How Internal Constraints Shape Interest Group Activities: Evidence from Access-seeking PACs
Online Appendix

Contents

A.1 A Simple Model of PACs' decisionmaking

This expository model is not meant to capture all the complexities of access-seeking PACs' decisionmaking. Nonetheless, it may provide some intuition for how an access-seeking PAC, facing donors with partisan preference, might allocate its PAC contributions across parties. In particular, the main point is that insofar as these allocations affect not only the rate of returns to PAC contributions, but also the amount of fundraising, the allocation that maximizes *total returns* to PAC contributions will generally differ from the one that maximizes the *rate of returns* or the one that maximizes *fundraising*.

Consider a one-shot decision problem for PAC j, which chooses PAC_R_{jt} to maximize Π_{jt}, the total returns to its PAC contributions in cycle t. Suppose total returns equal the product of the rate of returns to PAC contributions and the sum of donations raised from eligible PAC donors, i.e.,

$$\Pi_{jt}(PAC\_R_{jt}) = r_j(PAC\_R_{jt}; X_{jt}) \times \sum_{i \in I_{jt}} Donation_{ijt}(PAC\_R_{jt}; Indiv\_R_i) \tag{A.1}$$

where

- PAC_R_{jt} is the share of PAC j's contributions to Republican (as opposed to Democratic) politicians in cycle t;
- X_{jt} denotes a vector of $K \geq 1$ exogenous factors salient for PAC j's contributions in cycle t (e.g., partisan seat share in each chamber of Congress);
- $r_j : \mathbb{R} \times \mathbb{R}^K \to \mathbb{R}_+$ is a function that maps PAC_R_{jt} and X_{jt} jointly onto a non-negative rate of returns of PAC j's contributions.
- I_{jt} represents the set of individuals who are eligible to donate to PAC j in cycle t;
- $Indiv\_R_i$ is a measure of how Republican-leaning donor i is; and

- $Donation_{ijt} : \mathbb{R} \times \mathbb{R} \rightarrow \mathbb{R}_{+}$ is a function that maps PAC_R_{jt} and $Indiv\_R_{i}$ onto a nonnegative amount of donation that donor i gives to PAC j in cycle t.

To easily characterize the solution to the PAC's decision problem, I assume the following

1. $\Pi_{jt}(\cdot)$ is concave in PAC_R_{jt} and has an interior maximum;
2. $r_{j}(\cdot)$ is concave in PAC_R_{jt} and has an interior maximum; and
3. $Donation_{ijt}(\cdot)$ is concave in PAC_R_{jt} and has an interior maximum.

These assumptions are not necessary for proving the main insights from this expository model.

Given assumption no. 1, the choice of PAC_R_{jt} that maximizes total returns to PAC j's contributions in cycle t satisfies the following first-order condition:

$$\begin{aligned} &\frac{\partial}{\partial PAC\_R_{jt}} r_{j}(PAC\_R_{jt}; X_{jt}) \times \sum_{i \in I_{jt}} Donation_{ijt}(PAC\_R_{jt}; Indiv\_R_{i}) + \\ &r_{j}(PAC\_R_{jt}; X_{jt}) \times \sum_{i \in I_{jt}} \frac{\partial}{\partial PAC\_R_{jt}} Donation_{ijt}(PAC\_R_{jt}; Indiv\_R_{i}) \\ &= 0 \end{aligned} \tag{A.2}$$

In comparison, given assumption no. 2, the choice of PAC_R_{jt} that maximizes the rate of returns to PAC j's contributions in cycle t is given by this alternative first-order condition:

$$\frac{\partial}{\partial PAC\_R_{jt}} r_{j}(PAC\_R_{jt}; X_{jt}) = 0 \tag{A.3}$$

The solution for equation A.3, say $\widehat{PAC\_R}_{jt}$, is not the same as the one for equation A.2 whenever

$$r_j(\widehat{PAC\_R}_{jt}; X_{jt}) \times \sum_{i \in I_{jt}} \frac{\partial}{\partial PAC\_R_{jt}} Donation_{ijt}(\widehat{PAC\_R}_{jt}; Indiv\_R_i) \neq 0$$

In words, this says that if the rate of returns to PAC contributions is positive, and if the allocation of PAC contributions across parties affects donors' willingness to give to the PAC, then the allocation that maximizes said rate of return will generally differ from the one that maximizes total returns to PAC contributions. Note in particular that the second necessary condition—that PAC_R_{jt} affects donors' willingness to give depending on their partisanship—is exactly what I hypothesize and demonstrate in the paper.

We could also consider what choice of PAC_R_{jt} would maximize fundraising, which by assumption no. 3 can be characterized by the following

$$\sum_{i \in I_{jt}} \frac{\partial}{\partial PAC\_R_{jt}} Donation_{ijt}(PAC\_R_{jt}; Indiv\_R_i) = 0 \tag{A.4}$$

The solution for equation A.4, say $\widetilde{PAC\_R}_{jt}$, is not the same as that for equation A.2 whenever

$$\frac{\partial}{\partial PAC\_R_{jt}} r_j(\widetilde{PAC\_R}_{jt}; X_{jt}) \times \sum_{i \in I_{jt}} Donation_{ijt}(\widetilde{PAC\_R}_{jt}; Indiv\_R_i) \neq 0$$

In words, this says that if both the marginal change in rate of returns to PAC contributions (by tweaking PAC_R_{jt}) is nonzero, and that the sum of donations raised is positive, then the allocation that maximizes fundraising will generally differ from the one that maximizes total returns to PAC contributions.

Finally, by comparing equations A.2, A.3, and A.4, I note that whenever $PAC\_R^*_{jt}$ maximizes total returns to PAC contributions but not the rate of returns or fundraising, $PAC\_R^*_{jt}$ must be a strict compromise between these two conflicting objectives. This is because, for $PAC\_R^*_{jt}$ to be the solution to the first-order condition in equation A.2, either

$$\sum_{i \in I_{jt}} \frac{\partial}{\partial PAC\_R_{jt}} Donation_{ijt}(PAC\_R^*_{jt}; Indiv\_R_i) > 0 \text{ and } \frac{\partial}{\partial PAC\_R_{jt}} r_j(PAC\_R^*_{jt}; X_{jt}) < 0$$

i.e., $PAC\_R^*_{jt}$ is "not pro-Republican enough" for maximizing fundraising, but "too pro-Republican" for getting the most bang for the buck for PAC contributions, or

$$\sum_{i \in I_{jt}} \frac{\partial}{\partial PAC\_R_{jt}} Donation_{ijt}(PAC\_R^*_{jt}; Indiv\_R_i) < 0 \text{ and } \frac{\partial}{\partial PAC\_R_{jt}} r_j(PAC\_R^*_{jt}; X_{jt}) > 0$$

i.e., $PAC\_R^*_{jt}$ is "too pro-Republican" for maximizing fundraising, and yet "not pro-Republican enough" for maximizing the rate of returns to PAC contributions.

A.2 Study 1: Difference-in-differences Analysis of Campaign Finance Records

A.2.1 Composition of PACs by FEC classification

As noted in the paper, I examine interest group PACs that OpenSecrets categorizes as business PACs (Center for Responsive Politics 2018). Table A.1 breaks down the distribution of the FEC categories assigned to the 5,284 PACs that OpenSecrets codes as business PACs. As Table A.1 shows, a small minority of these business PACs are not sponsored by a corporation, a trade organization (e.g., the Mortgage Bankers Association), or membership organization (e.g., the National Association of Realtors). Corporate, trade organizations, and membership organizations that advocate for business interests are the focus of much of the existing literature on access-seeking interest groups (see, for example, Romer and Snyder 1994; Cox and Magar 1999; Bonica 2013; Drutman 2015). I therefore exclude PACs in OpenSecrets' data that do not belong in these categories.

Corporation (C)	3263	62%
Trade association (T)	1132	21.5%
NA	387	7.4%
Membership organization (M)	264	5%
Corporation without capital stock (W)	156	3%
Cooperative (V)	52	1%
Labor organization (L)	6	0.1%

Table A.1: Distribution of FEC Classifications in PAC Sample

A.2.2 *Summary Statistics*

Table A.2 reports summary statistics for all variables used in the observational study. I report the characteristics of $Indiv\_R_i$ separately for all partisan donors versus pure partisans only. A partisan donor is anyone who (in addition to having given to an access-seeking PAC) has made at least one itemized donation to Democratic or Republican candidates or party committees, i.e., $Indiv\_R_i \in [-1/2, 1/2]$. In comparison, pure partisan donors have given only to recipients of one party in their entire histories of giving, i.e., $Indiv\_R_i \in \{-1/2, 1/2\}$.

	N	mean	s.d.	min.	max
$Indiv\_R_i$ (all partisans)	270,031	0.12	0.445	-0.5	0.5
$Indiv\_R_i$ (pure partisans)	207,875	0.141	0.48	-0.5	0.5
PAC_R_{jt}	21,344	0.109	0.261	-0.5	0.5
$Give_{ijt}$	3,519,308	0.153	0.36	0	1
$Entry_{ijt}$	2,106,297	0.142	0.349	0	1
$Exit_{ijt}$	789,925	0.275	0.447	0	1

Table A.2: Summary Statistics for Difference-in-differences Analysis

A.2.3 *Timing of itemized individual donations to PACs versus PAC contributions to candidates/parties*

To gauge the extent to which dates of itemized individual donations to PACs trail those of PAC contributions to candidates/parties, or vice versa, one would ideally compare the distribution of these dates for each PAC during each cycle, which would not be feasible for the scope of this online appendix.

Barring that, Figures A.1 shows aggregate density plots for dates of individual donations to PACs versus PAC contributions to candidates/parties, pooling election cycles 1990 through 2016.[1] The light gray density plot denotes relative frequencies of itemized individual donations to PACs by date, with a bin width of 1 day. And the dark gray density plot shows relative frequencies of PAC contributions to candidates and party committees by date, also with a bin width of 1 day.

Figure A.1 shows that both individual donations to PACs and PAC contributions to candidates/parties are relatively spread out across an election cycle. In particular, for the most part individual donations to PACs do not noticeably precede PAC contributions to candidates or party committees. The only notable exception is that there tends to be a spike in PAC contributions around the third quarter of year 2 in an election cycle.

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[1]Both individual-to-PAC donations and PAC-to-candidate/party committee contributions became more frequent over time. To avoid later election cycles dominating the descriptive patterns presented here, I over-sampled donation dates from earlier election cycles with replacement such that the re-weighted pool of all donation dates, which I use to construct Figure A.1, puts equal weight on each election cycle from 1990 to 2016.

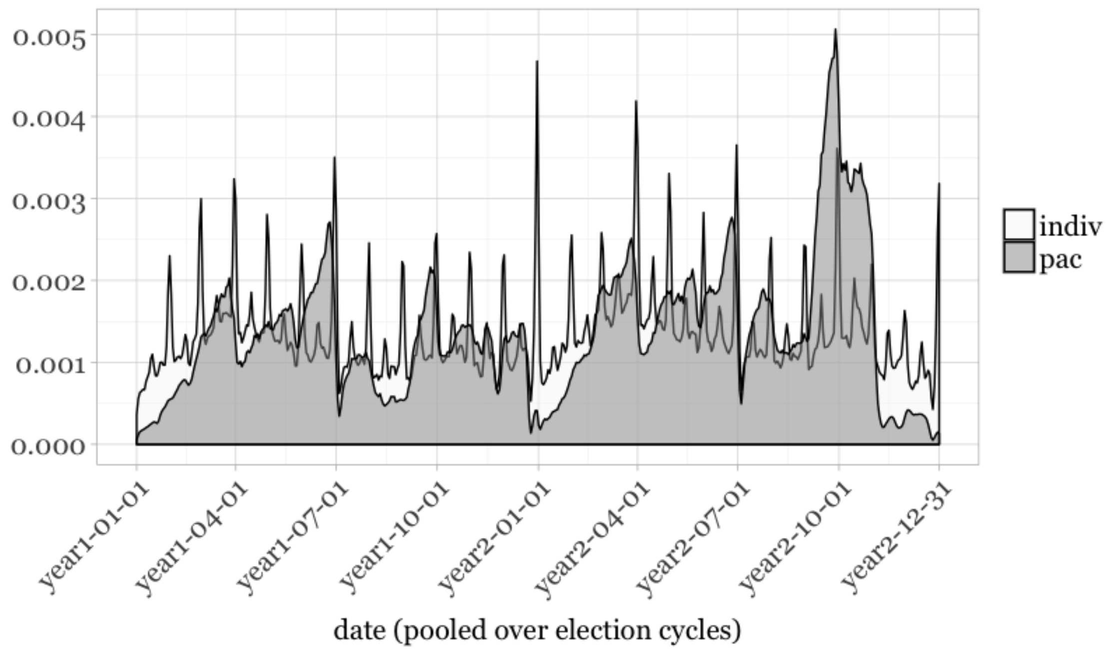

Figure A.1: Density Plots of Dates of Contributions, Pooling Election Cycles 1990-2016

A.2.4 PAC Disbursement vs. Receipts

The contribution patterns of most access-seeking PACs in my sample are largely consistent with PACs trying to contribute as much as they raise in each election cycle. As supporting evidence, I obtain total disbursements and receipts data from the FEC, and I calculate the disbursements-to-receipts ratio for each PAC in each cycle. Figure A.2 plots the weighted density of this ratio. I use the average amount of total receipts per PAC as weight, since all else constant PACs that tend to raise little have a much wider variance in their disbursements-to-receipts ratios. I also Winsorize at the 2% and 98% levels to get rid of extreme values (possibly caused by data reporting errors). Overall, the density plot shown in Figure A.2 centers around 1 (with 0.986 being the median). And while there is some dispersion in both directions—i.e., PACs sometimes under- or over-spent relative to how much they raised in the same election cycle—the interquartile range of $[0.876, 1.04]$ is still tight around the benchmark value of 1.

