXt = enviro_lifetime_overall_pp*treat_donor
label var eXt "Environment X Treat"

oprobit staffrank treat_donor
margins, dydx(*) post
est store first

oprobit staffrank total_receipts treat_donor tXt
margins, dydx(*) post
est store a

oprobit staffrank idealpoint treat_donor iXt
margins, dydx(*) post
est store b

oprobit staffrank enviro_lifetime_overall_pp treat_donor eXt
margins, dydx(*) post
est store c

oprobit staffrank total_receipts tXt idealpoint iXt enviro_
    lifetime_overall_pp treat_donor eXt
margins, dydx(*) post
est store d
```

```

outreg2 [first a b c d] using oprobit , tex replace sortvar(
    treat_donor tXt iXt total_receipts idealpoint) label
```

Bibliography

Abramoff, Jack. 2011. *Capitol Punishment: The Hard Truth About Washington.* WND Books.

Alexander, Mark C. 2006. "Let Them Do Their Jobs: The Compelling Government Interest in Protecting the Time of Candidates and Elected Officials." *Loyola University Chicago Law Journal* 37: 669-722.

Angrist, Joshua D. and Jorn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton: Princeton University Press.

Ansolabehere, Stephen, John M. de Figueiredo, and James M. Snyder Jr. 2003. "Why Is There So Little Money In U.S. Politics?" *Journal of Economic Perspectives* 17:105-30.

Aylsworth, Leon E. 1909. "Campaign Expenses as a Public Charge." *American Political Science Review* 3:382.

Barabas, Jason and Jennifer Jerit. 2010. "Are Survey Experiments Externally Valid?" *American Political Science Review* 104:226-42.

Barnes, Robert. 2012. "Go Beyond: Supreme Court Campaign Finance Decisions Timeline." *Columbia Law School Magazine* `http://www.law.columbia.edu/magazine/interactive/55569/go-beyond-supreme-court-campaign-finance-decisions-timeline` (April 8, 2014).

Bauer, Raymond, Ithiel De Sola Pool, and Lewis A. Dexter. 1963. *American Business and Public Policy.* Chicago: Aldine and Atherton.

Broockman, David E. 2013. "Mobilizing Candidates: A Field Experiment and a Review." Working Paper.

Butler, Daniel M. and David E. Broockman. 2011. "Do Politicians Racially Discriminate Against Constituents? A Field Experiment on State Legislators." *American Journal of Political Science* 55:463-77.

Butler, Daniel M. and David W. Nickerson. 2011. "Can Learning Constituency Opinion Affect How Legislators Vote? Results from a Field Experiment." *Quarterly Journal of*

Political Science 6:55-83.

Chin, Michelle L., Jon R. Bond, and Nehemia Geva. 2000. "A Foot in the Door: An Experimental Study of PAC and Constituency Effects on Access." *Journal of Politics* 62:534-49.

Citizens United v. Federal Election Commission, 588 U.S. 205 (2010).

Cox, Gary W. and Keith T. Poole. 2002. "On Measuring Partisanship in Roll-Call Voting: The U.S. House of Representatives, 1877-1999." *American Journal of Political Science* 46:477-489.

Cox, Gary W. and Matthew D. McCubbins. 2005. *Setting the Agenda: Responsible Party Government in the U.S. House of Representatives.* New York: Cambridge University Press.

Dale, Allison and Aaron Strauss. 2009. "Don't Forget to Vote: Text Message Reminders as a Mobilization Tool." *American Journal of Political Science* 53:787-804.

Drutman, Lee. 2010. "The Complexities of Lobbying: Toward a Deeper Understanding of the Profession." *PS: Political Science & Politics* 43:834-7.

Federal Election Commission. 2014. "2012 House and Senate Campaign Finance." `http://www.fec.gov/disclosurehs/hsnational.do` (February 24, 2014).

Feng, Shuaizhang, and Yingyao Hu. 2013. "Misclassification Errors and the Underestimation of the US Unemployment Rate." *American Economic Review* 103:1054-70.