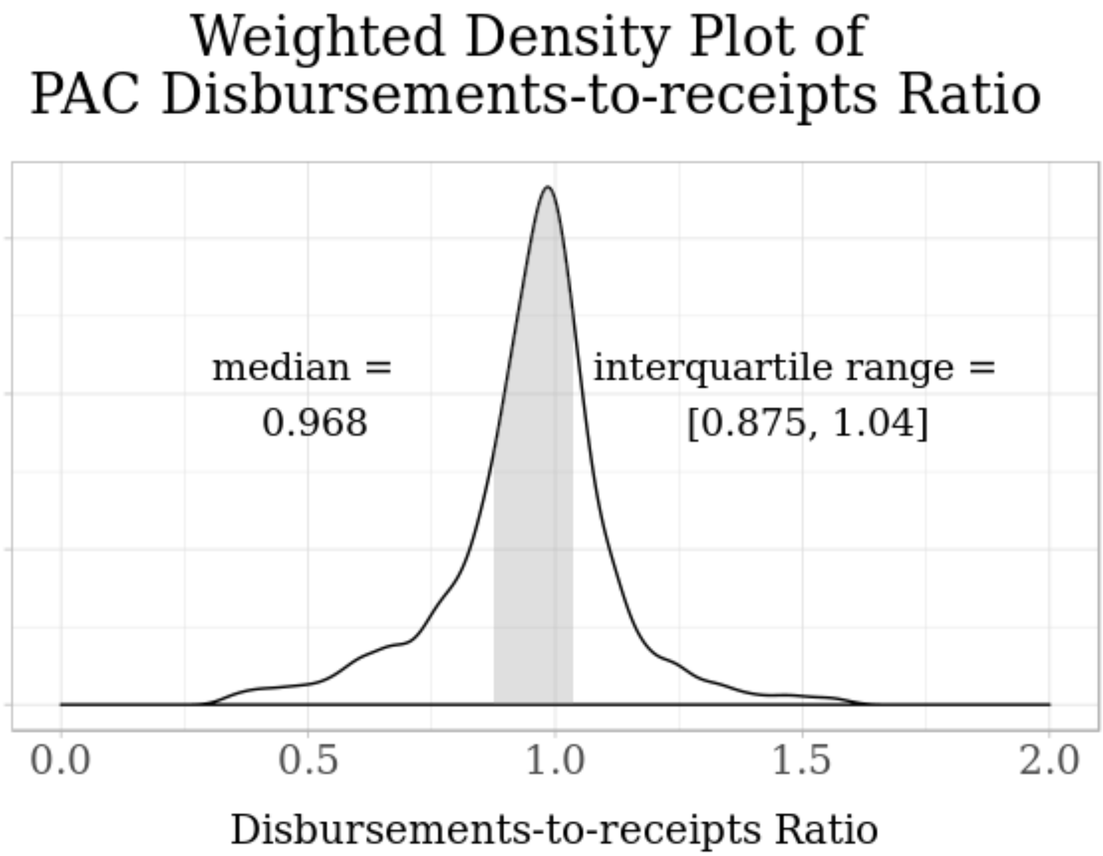

Figure A.2: Weighted Density Plot of PAC Disbursements-to-receipts Ratio

A.2.5 Modified Granger causality test

Here I verify that the difference-in-differences analysis presented in my paper is consistent with the parallel-trends assumption. To this end, I conduct a modified Granger causality test (Angrist and Pischke 2008, p. 237) of the following form:

$$\begin{aligned} Give_{ijt} = {} & \alpha_{ij} + \tau_{jt} + \beta(Indiv\_R_i \times PAC\_R_{jt}) \\ & + \sum_{m=1}^{2} \gamma_m (Indiv\_R_i \times PAC\_R_{j,t-m}) \\ & + \sum_{q=1}^{3} \lambda_q (Indiv\_R_i \times PAC\_R_{j,t+q}) + \epsilon_{ijt} \end{aligned} \tag{A.5}$$

Compared to specification 1 in the paper, here I additionally control for interaction terms between $Indiv\_R_i$ and either lagged (up to 2 cycles) or lead (up to 3 cycles) values of PAC_R_{jt}. The parallel-trends assumption is violated if any of the interaction terms with lead values of PAC_R_{jt} is statistically significant, which would suggest that future changes in the allocation of PAC contributions across parties could predict current donor behavior. In other words, I expect the lead coefficients λ_q for $q \in \{1, 2, 3\}$ to be zero. In contrast, any effect on the outcome variable from interaction terms with lagged values of PAC_R_{jt} is not a source of concern and simply reflects persistence in treatment effect.

Table A.3 reports the result of this modified Granger causality test, where the two models correspond to those in Table 1 of the paper. Controlling for $Indiv\_R_i$ interacted with lagged and lead values of PAC_R_{jt} does not appear to change the estimates for the treatment effects (i.e., the coefficient on $PAC\_R_{jt} \times Indiv\_R_i$) by much. Importantly, none of the lead terms are statistically significantly different from zero, so the parallel-trends assumption does not appear to be violated. The p-values for the joint null hypothesis of all lead coefficients being zero are 0.139 for column 1, and 0.218 for column 2, so I do not reject the null hypothesis in either case. At the same time, in both models, donors appear to be more willing to donate if their PAC gave a greater share of contributions to co-partisan recipients two cycles ago. This suggests some persistence in the treatment effect, and is consistent with the entry and exit phenomena discussed in the paper.

A.2.6 Analysis using lagged treatments

Here I examine how donor behavior responds to lagged PAC contribution patterns. In other words, instead of Specification 1 in the paper, I estimate

$$Give_{ijt} = \alpha_{ij} + \tau_{jt} + \beta(Indiv\_R_i \times PAC\_R_{j,t-1}) + \epsilon_{ijt} \tag{A.6}$$

where the independent variable $PAC\_R_{j,t-1}$ denotes the share of PAC j's contributions to

Table A.3: Modified Granger Causality Test for $Give_{ijt}$

	(1)	(2)
lagged 2 cycles	0.0324***	0.0313***
	(0.00941)	(0.00904)
lagged 1 cycle	0.00927	0.00794
	(0.00705)	(0.00713)
$PAC\_R_{jt} \times Indiv\_R_i$	0.0770***	0.0735***
	(0.00881)	(0.00913)
lead 1 cycle	0.00835	0.00951
	(0.0103)	(0.0106)
lead 2 cycles	-0.00832	-0.00776
	(0.0101)	(0.0106)
lead 3 cycles	0.0160	0.0108
	(0.00935)	(0.00943)
Donor-PAC fixed effect	Y	Y
PAC-cycle fixed effect	Y	Y
N	1485294	1969456
Sample	pure partisans	all partisans

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Republicans in the last cycle, $t-1$, rather than the current cycle, t.

In Table A.4, I present estimated treatment effects on rates of itemized giving using the partisan split of PAC contributions from the last cycle. In other words, columns 1-2 in Table A.4 are identical to those in Table 1 in the paper, except the former controls for lagged PAC contribution patterns. As Table A.4 shows, donors give itemized donations at lower rates when PAC contributions from the last cycle favored out-partisans; both estimates are positive and statistically significant. Nonetheless, the point estimates are smaller than those obtained by regressing on PAC contribution patterns in the same cycle.

Table A.4: Lagged Differences-in-differences Estimates for $Give_{ijt}$

	(1)	(2)
$PAC\_R_{j,t-1} \times Indiv\_R_i$	0.0598^{***}	0.0578^{***}
	(0.00747)	(0.00704)
Donor-PAC fixed effect	Y	Y
PAC-cycle fixed effect	Y	Y
N	2392183	3177625
Sample	pure partisans	all partisans

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2.7 Analysis using instrument for PAC contribution patterns

In this section, I present regression estimates from Specification 1 in the paper using an instrument for PAC contributions. One possible concern with Specification 1 is the possibility that PACs might adjust their contribution strategies in response to differential trends in donor behavior by partisanship. A natural starting point for isolating exogenous variations in how PACs allocate contributions across parties is to examine changes in partisan seat shares in Congress, since access-seeking PACs give disproportionately to incumbents (Fouirnaies and Hall 2014). The drawback, though, is that this shock is identical to all PACs. To allow it to affect the contribution strategy of each access-seeking PAC differently, I focus on the fact that access-seeking PACs prioritize contributing to incumbents in districts of geographic importance (Sabato 1985; Biersack, Herrnson, and Wilcox 1994). This means access-seeking PACs will react more strongly to a given change in Congressional seat shares insofar as the marginal electoral turnovers happen in those districts.

I construct this PAC-specific instrument for PAC_R_{jt} using the following two steps. First, to infer districts of geographic importance, I calculate the average amount of contributions that PAC j gives to the incumbent in congressional district d across all cycles *except cycle t*:

$$\overline{PAC\_Contrib}_{jd,-t} = \sum_{\tau \neq t} PAC\_Contrib_{jd\tau} / (T_j - 1) \tag{A.7}$$

Here, $PAC\_Contrib_{jd\tau}$ denotes the dollar amount of contributions that PAC j gave to the incumbent in congressional district d in cycle τ. T_j is the total number of cycles for which PAC j was active. Insofar as $\overline{PAC\_Contrib}_{jd,-t}$ is high for district d relative to other districts, it reveals that access to the incumbent in district d has consistently been PAC j's top priority. Consequently, any partisan swing in district d should disproportionately impact PAC_R_{jt}.

Next, I compute the predicted value of PAC_R_{jt} as

$$\widehat{PAC\_R}_{jt} = \frac{\sum_d \overline{PAC\_Contrib}_{jd,-t} \times I(R_{dt} = 1)}{\sum_d \overline{PAC\_Contrib}_{jd,-t} \times I(R_{dt} = 1 \vee D_{dt} = 1)} \tag{A.8}$$

where $I(R_{dt} = 1)$ is an indicator for the incumbent in district d in cycle t being a Republican, and $I(D_{dt} = 1)$ is an indicator for said incumbent being a Democrat. In other words, an electoral turnover in district d is weighted by d's relative importance to the PAC. I compute these predicted values of PAC_R_{jt} for all PACs that are at or above the 5^{th} percentile in terms of average total amounts of contributions made per cycle. Focusing on these PACs substantially enhances the relevance of the instrument.

For the predicted share in Equation A.8 to be a valid instrument, it must affect donor behavior only through variations in the actual share of PAC contributions to Republican recipients for each PAC and cycle. In particular, after controlling for donor-PAC fixed effect α_{ij} and PAC-cycle fixed effect τ_{jt}, the predicted share of PAC contributions to Republican recipients in Equation A.8 needs to be independent from unobserved differential trends in donor behavior.

There are good reasons *a priori* to believe that this exclusion restriction would hold. The predicted share of PAC contributions to Republicans depends on long-run averages of PAC contributions to incumbents in different districts, $\overline{PAC\_Contrib}_{jd,-t}$, which are unaffected by temporary shocks to donor behavior. Importantly, the contributions that PAC j made to the incumbent in district d in cycle t were left out of $\overline{PAC\_Contrib}_{jd,-t}$ by construction. Additionally, PAC contributions alone almost never tip any congressional races (Stratmann 2005), so the identity of the incumbent party in each district is likely exogenous to PAC giving.

Table A.5 reports estimates obtained with the proposed instrument, where columns 1 and 2 are analogous to those in Table 1 of the paper. Both coefficients are positive and statistically significant.

Table A.5: Differences-in-differences Estimates for $Give_{ijt}$ with Instrument

	(1)	(2)
$PAC\_R_{jt} \times Indiv\_R_i$	0.156^{***}	0.159^{***}
	(0.0186)	(0.0183)
Donor-PAC fixed effect	Y	Y
PAC-cycle fixed effect	Y	Y
N	2626453	3489750
Sample	pure partisans	all partisans
Instrument for PAC_R_{jt}	Y	Y
F-statistic for first stage	361	387

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6 reports results from a modified Granger causality test for the IV regressions. Here, in addition to the independent variable of interest, $PAC\_R_{jt} \times Indiv\_R_i$, I control for its lead (up to 3 cycles after) as well as lagged (up to 2 cycles prior) values. None of the lead terms are statistically significant in column 1 (estimated using only pure partisan donors), but in column 2 (estimated using all partisan donors) "lead 2 cycles" is statistically significant at the 5% level. When I perform an F-test on all lead coefficients in each column of Table A.6, the p-values for these F-tests are 0.000124 and 0.000156, respectively. In other words, I reject the null hypothesis that all lead coefficients in each column are jointly indistinguishable from zero.

Table A.6: Modified Granger Causality Test for $Give_{ijt}$, with Instrument

	(1)	(2)
lagged 2 cycles	0.0967**	0.0850**
	(0.0313)	(0.0307)
lagged 1 cycle	-0.00360	-0.00315
	(0.0250)	(0.0227)
$PAC\_R_{jt} \times Indiv\_R_i$	0.179***	0.175***
	(0.0274)	(0.0245)
lead 1 cycle	0.0252	0.0230
	(0.0359)	(0.0352)
lead 2 cycles	0.0845	0.0890*
	(0.0433)	(0.0414)
lead 3 cycles	0.0462	0.0312
	(0.0381)	(0.0386)
Donor-PAC fixed effect	Y	Y
PAC-cycle fixed effect	Y	Y
N	1478239	1959695
Sample	pure partisans	all partisans
Instrument for PAC_R_{jt}	Y	Y

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Despite what appears to be differential pre-trends in the IV estimates, it is worth noting that the actual treatment effects are at least twice the size of even the largest coefficients on the lead terms. This is suggestive evidence that not all of the IV results can be explained away by differential pre-trends. Moreover, the fact that the parallel-trends assumption is strongly robust in the non-IV results, together with the fact that my original survey of corporate PAC donors replicates the non-IV findings in an experimental setting, upholds a causal relationship between the share of PAC contributions to one party and the likelihood of giving by donors who support this party. This core result, validated in both observational and experimental settings, is unaffected by the sensitivity of the IV estimates to the modified Granger causality test.

Nonetheless, the contrast between Table A.6 and A.3 suggests that while actual changes in how PACs allocate contributions across parties do not suffer from unobserved differential trends in donor behavior, the predicted changes in PAC contribution patterns using my instrument do. I provide a possible explanation for this next.

A.2.8 Analysis of unobserved confounding trends in IV estimates

As discussed in the last section, I reject the null hypothesis of parallel trends only when I instrument for PAC contributions. One likely explanation is that partisan swings in districts of geographic importance may be capturing unobserved changes in the partisan composition of a given PAC's donor pool (e.g. its set of employees). In this section, I analyze why my instrument could be correlated with this unobserved source of differential trends, and I show that the regression estimates reported in the paper (i.e., without instrument) are robust to controlling for a proxy for these trends.

A.2.8.1 What could be the source of unobserved confounding trends?

Recall that my outcome variable is $Give_{ijt}$, i.e., an indicator for whether donor i gave any itemized donations to PAC j during cycle t. With perfect measurement, $Give_{ijt}$ should be coded as NA for any cycle during which donor i was not affiliated with the parent organization of PAC j (and hence ineligible to give to PAC j). However, because employment turnover is not observed in campaign finance data, when $Give_{ijt} = 0$ in my data it could mean that either donor i chose not to give, or donor i had not joined or already left the parent organization of PAC j in cycle t. Since these two scenarios are indistinguishable to the researcher, at least some of the variations in $Give_{ijt}$ could be due to personnel turnovers.

Put formally, the true data-generating process may resemble the following:

$$
\begin{aligned}
Give_{ijt} = \alpha_{ij} + \tau_{jt} + \beta(Indiv\_R_i \times PAC\_R_{jt}) \\
+ \gamma Org\_Member_{ijt} + \epsilon_{ijt}
\end{aligned}
\tag{A.9}
$$

where Org_Member_{ijt} is an indicator for whether donor i was affiliated with the parent organization of (i.e., hence eligible to give to) PAC j in cycle t, and $\gamma \geq 0$. Rearranging the above leads to

$$
\begin{aligned}
\mathbb{E}[\Delta_p \Delta_t Give_{ijt}] = \beta(PAC\_R_{jt} - PAC\_R_{j,t-1}) \\
+ \gamma \mathbb{E}[\Delta_p \Delta_t Org\_Member_{ijt}]
\end{aligned}
\tag{A.10}
$$

where, on the left-hand side,

$$
\begin{aligned}
\mathbb{E}[\Delta_p \Delta_t Give_{ijt}] = \mathbb{E}[Give_{ijt} - Give_{ij,t-1} \mid Indiv\_R_i = 1/2] \\
- \mathbb{E}[Give_{ijt} - Give_{ij,t-1} \mid Indiv\_R_i = -1/2]
\end{aligned}
$$

denotes the difference in changes in rates of itemized giving between Republican and Democratic donors in PAC j across cycles $t-1$ and t. Likewise, on the right-hand side of Equation A.10,

$$\mathbb{E}[\Delta_p \Delta_t Org\_Member_{ijt}] = \mathbb{E}[Org\_Member_{ijt} - Org\_Member_{ij,t-1} \mid Indiv\_R_i = 0.5] \\ - \mathbb{E}[Org\_Member_{ijt} - Org\_Member_{ij,t-1} \mid Indiv\_R_i = -0.5]$$

denotes the difference in net entry rates of Republican versus Democratic organizational members for PAC j across cycles $t-1$ and t. In words, observed differences in rates of itemized giving between Democratic and Republican donors of a PAC could be due to not only changes in how the PAC allocates contributions across parties (with true effect size of β), but also changes in the partisan composition of the pool of eligible donors (with true effect size of γ). If trends in PAC contribution patterns and changes in the partisan composition of the donor pool are uncorrelated, measurement error in $Give_{ijt}$ should not bias the estimated $\hat{\beta}$ in either direction. If, however, the two trends are positively correlated, $\hat{\beta}$ would be biased upward.

The fact that I cannot reject the null hypothesis of all lead coefficients being zero in Table A.3 suggests that actual PAC contribution patterns do not appear to be correlated with unobserved changes in the partisan composition of the donor pool. Indeed, each could change for idiosyncratic reasons. On one hand, changes in the partisan composition of the donor pool could simply result from personnel turnovers in an organization that altered the demographic makeups of its employees. At the same time, PACs may shift how much proportionally they give to Republican versus Democratic legislators depending on the specific types of access they are seeking (e.g., lobbying pivotal legislators to kill an unfavorable bill; lobbying allies to push new policy agenda forward) in any particular cycle.

The instrument for PAC contributions, in contrast, might be correlated by construction with such an unobserved confounding trend. Recall that in the last section, I instrumented how PACs allocate contributions across parties with changes in Republican seat shares in Congress *weighted by geographic importance of each district to a PAC*. In other words, this instrument creates a disproportionate change in predicted PAC contribution patterns if, say, the congressional district that contains an organization's headquarters experienced a partisan turnover. The structure of this instrument may have created two problems.

First, partisan turnovers in congressional districts are often driven by long-run trends in vote shares in favor of the new party, which in turn may have been driven by migration in and out of districts that changed the partisan composition of local residents, including PAC donors who reside in these districts. For example, Figure A.3 shows trends in vote shares in congressional districts that had one partisan turnover during my panel, which account for 46.8% of all districts that had any partisan swings.[2] If these long-run trends in vote shares were primarily caused by migration, it is plausible that PACs whose parent organizations operate in these districts might also experience personnel movements that changed the partisan composition of eligible donors.

[2]I thank Andy Hall and Jim Snyder for sharing congressional election data.

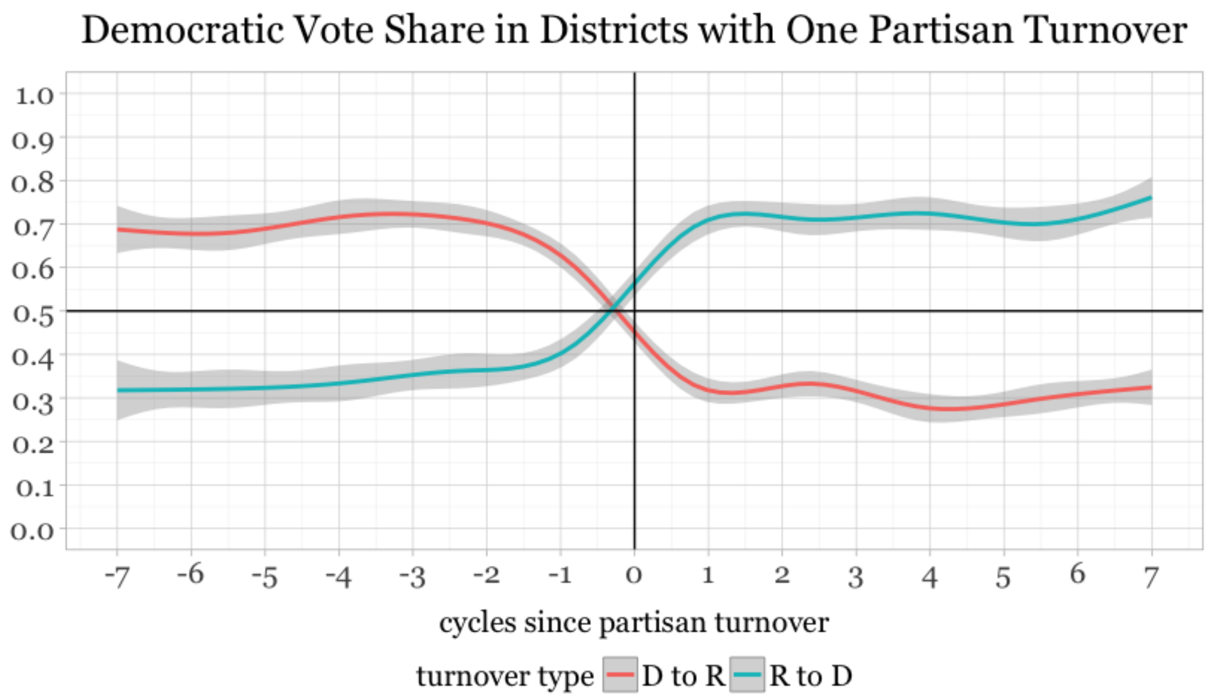

Figure A.3: Partisan Turnovers and Long-run Trends in Vote Shares

Second, the correlation between my instrument for PAC contribution patterns and unobserved changes in the partisan composition of the donor pool might be further compounded by how I weight district (refer back to equations A.7 and A.8 for detail). Recall that partisan turnover in any given congressional district receives more weight the more geographically important it is to a PAC, where I infer geographic importance based on how much contribution a PAC gives to the incumbent in this district in a typical election cycle. It is likely that many eligible PAC donors reside in districts that receive the most weight in the construction of my instrument.

A.2.8.2 Non-IV estimates are highly robust to unobserved confounding trends

To provide further supporting evidence for the parallel-trends assumption for Specification 1 (non-IV regression) in the paper, here I show that these non-IV estimates are unaffected by controlling for a proxy of unobserved changes in the partisan composition of PACs' donor pools.

As discussed earlier, trends in vote shares in districts that are geographically important to a PAC, which I do observe, should be correlated with trends in the partisan composition of eligible donors in the PAC, which I do not observe. So I could approximate unobserved changes in the partisan composition of PAC donors with

$$Weighted\_VoteShare\_R_{jt} = \frac{\sum_d [\overline{PAC\_Contrib}_{jd,-t} \times VoteShare\_R_{dt}]}{\sum_d \overline{PAC\_Contrib}_{jd,-t}}$$

where $Weighted\_VoteShare\_R_{jt}$ denotes the weighted average Republican general-election vote share in congressional races across districts in cycle t, with the weight being how much contributions PAC j gives on average to the incumbent in a given district across all cycles except the current one (i.e., the identical weight used in the construction of my instrument). Therefore, a robustness check for Specification 1 in the paper is to estimate the following

$$\begin{aligned} Give_{ijt} = \alpha_{ij} + \tau_{jt} + \tilde{\beta}(Indiv\_R_i \times PAC\_R_{jt}) \\ + \gamma(Indiv\_R_i \times Weighted\_VoteShare\_R_{jt}) + \epsilon_{ijt} \end{aligned} \tag{A.11}$$

Here I expect $\gamma \geq 0$. This says that if vote shares in districts of geographic importance to PAC j are trending Republican, such trends could increase the likelihood of donor i entering the parent organization of PAC j, and thereby giving itemized donations to PAC j, if donor i is Republican-leaning. In particular, this effect is independent of how PAC j allocates contributions across parties. In other words, if $\tilde{\beta}$ in the above specification above is close to β from Specification 1 in the paper, it would suggest that the non-IV estimated treatment effects are not biased by differential trends due to unobserved changes in the partisan composition of PACs' donor pools.