Fenno Jr., Richard F. 1978. *Home Style: House Members in Their Districts*. Boston: Little, Brown, and Co.

Feller, Avi and Chris C. Holmes. 2009. *Beyond toplines: Heterogeneous treatment effects in randomized experiments. Technical report.* Oxford, UK: University of Oxford.

Findley, Michael G., Brock Laney, Daniel L. Nielson, and J.C. Sharman. 2013. "Deceptive Studies or Deceptive Answers? Competing Global Field and Survey Experiments on Anonymous Incorporation." Working Paper.

Fisher, R.A. 1935. *The Design of Experiments*. Oliver and Boyd: Edinburgh.

Fox, Justin and Lawrence Rothenberg. 2011. "Influence without Bribes: A Noncontracting Model of Campaign Giving and Policymaking." *Political Analysis* 19:325-41.

Gerber, Alan S., Donald P. Green, and David Nickerson. 2001. "Testing for publication bias in political science." *Political Analysis* 9:38592.

Gerber, Alan, and Neil Malhotra. 2008. "Do statistical reporting standards affect what is published? Publication bias in two leading political science journals." *Quarterly Journal of Political Science* 3:31326.

Gerber, Alan and Donald Green. 2012. *Field Experiments: Design, Analysis, and Interpretation.* W.W. Norton: New York.

Gerber, Alan S., Donald P. Green, and Edward H. Kaplan. 2014. "The Illusion of Learning from Observational Research." In *Field Experiments and Their Critics: Essays on the Uses and Abuses of Experimentation in the Social Sciences*, ed. Dawn Langan Teele. New Haven: Yale University Press, 9-32.

Gilens, Martin. 2014. *Affluence and Influence: Economic Inequality and Political Power in America.* Princeton University Press: Princeton, NJ.

Green, Donald P. and Alan S. Gerber. 2003. "The Underprovision of Experiments in Political Science." *Annals of the American Academy of Political and Social Science* 589:94-112.

Green, Donald P. and Holger L. Kern. 2012. "Modeling Heterogeneous Treatment Effects in Survey Experiments with Bayesian Additive Regression Trees." *Public Opinion Quarterly* 76:491-511.

Grimmer, Justin and Eleanor Neff Powell. 2013. "Money in Exile: Campaign Contributions and Committee Access." Working Paper.

Hall, Richard L. and Frank W. Wayman. 1990. "Buying Time: Moneyed Interests and the Mobilization of Bias in Congressional Committees." *American Political Science Review* 84: 797-820.

Hall, Richard L. 1996. *Participation in Congress.* Yale University Press.

Hall, Richard L. and Alan V. Deardorff. 2006. "Lobbying as Legislative Subsidy." *American Political Science Review* 100:69-84.

Heberlig, Eric, Marc Hetherington, and Bruce Larson. 2006. "The Price of Leadership: Campaign Money and the Polarization of Congressional Parties." *Journal of Politics* 68:992-1005.

Herndon, James F. 1982. "Access, Record, and Competition as Influences on Interest Group Contributions to Congressional Campaigns." *Journal of Politics* 44:996-1019.

Holland, Paul W. 1986. "Statistics and Causal Inference." *Journal of the American Statistical Association.* 81:945-960.

House Committee on Standards of Official Conduct. 2008. *House Ethics Manual*. 110th Congress, 2d Session.

Humphreys, Macartan, Raul Sanchez de la Sierra, and Peter van der Windt. 2013. "Fishing, Commitment, and Communication: A Proposal for Comprehensive Nonbinding Research Registration." *Political Analysis* 21:1-20.

Ioannidis, John P. A. 2005. "Why most published research findings are false." PLoS Medicine 2:696-701.

Jackman, Simon. 2000. "Models for Ordered Outcomes." *Stanford University*. `http://www.stanford.edu/class/polisci203/ordered.pdf` (March 27, 2014).