Table A.7 compares estimates obtained from Specification 1 in the paper with estimated obtained from Specification A.11 above. Columns 1 and 3 of Table A.7 are simply columns 1 and 2 of Table 1 in the paper. In particular, column 1 uses the sub-sample of pure partisan PAC donors whereas column 3 uses all partisan PAC donors. In comparison, columns 2 and 4 of Table A.7 show estimates obtained from Specification A.11, using pure partisans only versus all partisans.[3] Regardless of sample restriction, the estimated treatment effects on $PAC\_R_{jt} \times Indiv\_R_i$ are almost identical (and only slightly smaller) to those reported in the paper when I control for weighted vote share interacted with each donor's partisanship. Insofar as weighted vote share proxies for unobserved personnel movements that changed the partisan composition of a PAC's donor pool, Table A.7 suggests that such trends are uncorrelated with the treatment of interest, i.e., changes in how PACs allocate contributions across parties.

Table A.7: Differences-in-differences Estimates for $Give_{ijt}$

	(1)	(2)	(3)	(4)
$PAC\_R_{jt} \times Indiv\_R_i$	0.0853***	0.0852***	0.0846***	0.0841***
	(0.00813)	(0.00868)	(0.00822)	(0.00878)
$Weighted\_VoteShare\_R_{jt} \times Indiv\_R_i$		0.0196		0.0347
		(0.0265)		(0.0256)
Donor-PAC fixed effect	Y	Y	Y	Y
PAC-cycle fixed effect	Y	Y	Y	Y
N	2647085	2627323	3516248	3490623
Sample	pure partisans	pure partisans	all partisans	all partisans
Instrument for PAC_R_{jt}	N	N	N	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2.9 Permutation tests

Here I perform permutation tests to assess the sampling variability of the estimated treatment effect on rates of itemized giving under the null of no effects. To do so, I run 100 regressions identical to Specification 1 in the paper, except in each run every donor's par-

[3] Differences in the sample size of columns 1 vs. 2 and columns 3 vs. 4 are due to missing observations in $Weighted\_VoteShare\_R_{jt}$.

tisanship, $Indiv\_R_i$, is independently reversed with probability 0.5. Recall that $Indiv\_R_i$ ranges from $-1/2$ to $1/2$ (in 77% of all cases it equals one of these extreme values), with higher values indicating greater leaning towards the Republican (rather than Democratic) party. So in each of the 100 runs, every donor's $Indiv\_R_i$ will be replaced with $-Indiv\_R_i$ with probability 0.5.

Figures A.4 and A.5 show how the actual estimated treatment effects compare to the null distributions generated under 100 permutations. They correspond to models 1-2 in Table 1 in the paper, respectively. Two important features stand out in both plots. First, the null distribution is always centered around 0, as one would expect. Second, the actual estimated treatment effects are always around 10 times in magnitude than even the most extreme values generated by the null distributions. So in all cases the p-values are effectively zero. Importantly, given how the null distribution concentrates around 0, relative to the actual estimated treatment effects, qualitative conclusions from these permutation tests are highly unlikely to change even under a much greater number of permutations.

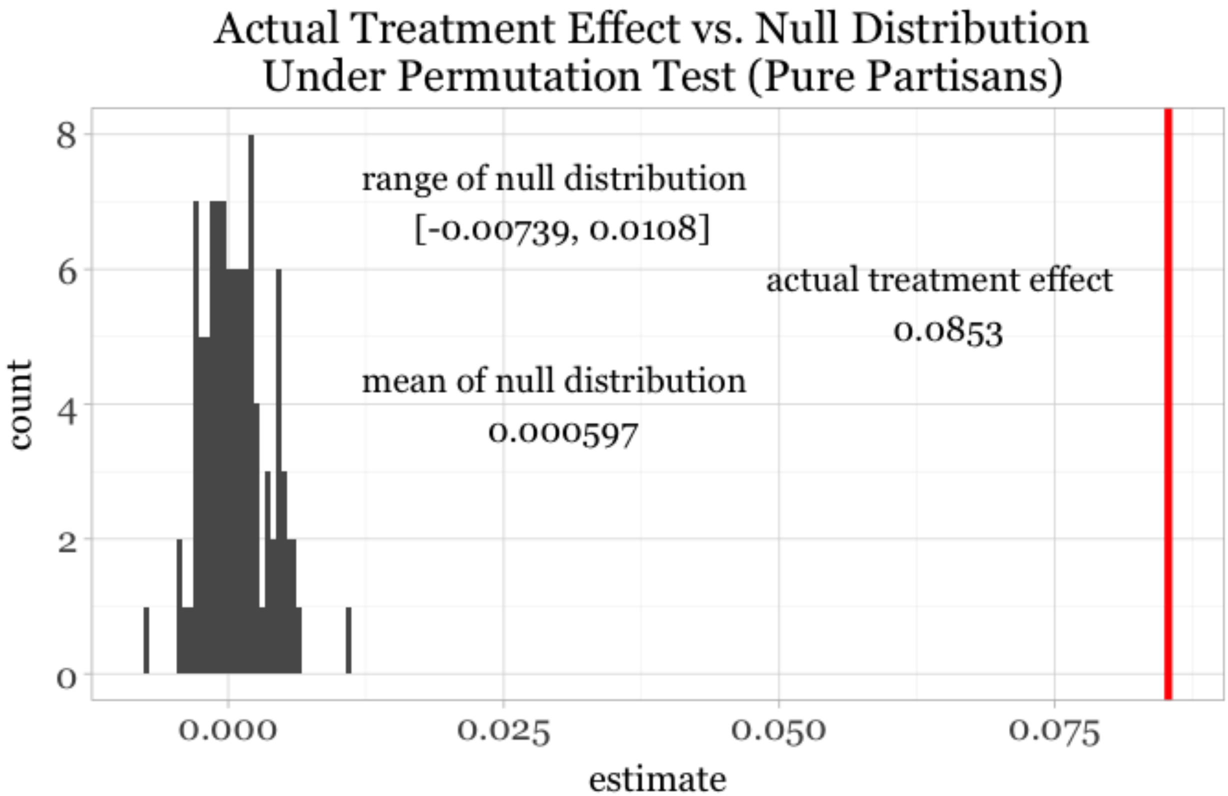

Figure A.4: Permutation Test (pure partisans only; no IV)

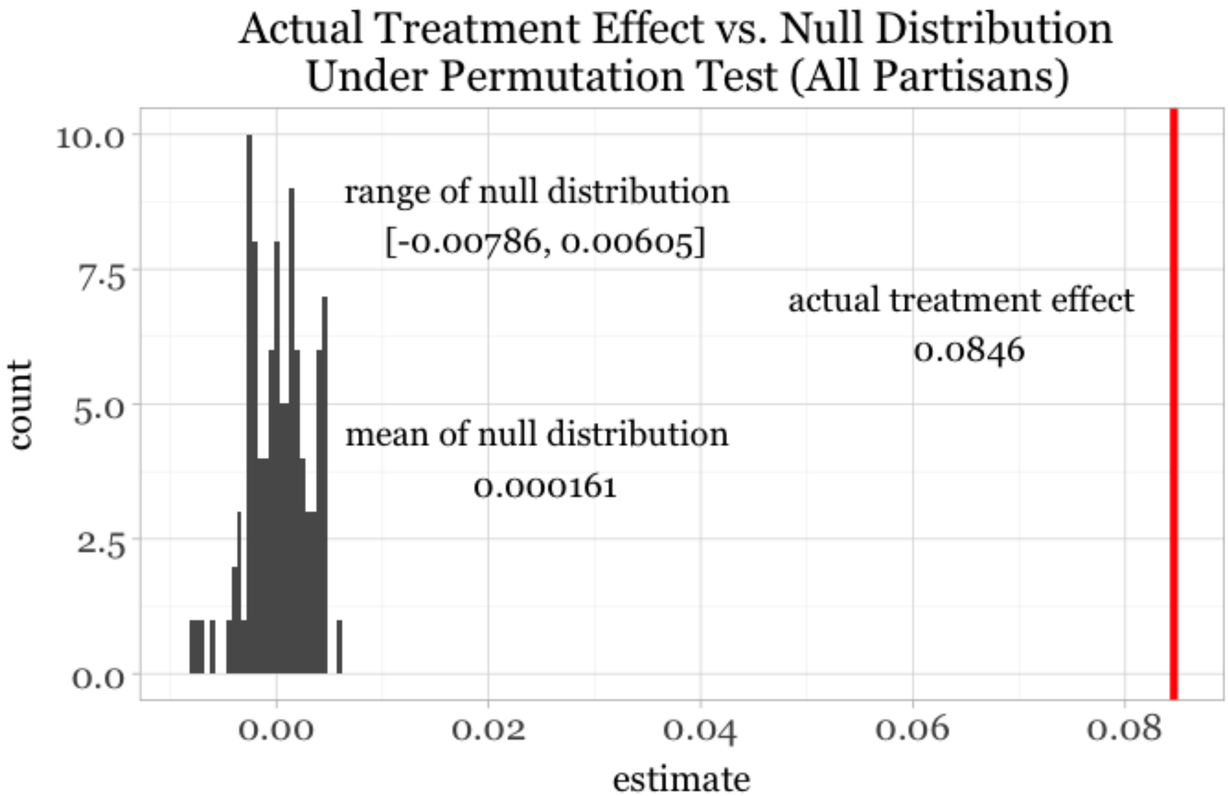

Figure A.5: Permutation Test (all partisans; no IV)

A.2.10 Analysis using only cross givers

Table 1 in the paper shows that donors are more likely to give itemized donations to their PACs when a greater share of PAC contributions goes to co-partisan recipients, consistent with my primary hypothesis. In particular, this result holds whether or not I restrict the sample to all donors with inferred partisan leanings (i.e., "all partisans") or just those who have only given to recipients of one of the major parties (i.e., "pure partisans"). Here, I provide further evidence that my primary hypothesis appears to hold even for the much smaller subset of PAC donors (approximately 23%) who have donated to recipients of both parties, who I hereafter refer to as the "cross givers".

Table A.8 reports the estimated treatment effect on rates of itemized giving using only cross givers. In other words, except for this sample restriction, Table A.8 is identical to Table 1 in the paper. The coefficient in Table A.8 is positive and statistically significant, suggesting that even within cross givers rates of itemized giving increase in the share of PAC contributions to said cross-givers' preferred parties (i.e., the party that receives the majority of a cross giver's donations over her history of giving). Nonetheless, the t-value here is only 3.59, which is noticeably smaller than the analogous t-value of 10.5 from Table 1 in the paper.

Table A.8: Differences-in-differences Estimates for $Give_{ijt}$

	(1)
$PAC\_R_{jt} \times Indiv\_R_i$	0.0710***
	(0.0198)
Donor-PAC fixed effect	Y
PAC-cycle fixed effect	Y
N	866116
Sample	cross-givers
Instrument for PAC_R_{jt}	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2.11 Analyasis on imputed dollar amounts of giving

As mentioned in footnote 12 of the paper, I cannot causally estimate how the share of PAC contributions to one's co-partisan recipients influences amounts of donations to one's PAC. This is because federal campaign finance law only requires PACs to itemize donations totaling $200 or more from a given donor in a given calendar year.[4] As a result, for a given PAC donor I can only observe the subset of donations she made that were itemized and, as I have shown in the paper, itemizated giving is itself influenced by how PACs allocate contributions across parties. Hence, observed amounts of donations to PACs suffer from a post-treatment censoring problem; for a detailed discussion, see pages 64-68

[4] For details, see https://www.fec.gov/help-candidates-and-committees/keeping-pac-records-nonconnected/recording-nonconnected-pac-receipts/; accessed Feb 21, 2018.

and 99-102 of Angrist and Pischke (2008). This means that simply replacing the outcome variable in Specification 1 in the paper with the observed amounts of itemized donations to PACs will not yield causal estimates of the treatment effect on amounts of giving.

While Tobit models can generally help to address the issue of censored data, they are not feasible in my application. There is no conditional estimator of treatment effect in fixed-effects Tobit models; see, for example, page 901 of Greene (2012). And with both donor-PAC and PAC-cycle fixed effects, it is impossible for me to estimate any unconditional fixed-effects Tobit models. In addition, while Honoré (1992) provides a semiparametric estimator for fixed-effects Tobit models through a Stata function *pantob*, *pantob* does not allow for more than one fixed effect. In spite of these limitations, I did try to use *pantob* to estimate a fixed-effect Tobit model of amounts of itemized donations with PAC-donor fixed effects, controlling directly for cycle dummies in lieu of PAC-specific cycle fixed effects, but the model would not converge.

The best remaining option is to run a series of difference-in-differences models, analogous to Specification 1 in the paper, using *imputed* amounts of donations. In particular, I estimate the following model

$$ImputedAmount_{ijt} = \alpha_{ij} + \tau_{jt} + \beta(Indiv\_R_i \times PAC\_R_{jt}) + \epsilon_{ijt} \tag{A.12}$$

where $ImputedAmount_{ijt}$ equals the observed dollar amount of donations that donor i made to PAC j in cycle t if i gave any itemized donations to j in that cycle. Otherwise, I assume donor i gave a specific amount of unitemized donation to PAC j in any cycle where no itemized donations from i to j were observed. I estimate Specification A.12 using four imputation schemes—\$0, \$50, \$100, and \$150—which collectively span a wide range of possible values of unitemized donations (including zeros).

These imputations all make the very strong assumption that anytime a donor did not make any itemized donations to a PAC that she was at some point affiliated with, she in fact gave whatever amount that my choice of imputation scheme assumes. This creates three potential sources of bias, mostly in the form of attenuation.

First, attenuation bias could arise from the fact that my imputation schemes preclude any and all (unobserved) variations in unitemized amounts of giving that could be affected by the treatment condition. In other words, my imputation schemes assume that once a donor decides to give an unitemized (potentially zero) donation, the amount of that unitemized donation is insensitive to how her PAC allocates contributions across parties.

Second, whenever a donor has made at least one itemized donation to an access-seeking PAC, I impute donations between said donor and PAC across cycles 1990 through 2016 whenever no itemized donations are observed. As a result, my imputation scheme further bias the estimates towards zero by the inclusion of donor-cycles during which donors were ineligible to give and hence were not "treated" by changes in the allocations of PAC contributions across parties.

Third, depending on what the typical amount of unitemized (including zero) donations is, which we never get to observe, my particular imputation scheme may bias the estimated effect in either direction depending on how the imputed amount departs from the true average. That being said, insofar as most cases of absence of itemized donations are cases where donors did not give anything to their affiliated PACs, most of my imputation schemes (i.e., those above $0) will likely attenuate the estimated treatment effects on amounts of donations.

Tables A.9 and A.10 report estimates obtained using Specification A.12. Tables A.9 uses only the sub-sample of pure partisan PAC donors, while Tables A.10 uses all partisan PAC donors. Both tables are organized such that columns 1-4 report the estimated treatment effect where amounts are imputed at $0, $50, $100, and $150, respectively.

Across all columns of both tables, the estimated treatment effects are positive and statistically significant. In particular, in each table, the point estimates as well as the standard errors do not change much with the particular imputation scheme used, suggesting that

Table A.9: Differences-in-differences Estimates for Imputed $Amount_{ijt}$

	(1)	(2)	(3)	(4)
Imputation level	$0	$50	$100	$150
$PAC\_R_{jt} \times Indiv\_R_i$	137.5***	133.2***	128.9***	124.7***
	(14.31)	(14.05)	(13.79)	(13.55)
Donor-PAC fixed effect	Y	Y	Y	Y
PAC-cycle fixed effect	Y	Y	Y	Y
N	2647085	2647085	2647085	2647085
Sample	pure partisans	pure partisans	pure partisans	pure partisans
Instrument for PAC_R_{jt}	N	N	N	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10: Differences-in-differences Estimates for Imputed $Amount_{ijt}$

	(1)	(2)	(3)	(4)
Imputation level	$0	$50	$100	$150
$PAC\_R_{jt} \times Indiv\_R_i$	158.7***	154.5***	150.3***	146.0***
	(17.62)	(17.35)	(17.08)	(16.83)
Donor-PAC fixed effect	Y	Y	Y	Y
PAC-cycle fixed effect	Y	Y	Y	Y
N	3516248	3516248	3516248	3516248
Sample	all partisans	all partisans	all partisans	all partisans
Instrument for PAC_R_{jt}	N	N	N	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

most of the identifying variation comes from the extensive margin.

I now compare estimates shown in Table A.9, which uses the sub-sample of pure partisan donors, to those obtained from the survey experiment. Recall that based on column 1 of Table 1 in the paper, a one-standard deviation decrease in the residualized share of PAC contributions to one party (19.7 percentage points) reduces the amounts given by PAC donors aligned with the opposite party by 15.6% on average. In comparison, based on Table A.9, identical change in treatment condition reduces the imputed amounts of donations by 6.01%, 4.88%, 4.06%, or 3.45%, depending on whether donations are imputed at $0, $50, $100, or $150. While these relative effect sizes appear smaller than that obtained from the survey experiment, because of the caveats of my imputation schemes discussed earlier, the true relative effect sizes are likely to be much larger and more in line with the survey experiment result.

A.2.12 Linear time trends in estimated treatment effects

Figure A.6 plots linear trends in the estimated $\hat{\beta}$ for Specification 1 in the paper. There is suggestive evidence that the estimated treatment effects on donors' rates of giving due to changing shares of PAC contributions to co-partisan politicians have nearly doubled between 1990 and 2016, although the estimated slopes are marginally significant with t-statistics of 1.89 (estimated using pure partisan donors only) or 1.95 (estimated using all partisan donors).

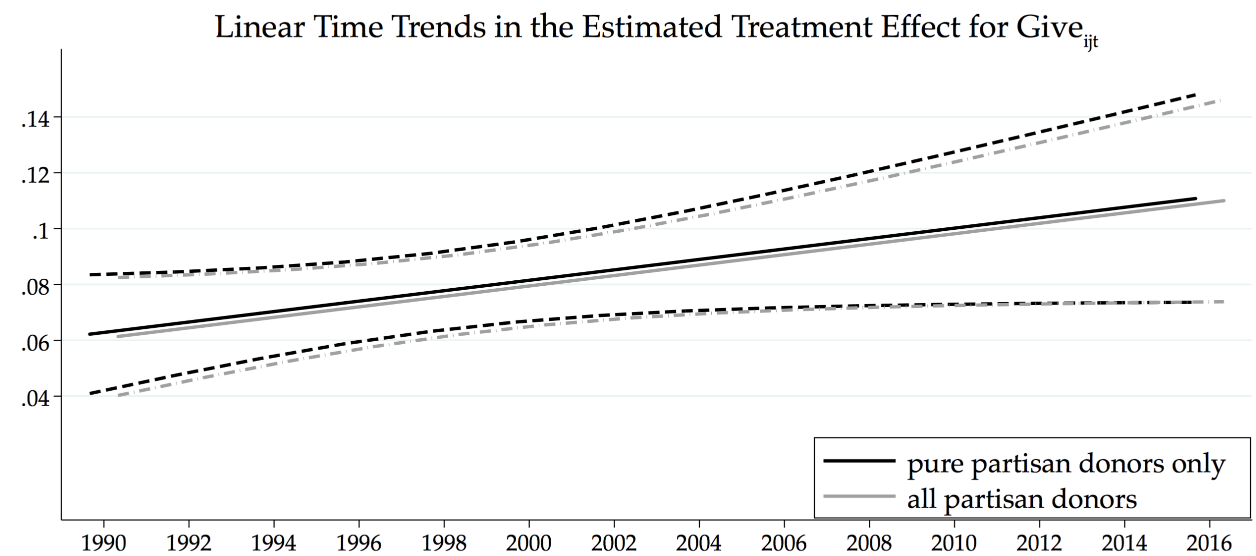

Figure A.6: Donations Increasingly Depend on Share of PAC Contributions to Co-partisans

One might wonder whether this linear time trend results mechanically from the fact that the itemization threshold—$200—is not adjusted for inflation, since my outcome variable $Give_{ijt}$ is an indicator of itemized giving. Insofar as donors tend to donate a fixed proportion of their income to their PACs, it is certainly plausible that rates of itemized giving have risen over time simply because $200 is a smaller share of individual or household income in 2016 than in 1990. However, inflation by itself should not affect linear trends in the estimated $\hat{\beta}$ as shown in Figure A.6, because the PAC-cycle fixed effect τ_{jt} in Specification 1 in the paper already absorbs any general trend in rates of itemized giving due to rising nominal income levels. Inflation may exaggerate the estimated linear trends shown in Figure A.6 only when (a) there are heterogeneous treatment effects across donors; and (b) the sub-set of donors for whom treatment effects are stronger were more likely to be on the margin of giving itemized (as opposed to unitemized) donations during earlier cycles of my panel.

A.2.13 Heterogeneity in estimated treatment effects on rates of itemized giving

A promising direction of future work is to examine the sources of heterogeneity in how donor behavior responds to changes in the share of PAC contributions to co-partisan (as opposed to out-partisan) politicians. Here I provide some suggestive analyses.

A.2.13.1 Partisan leaning

One may wonder whether PAC donations made by donors of different partisan leanings are more sensitive to changes in the share of PAC contributions to co-partisan recipients. To this end, I estimate

$$\begin{aligned} Give_{ijt} = \alpha_{ij} + \tau_{jt} &+ \beta_1 Indiv\_R_i \times PAC\_R_{jt} \\ &+ \beta_2 Indiv\_R_i \times PAC\_R_{jt} \times I(Indiv\_R_i > 0) \\ &+ \beta_3 PAC\_R_{jt} \times I(Indiv\_R_i > 0) + \xi_{ijt} \end{aligned} \tag{A.13}$$

where $I(Indiv\_R_i > 0)$ is an indicator of donor i being Republican-leaning (i.e., Republican politicians have received the majority of donor i's direct donations throughout her history of giving). Given the above specification, I detect a significant difference in estimated treatment effect by party if $\beta_2 \neq 0$. Note that I do not separately control for either $I(Indiv\_R_i > 0)$ or the interactions between $Indiv\_R_i$ and $I(Indiv\_R_i > 0)$ as both are absorbed by the fixed effect α_{ij}. Note also that I cannot estimate the above specification with the subset of pure partisan donors (i.e., when $Indiv\_R_i \in \{-1/2, 1/2\}$) because in that case $Indiv\_R_i$, which would take the value of either $-1/2$ or $1/2$, would be collinear with the indicator $I(Indiv\_R_i > 0)$.

Table A.11 presents results obtained from Specification A.13. Since the estimated β_2 is indistinguishable from zero, there appears to be no heterogeneity in the estimated treatment effect by partisan leaning.

Table A.11: Differences-in-differences Estimates for $Give_{ijt}$

	(1)
$PAC\_R_{jt} \times Indiv\_R_i$	0.0726***
	(0.0205)
$PAC\_R_{jt} \times Indiv\_R_i \times I(Indiv\_R_i > 0)$	0.0604
	(0.0357)
$PAC\_R_{jt} \times I(Indiv\_R_i > 0)$	-0.0182
	(0.0121)
Donor-PAC fixed effect	Y
PAC-cycle fixed effect	Y
N	3516248
Sample	all partisans
Instrument for PAC_R_{jt}	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2.13.2 Average imputed amount of donations to PAC

Another potential source of heterogeneity is the average imputed amount of donations given to one's PAC in an average election cycle. To test this, I estimate

$$
\begin{aligned}
Give_{ijt} = \alpha_{ij} + \tau_{jt} + \beta_1 Indiv\_R_i \times PAC\_R_{jt} \\
+ \beta_2 Indiv\_R_i \times PAC\_R_{jt} \times \log(Ave\_Imputed\_PAC\_Amount_{ij} + 1) \\
+ \beta_3 PAC\_R_{jt} \times \log(Ave\_Imputed\_PAC\_Amount_{ij} + 1) + \xi_{ijt}
\end{aligned} \tag{A.14}
$$

where $\log(Ave\_Imputed\_PAC\_Amount_{ij} + 1)$ is a log transformation of the average *imputed* amount of PAC donations that donor i gives to PAC j, where I impute missing observations (i.e., no itemized donations made) at \$0. Given the above specification, I detect a significant difference in estimated treatment effect by this average imputed amount of PAC donations if $\beta_2 \neq 0$.

Table A.12 presents results obtained from Specification A.14, where column 1 uses the sub-sample of pure partisan donors while column 2 uses all partisan donors. In both columns, estimated treatment effects are larger for donors with higher average imputed amount of PAC donations.

Table A.12: Differences-in-differences Estimates for $Give_{ijt}$

	(1)	(2)
$PAC\_R_{jt} \times Indiv\_R_i$	-0.115	-0.130
	(0.0808)	(0.0873)
$PAC\_R_{jt} \times Indiv\_R_i \times \log(Ave\_Imputed\_PAC\_Amount_{ij} + 1)$	0.0307*	0.0327*
	(0.0127)	(0.0136)
$PAC\_R_{jt} \times \log(Ave\_Imputed\_PAC\_Amount_{ij} + 1)$	0.0171	0.0168
	(0.00928)	(0.00922)
Donor-PAC fixed effect	Y	Y
PAC-cycle fixed effect	Y	Y
N	2647085	3516248
Sample	pure partisans	all partisans
Instrument for PAC_R_{jt}	N	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2.13.3 *Average imputed amount of direct donations*

A final source of heterogeneity I examine is the average *imputed* amount of direct donations (i.e., those made to candidates or party committees) in a given cycle:

$$\begin{aligned} Give_{ijt} = {} & \alpha_{ij} + \tau_{jt} + \beta_1 Indiv\_R_i \times PAC\_R_{jt} \\ & + \beta_2 Indiv\_R_i \times PAC\_R_{jt} \times \log(Ave\_Imputed\_Direct\_Amount_i + 1) \\ & + \beta_3 PAC\_R_{jt} \times \log(Ave\_Imputed\_Direct\_Amount_i + 1) + \xi_{ijt} \end{aligned} \tag{A.15}$$

where $\log(Ave\_Imputed\_Direct\_Amount_{ij} + 1)$ is a log transformation of the average *im-*

puted amount of direct donations that donor i gives, where I impute missing observations (i.e., no itemized donations made) at \$0. Given the above specification, I detect a significant difference in estimated treatment effect by this average imputed amount of direct donations if $\beta_2 \neq 0$.