Jackman, Simon. 2013. "Ideal Point Estimates for the 113th United States House." `http://jackman.stanford.edu/ideal/currentHouse/estimates.csv` (May 30, 2013).

Jessee, Stephen and Neil Malhotra. 2010. "Are Congressional Leaders Middlepersons or Extremists? Yes." *Legislative Studies Quarterly* 35:361-92.

Kalla, Joshua and David Broockman. 2014. "Campaign Donations Facilitate Access to Congressional Officials: A Field Experiment." Working Paper.

Keele, Luke, Corrine McConnaughy, and Ismail White. 2012. "Strengthening the experimenters toolbox: Statistical estimation of internal validity." *American Journal of Political Science* 56: 484-499.

Kingdon, John W. 1984. *Agendas, Alternatives, and Public Policies.* Boston: Little, Brown.

Lewis, Charles and the Center for Public Integrity. 1998. *The Buying of Congress.* Avon Books.

Mansbridge, Jane. 2003. "Rethinking Representation." *American Political Science Review* 97:515-28.

McCutcheon v. Federal Election Commission, 572 U.S. (2014).

Mettler, Suzanne. 2011. *The Submerged State: How Invisible Government Policies Undermine American Democracy.* Chicago: University of Chicago Press.

Miler, Kristina C. 2009. "The Limitations of Heuristics for Political Elites." *Political Psychology* 30: 863-894.

Miler, Kristina C. 2010. *Constituency Representation in Congress: The View from Capitol Hill.* Cambridge: Cambridge University Press.

Moore, Ryan T. 2012. "Multivariate Continuous Blocking to Improve Political Science Experiments." *Political Analysis* 20:460-79.

Moore, Ryan T. and Keith Schnakenberg. 2013. "blockTools: Blocking, Assignment, and Diagnosing Interference in Randomized Experiments," Version 0.5-7, July 2013.

Morgan, Stephen L. and Christopher Winship. 2007. *Counterfactuals and Causal Inference: Methods and Principles for Social Research*. New York: Cambridge University Press.

Ornstein, Norman J., Thomas E. Mann, Michael J. Malbin, and Andrew Rugg. 2013. "Vital Statistics on Congress, Chapter 5: Congressional Staff and Operating Expenses." *Brookings Institution.* `www.brookings.edu/vitalstats` (March 26, 2014).

Pearl, Judea. 2000. *causality: Models, Reasoning, and Inference*. Cambridge: Cambridge University Press.

Persily, Nathaniel and Kelli Lammie. 2004. "Perceptions of Corruption and Campaign Finance: When Public Opinion Determines Constitutional Law." *University of Pennsylvania Law Review* 153:119-180.

Progressive Punch. 2013. "Environment." `http://www.progressivepunch.org/topic-scores.htm?topic=NO&house=house` (January 11, 2014).

Silberman, Jonathan I. and Garey C. Durden. 1976. "Determining Legislative Preferences on the Minimum Wage: An Economic Approach." *Journal of Political Economy* 84:317-29.

Stratmann, Thomas. 2005. "Some talk: Money in politics. A (partial) review of the literature." *Public Choice* 124:135-56.

Tullock, Gordon. 1972. "The Purchase of Politicians." *Western Economic Journal* 10:354-5.

Wawro, Gregory. 2001. "A Panel Probit Analysis of Campaign Contributions and Roll-Call Votes." *American Journal of Political Science* 45:563-79.

Weyl, Walter E. 1913. "The Democratization of Party Finances." *American Political Science Review* 7:178-182.

Wiley, David E. and James A. Wiley. 1970. "The Estimation of Measurement Error in Panel Data." *American Sociological Review* 35:112-117.

Wooldridge, Jeffrey M. 2009. *Introductory Econometrics: A Modern Approach*, Fourth Edition. Mason, OH: South-Western Cengage Learning.

Zhang, Baobao. 2013. "International Dimensions of American Public Opinion: Methodological Critiques and Three Empirical Essays." Working Paper.