Table A.13 presents results obtained from Specification A.15, where column 1 uses the sub-sample of pure partisan donors while column 2 uses all partisan donors. In each column the estimated β_2 is indistinguishable from zero, so there does not seem to be any heterogeneity by the average imputed amount of direct donations.

Table A.13: Differences-in-differences Estimates for $Give_{ijt}$

	(1)	(2)
$PAC\_R_{jt} \times Indiv\_R_i$	0.0778*	0.0832*
	(0.0356)	(0.0347)
$PAC\_R_{jt} \times Indiv\_R_i \times \log(Ave\_Imputed\_Direct\_Amount_{ij} + 1)$	0.00149	0.000234
	(0.00914)	(0.00834)
$PAC\_R_{jt} \times \log(Ave\_Imputed\_Direct\_Amount_{ij} + 1)$	0.00740	0.00620
	(0.00450)	(0.00409)
Donor-PAC fixed effect	Y	Y
PAC-cycle fixed effect	Y	Y
N	2647085	3516248
Sample	pure partisans	all partisans
Instrument for PAC_R_{jt}	N	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2.14 Patterns of PAC donors' direct donations to candidates and party committees

My paper shows that donors withhold donations to access-seeking PACs when PACs give to out-partisan politicians. One might wonder whether PAC contribution patterns also affect how these donors make direct donations to candidates and party committees. In this case, prior expectations are less clear.

On one hand, one might conjecture a particular type of substitution pattern: when a greater share of PAC contributions goes to politicians of one party, donors who prefer the opposite party might not only refrain from giving to the PAC, but also increase their direct donations to politicians of their preferred party. This could be the case if donors obtain more expressive value from their direct donations when PAC contributions conflict with their partisan preferences. Such donor behavior would also be consistent with the notion that donors intend to channel a fixed amount of donations to their preferred candidates or party via either direct donations or indirect donations to their PACs, thereby leading donors to substitute to giving more directly when PAC contributions increasingly end up in the campaign accounts of out-partisan politicians.

On the other hand, the opposite is also plausible: PAC donors might give more directly to politicians of their preferred party when PAC contributions to co-partisan politicians increase. One explanation is that donors are more likely to make direct donations to their preferred candidates or party (as opposed to expressing support via less costly means such as voting) when their employers mobilize them to do so in order to advance their companies' bottom lines (Hertel-Fernandez 2016). Indeed, Babenko, Fedaseyeu, and Zhang (2018) find that employees are more likely to donate to candidates supported by their CEOs than those that do not receive their CEOs' endorsements.

It is worth stressing that my method of inferring donor partisanship remains valid even if being endorsed by a PAC increases the chance of a candidate receiving donations from PAC donors. Recall that $Indiv\_R_i$ equals the net share of donor i's direct donations to Republican (as opposed to Democratic) candidates and party committees *throughout donor i's entire donation history*. If PAC donors indiscriminately donate to whoever their organizations endorse, they would very likely have given to recipients of both parties at least once given the nature of access seeking. However, 77% of all PAC donors in my sample have only ever given to recipients of one party, suggesting that the vast majority of PAC donors might be responsive to workplace pressure to make direct donations only when the recipients are co-partisan. In other words, for a given candidate to receive direct donations from a PAC donor, being a co-partisan is still a stronger pre-condition than receiving an endorsement from the donor's organization.

To understand which of these competing hypotheses holds in the data, I estimate a series of regressions of PAC donors' direct donation behavior on PAC contribution patterns. First, using the subset of pure partisan PAC donors, I estimate

$$Direct\_N\_Copartisan_{it} = \alpha_{ij} + \tau_{jt} + \eta Indiv\_R_i \times PAC\_R_{jt} + \zeta_{ijt} \tag{A.16}$$

where $Direct\_N\_Copartisan_{it}$ equals the number of co-partisan candidates or party committees that donor i donated to during election cycle t. Here, $\eta < 0$ would lend support to the first hypothesis that PAC donors substitute away from giving to PACs, instead donating directly to candidates and party committees, when PAC contributions increasingly go to out-partisan recipients. If instead $\eta > 0$, it would be consistent with my alternative hypothesis that PAC donors make more direct contributions to co-partisan candidates and party committees, likely because of mobilization by their parent organizations, when their PACs are donating more to co-partisan recipients.

Table A.14 presents the estimated $\hat{\eta}$ from Specification A.16, using the sub-sample of pure partisan donors. The result suggests that the number of co-partisan recipients that a donor gives to is increasing in the share of PAC contributions to co-partisan politicians. In particular, a standard-deviation increase in the residualized percentage of PAC contributions to co-partisan recipients (19.7 percentage points) increases the number of co-partisan candidates a pure partisan PAC donor donates to in the same cycle by 0.0219, roughly a 8.39% increase from the baseline of 0.262 co-partisan candidates/party committees given to per cycle. This is not consistent with the substitution hypothesis, but is consistent with the conjecture that PAC donors might be more likely to make direct contributions to *co-partisan* candidates and party committees when doing so is also in the strategic interest of their parent organizations.

I can conduct similar analysis for PAC donors who have given to both parties at least

Table A.14: Differences-in-differences Estimates for $Direct\_N\_Copartisan_{ijt}$

	(1)
$PAC\_R_{jt} \times Indiv\_R_i$	0.223^{***}
	(0.0181)
Donor-PAC fixed effect	Y
PAC-cycle fixed effect	Y
N	2647049
Sample	pure partisans
Instrument for PAC_R_{jt}	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

once (i.e., the "cross givers"). I again estimate Specification A.16 for the cross givers, where $Direct\_N\_Copartisan_{it}$ is re-defined to represent the number of candidates of donor i's *preferred* party.[5] Additionally, I examine the following

$$Direct\_N\_Outpartisan_{it} = \alpha_{ij} + \tau_{jt} + \rho Indiv\_R_i \times PAC\_R_{jt} + \zeta_{ijt} \tag{A.17}$$

where $Direct\_N\_Outpartisan_{it}$ equals the number of recipients from donor i's *less preferred* party that donor i donated to during election cycle t. If $\rho < 0$, it would suggest that cross givers donate to politicians of their less preferred party more when PAC contributions increasingly support the same party. This would lend further support to the hypothesis that PAC donors' direct giving is influenced partly by mobilization at the workplace.

Table A.15 presents the estimated $\hat{\eta}$ from Specification A.16 using the sub-sample of cross givers. The estimate obtained suggests that a standard-deviation increase in the residualized percentage of PAC contributions to co-partisan recipients (19.7 percentage points) increases the number of co-partisan candidates a cross giver donates to by 0.133 during the same cycle, roughly a 15% increase from the baseline of 0.882 co-partisan

[5]For example, if 80% of a donor's direct donations over her history of giving went to Democratic (as opposed to Republican) recipients, then I say that this donor appears to prefer the Democratic party.

candidates/party committees given to per cycle.

Table A.15: Differences-in-differences Estimates for $Direct\_N\_Copartisan_{ijt}$

	(1)
$PAC\_R_{jt} \times Indiv\_R_i$	1.348***
	(0.174)
Donor-PAC fixed effect	Y
PAC-cycle fixed effect	Y
N	866112
Sample	cross givers
Instrument for PAC_R_{jt}	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Additionally, Table A.16 presents the estimated $\hat{\rho}$ from Specification A.17. Here, a standard-deviation increase in the percentage of PAC contributions to co-partisan recipients (19.7 percentage points) reduces the number of out-partisan candidates a cross giver donates to by 0.035 in the same cycle, roughly a 14.9% decline from the baseline of 0.236 out-partisan candidates/party committees given to per cycle.

Table A.16: Differences-in-differences Estimates for $Direct\_N\_Outpartisan_{ijt}$

	(1)
$PAC\_R_{jt} \times Indiv\_R_i$	-0.356***
	(0.0311)
Donor-PAC fixed effect	Y
PAC-cycle fixed effect	Y
N	866112
Sample	cross givers
Instrument for PAC_R_{jt}	N

Standard errors are clustered at the PAC level and reported in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

At this point, one may wonder whether inferred partisanship for cross givers remains valid. Insofar as cross givers make direct donations to candidates and party committees endorsed by their PACs regardless of partisan labels, $Indiv\_R_i$ for cross givers (as opposed to pure partisans) could in theory be a poor proxy for their true partisan leanings.

For example, a cross giver who has given 80% of her direct donations to Republican recipients might simply have done so because over time her employer had, for access-seeking reasons, encouraged her to donate to more Republican politicians than Democratic ones. However, if it were truly the case that $Indiv\_R_i$ for cross givers simply reflects cumulative influence of workplace political mobilization, donations of cross givers *to their PACs* should be insensitive to how well the partisan allocation of PAC contributions aligns with these donors' "preferred" parties as measured by $Indiv\_R_i$. But this is false given evidence presented in Section A.2.10. Therefore, even for cross givers, their direct donation history as summarized in $Indiv\_R_i$ might still provide a noisy but informative signal of their true partisan leanings.

A.3 Study 2: Original Survey of Recent PAC Donors

A.3.1 Respondent characteristics

Each of the respondents from either the "Compustat sampling frame" or the "other sampling frame" has donated exclusively to one of the two major parties. There is no strong theoretical reason why I couldn't also sample PAC donors who have given to both parties. As Table 1 in the paper shows, the estimated treatment effects in my difference-in-differences analysis (i.e., how much willingness to donate corresponds to the share of co-partisan candidates that one's PAC contributes to) hardly vary by whether I use the sample of all partisan PAC donors or the sub-sample of pure partisan PAC donors. In other words, it appears that even for cross-party PAC donors, willingness to donate is higher when a greater fraction of PAC contributions goes to a donor's preferred party.

Nonetheless, I chose not to sample donors who have given across parties in the survey. This reflected a purely practical concern: I wanted to maximize the power of my survey given a limited budget for sampling. As discussed in Section A.2.10, difference-in-differences treatment effects estimated using only cross givers have noticeably lower t-statistics compared to those reported in Table 1 of the paper. This could be due to the fact that I empirically infer PAC donors' partisan leaning using the Republican share of their donations to candidates and party committees over time—these shares could be a much noisier proxy for partisan leanings of cross givers than for donors who have only ever given to politicians of one party (see Section A.2.14 for suggestive evidence). As a result, if I were to include donors who have given across parties in my survey sample, I would have needed a larger sample size to reach the same expected level of power, all else constant. Given my budget constraint, I decided to maximize power by sampling only donors who are pure partisans as revealed by their direct donation histories.

In the remainder of this section, I describe demographic and political characteristics of the respondents in comparison to that of the sampling frame.

A.3.1.1 Inferred partisanship

Within the Compustat sampling frame, respondents are more likely to be Democratic leaning as revealed by their direct donation histories, as shown in Figure A.7. By design, this sampling frame has equal numbers of Democratic- versus Republican-leaning donors. In comparison, 56.9% of the respondents from this sampling frame are Democratic donors.

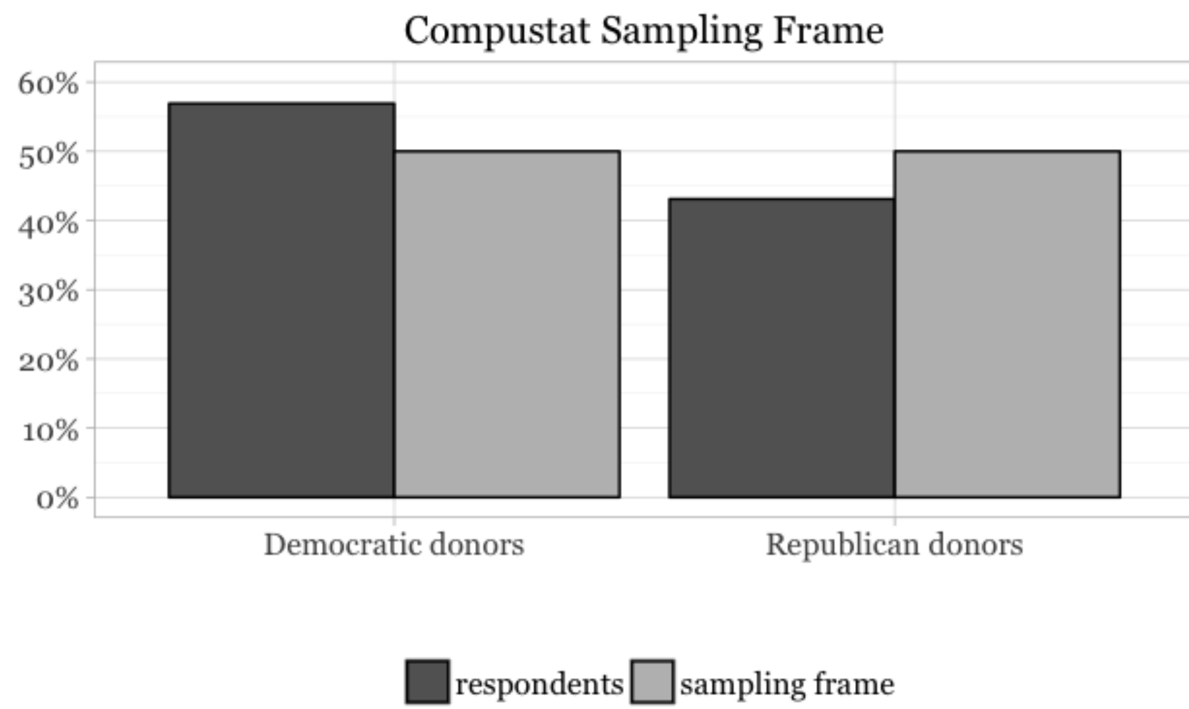

Figure A.7: Inferred Partisanship for the Compustat Sampling Frame

Similarly, the other sampling frame also has, by design, equal numbers of donors by inferred partisan leanings. But 72.5% of respondents from this sampling frame are Democratic donors, as shown in Figure A.8.

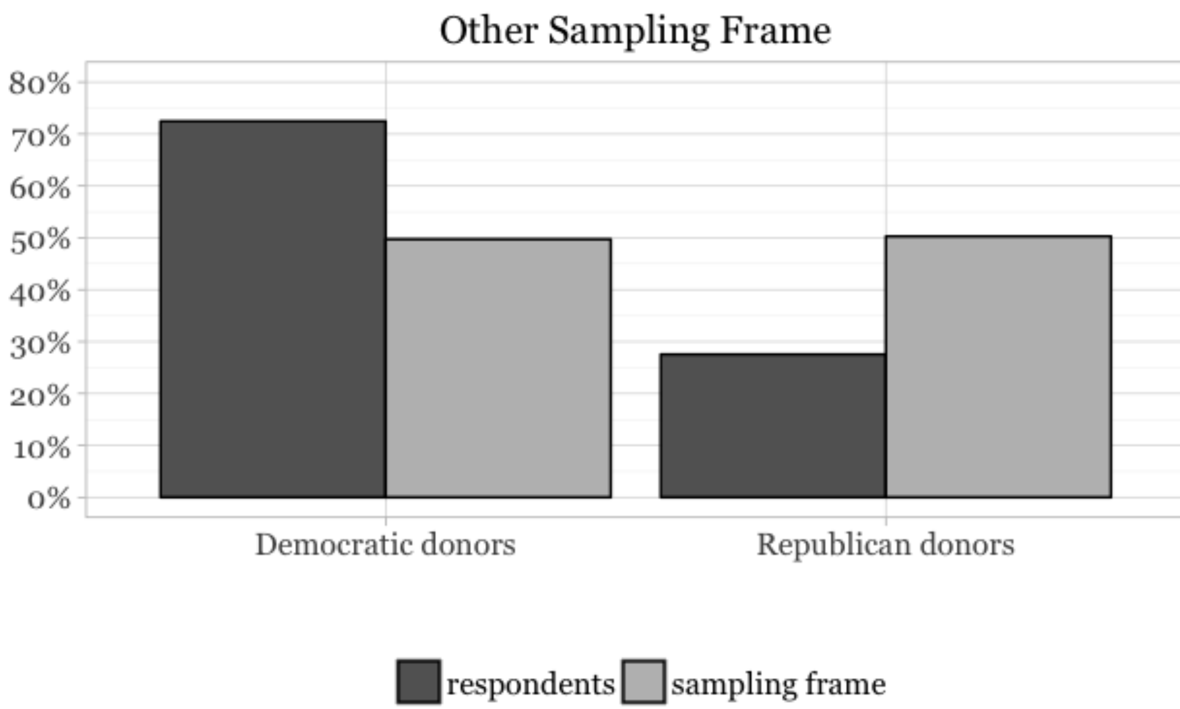

Figure A.8: Inferred Partisanship for the Other Sampling Frame

A.3.1.2 *Average sum of itemized donations by cycle*

For all individual donors I tried to sample, I compute the average sum of itemized donations (to all recipients) per cycle conditional on giving, using individual donation records since the 2008 election cycle provided in Bonica (2016b). Within each sampling frame, those who responded give smaller total amounts of itemized donations on average.

Figure A.9 shows the distribution of the logarithm of these average sums for respondents versus all those in the Compustat sampling frame. Indeed, the average sum of itemized donations in a cycle, conditional on donating, is $1,566 in this sampling frame. In comparison, among respondents in the same sampling frame, this average is $1,057.

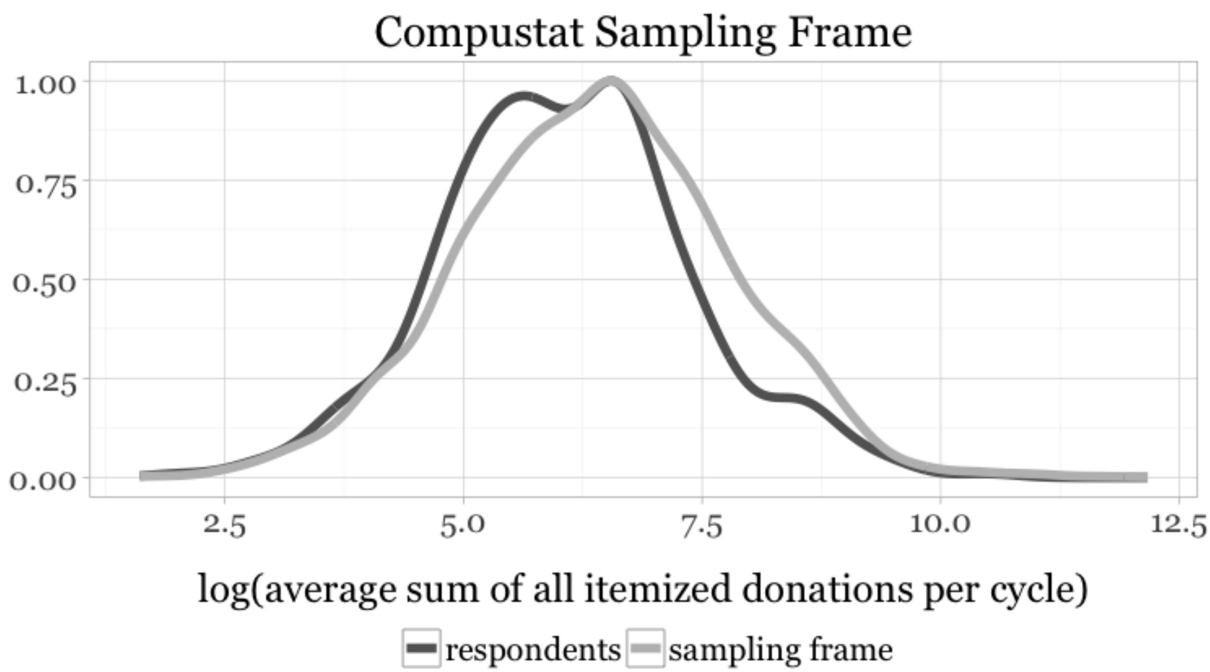

Figure A.9: Log Average Sum of Itemized Donations Per Cycle for the Compustat Sampling Frame

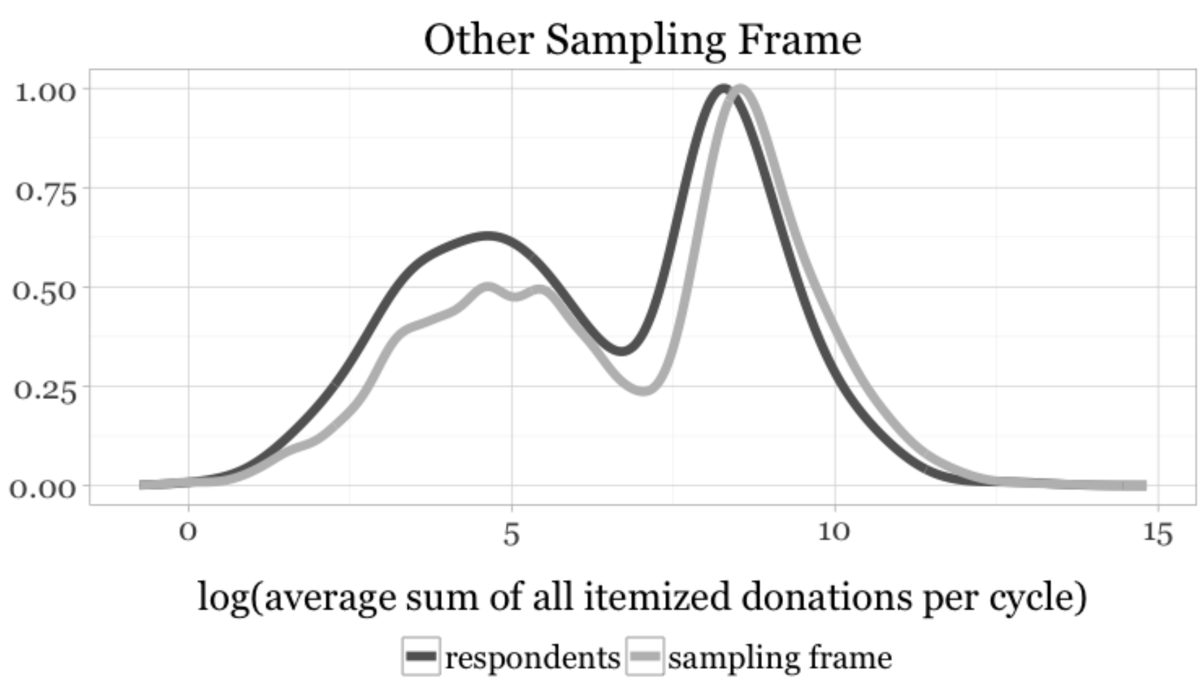

Figure A.10: Log Average Sum of Itemized Donations Per Cycle for the Other Sampling Frame

Similarly, Figure A.10 shows the distribution of the logarithm of these average sums for respondents versus all those in the other sampling frame. Indeed, the average sum of itemized donations in a cycle, conditional on donating, is \$8,008 for everyone in this sampling frame. In contrast, among respondents in the same sampling frame, this average is only \$4,964.

A.3.1.3 Average sum of itemized donations by cycle

Within the Compustat sampling frame, I calculate the average sum of itemized donations to PACs sponsored by Compustat-listed companies (i.e., donors' employers) per cycle conditional on giving (Compustat North America 2017), using individual donation records since the 2008 election cycle provided in Bonica (2016b). Figure A.11 shows the distribution of the logarithm of these average sums for respondents versus everyone in the Compustat sampling frame, which shows that respondents tend to give less itemized donations to their employers' PACs on average than those who did not respond. The average sum of itemized donations to these PACs per cycle, conditional on giving, is \$786 in the Compustat sampling frame. In comparison, within just respondents in this sampling frame, this average is \$534.

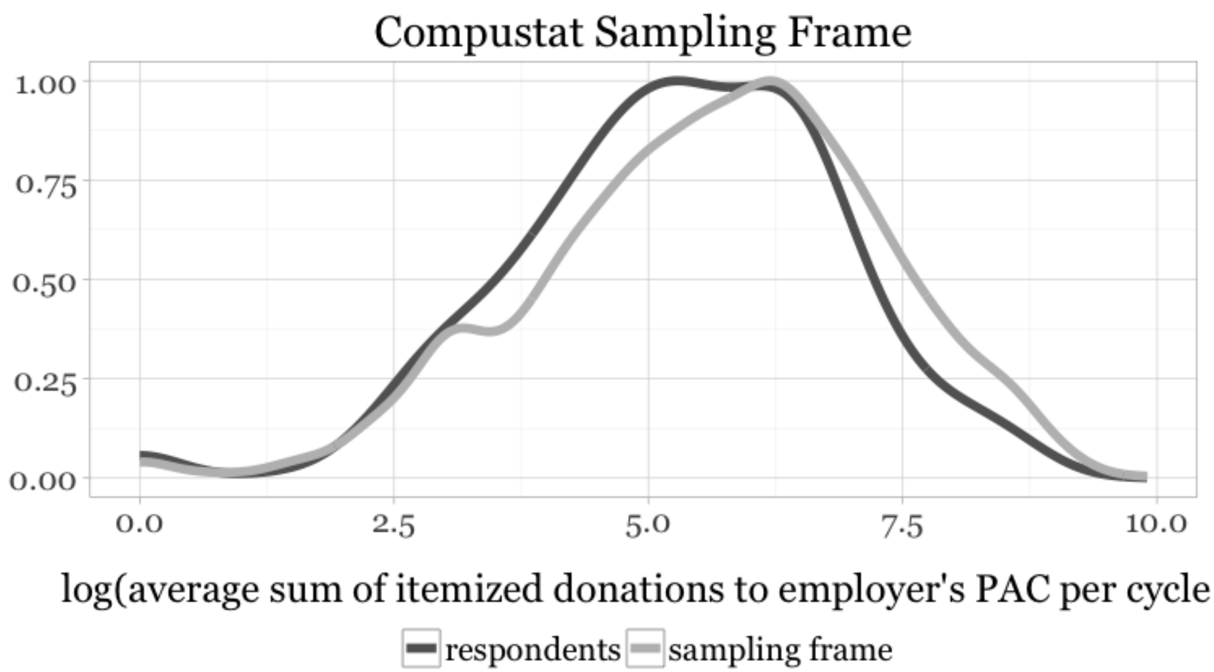

Figure A.11: Log Average Sum of Itemized Donations Per Cycle for the Compustat Sampling Frame

A.3.1.4 Sector

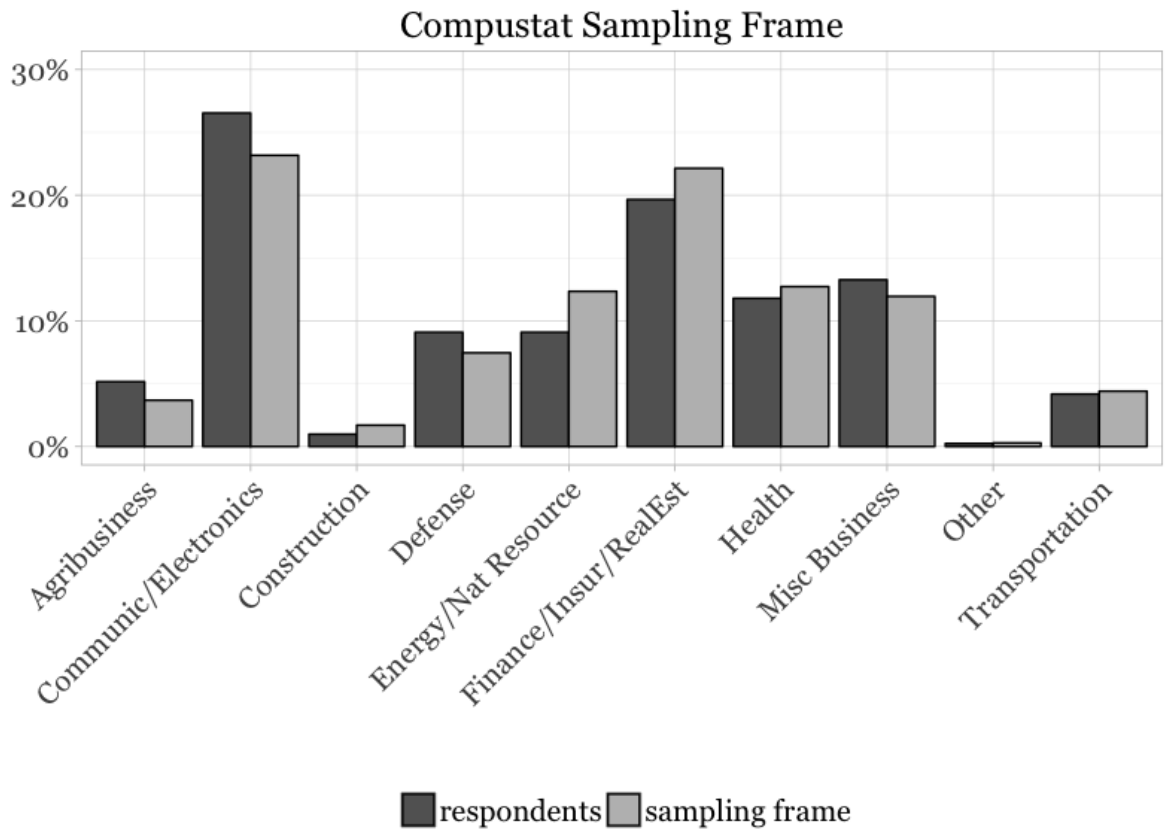

Figure A.12: Distribution of Sectors within the Compustat Sampling Frame

I also compare the distribution of the sectors represented by respondents versus all individual donors in the Compustat sampling frame, where sectors are categorized by OpenSecrets based on market characteristics of donors' employers (Center for Responsive Politics 2018). As shown in Figure A.12, respondents versus non-respondents in the Compustat sampling frame do not differ markedly on representation of sectors. That being said, relative to the entire Compustat sampling frame, respondents do seem to somewhat over-represent the communications/electronics sector while under-representing the finance/insurance/real estate and energy/natural resources sectors.

A.3.2 Questionnaire design

Here I present a sample recruitment letter as well as survey items in the order presented to respondents.

A.3.2.1 Sample recruitment letter

Figure A.13 shows a sample recruitment letter, with the recipient's identifying information redacted for privacy. Note that the letter describes the survey broadly as a study of political opinions because the survey is designed to also be used for other studies aside from my analysis of PAC donors. As a result, sampled donors likely did not perceive the main purpose of the survey as a study of access-seeking PACs when deciding whether to participate. So there is little reason to expect that sampled donors self-selected into participating in the survey based on their knowledge of or interest in the contribution activities of their PACs.

[redacted]

Your Invitation

[redacted] PUBLIC OPINION STUDY

February 15, 2017

11*2*1937***********ALL FOR AADC 940

[redacted]

Dear [redacted]:

You are among a small number of people who has been chosen to participate in a [redacted] public opinion study, a survey being conducted at [redacted] of people across the United States.

You were randomly chosen to complete a survey and represent the opinions and experiences of people like you. Your participation in this survey is voluntary.

The survey will take 5-10 minutes to complete. You can participate on your computer or smartphone by visiting http://www.[redacted].org with the below login information.

To access the survey at [redacted].org, use your access code below.

For: [redacted] Access Code: [redacted]

The survey is available now. We would appreciate it if you were to **respond by March 3.**

The survey will not ask you for any sensitive personal information.

If you have trouble accessing the survey, please email [redacted] at [redacted]. You can also call [redacted].

Thank you for being part of this study.

Sincerely,

[redacted]

P.S. A computer or smartphone is required. The survey cannot be completed by phone or mail.

Figure A.13: Sample Recruitment Letter

A.3.2.2 Screening questions

I use the following screening question to allow respondents from the other (non-Compustat) sampling frame to self-identify whether they have worked for an employer that sponsors a PAC:

> *Does your current employer, or did any of your previous employer(s), sponsor a political action committee (PAC)?*
>
> - *Yes*
> - *No*

198 of the respondents from the other sampling frame answered "Yes" to this screening question. These 198 respondents, together with all 413 respondents from the Compustat sampling frame, collectively form the group of "known PAC donors" that I refer to throughout my analysis of the survey. Those respondents from the other sampling frame who either answered "No" to the screening question or skipped the question are referred to as "other donors".

Respondents who are known PAC donors then saw the following prompt before proceeding to the survey experiment:

> For the next few questions, "your PAC" refers to any political action committee sponsored by your current or former employer. And "your organization" refers to the current or former employer that sponsors "your PAC".

Alternatively, respondents who were not known PAC donors saw the following prompt before proceeding to the survey experiment:

For the purpose of the next few questions, imagine your employer had sponsored a political action committee. "Your PAC" refers to this committee, and "your organization" refers to your employer.

These pages do not record any responses, but they are constructed such that Qualtrics recorded whether a respondent ever saw either of these pages. A respondent who saw neither must, by definition, have dropped out of the survey prior to the experimental section, and is therefore excluded from my analysis of attrition (see Section A.3.5 for detail).

A.3.2.3 Hypothetical PAC solicitation letters

After the screening questions, all respondents were told that they would be asked a series of questions regarding their respective employer's PAC. First, for my survey experiment, every respondent was shown a random permutation of two hypothetical PAC solicitation letters in sequence. Below I provide an example of the first hypothetical solicitation letter that a respondent would receive.

Suppose your PAC sends you a letter that contains the following excerpt:

Our PAC works hard to enhance representation of our organization's interests in Washington, D.C. And thanks to your continued support for our PAC, in the last election cycle alone we successfully defeated several bills that would have threatened the growth of our organization. We could not have done so, however, without allies in Congress who make your voices heard throughout the legislative process.

As we anticipate new challenges ahead following the latest shift in congressional power, we need our allies from both sides of the political spectrum to be able to continue fighting for us on Capitol Hill. Therefore, our PAC has decided to support the following list of legislators–all of whom share our organization's key principles–in this federal election cycle:

- Roberta S. Bow (R)
- Christina J. Frazier (D)
- Frances D. Guerrero (R)
- Chris Lackey (R)
- Robert H. Marion (R)
- Ernest C. Monroe (R)
- Scott Myers (R)
- Glenn P. Towns (R)
- Margaretta P. Webre (R)

How much would you be willing to donate to your PAC after reading this letter? Please enter a number between 0 and 5,000. If you would not be willing to donate anything, just enter 0. Alternatively, if you would be willing to donate more than $5,000, just enter 5,000.

The second letter assigned to each respondent is almost identical except for two differences. First, the prompt becomes "Suppose that, *in the next election cycle*, your PAC sends you another letter that contains the following excerpt" (emphasis added). Secondly, the list of candidates as well as the number of Republican candidates will be different.

These letters are designed to resemble the solicitation letters and other materials that access-seeking PACs would send out to prospective donors, though a couple of important differences remain for the purpose of experimental design. First, real solicitation letters tend to focus on describing policy concerns that are specific to the parent organization's economic interests. While it is theoretically possible to customize the solicitation letters to each respondent's sector of employment, I decided not to do so both out of respect for their privacy and to make the letters as comparable across respondents as possible.

Second, real solicitation letters mention real candidates. I decided to use fictitious candidates instead partly because real candidates could affect PAC donors' willingness to give for reasons other than their party affiliations, such as policy expertise, familiarity, and affect. Additionally, because many access-seeking PACs donate to politicians based on organization- or industry-specific characteristics such as relevance of committee membership and geographic importance, it would be hard to construct hypothetical PAC solicitation letters that reference real candidates in a way that would seem believable to PAC donors across a diverse set of backgrounds. Customizing these letters would also make the treatment conditions hard to compare across respondents.

In addition, one might also wonder why I did not include a treatment condition of candidates without partisan labels. While contrasts in donor behavior when shown candidates with or without partisan labels could be illuminating, my theory does not have clear predictions for how willingness to donate to one's PAC should differ in this case. On one hand, PAC solicitation letters that exclude recipients' partisan affiliations may downplay the salience of partisanship in PAC donors' decisionmaking relative to other factors (e.g., how much PAC contributions help to advance the interests of one's parent organization), which may increase willingness to donate. On the other hand, insofar as PAC donors care about how well PAC activities align with their own partisan preferences, omitting the partisan affiliations of those who receive PAC contributions may backfire by raising suspicion that the partisan identity of the recipients, if publicized, could offend many PAC donors.

Moreover, while related existing work provides some reference point for how the inclusion or exclusion of partisan labels could affect the behavior of PAC donors, their findings might be ill-suited for the context of my study. For example, McConnell et al. (2018) conducted a field experiment to see if job seekers' reservation wages differ by employers' partisan identities. They find that relative to either the case where an employer is an out-partisan or the case where an employer's partisan leaning is unknown, job seekers demand lower reservation wages if an employer is a co-partisan. However, the same qualitative patterns need not manifest in PAC giving because the contexts are different: McConnell et al. (2018) explicitly focus on understanding the role of partisanship in apolitical economic transactions, whereas I am studying partisanship in the context of donations to access-seeking PACs, which are by definition political in nature.

A.3.2.4 Question on donors' self-reported preference

In addition to the survey experiment, I also directly ask all respondents with what probability they would stop donating if their PACs supported politicians that they dislike.

> *Imagine your PAC had contributed to a politician candidate that you oppose. What is the chance that you would stop donating to your PAC as a result? In the box below, please indicate a number between 0 and 100.*
>
> *[ ]%.*

A.3.2.5 Questions on donors' knowledge of PAC activities

Next, I ask each respondent from the Compustat sampling frame to guess the percentage of their PAC's contributions in the last federal election cycle that went to Democratic versus Republican candidates, respectively. I wrote this set of questions in Qualtrics such that the pair of percentages that each respondent reported must add up to 100%.

> *If you had to guess, what percentage of your PAC's contributions went to Democrat candidates in the 2015-2016 federal election cycle? In the box below, please enter a number between 0 and 100 that represents your best guess.*
> *[ ]%.*

> *And what percentage of your PAC's contributions went to Republican candidates in the 2015-2016 federal election cycle? In the box below, please enter a number between 0 and 100 that represents your best guess.*
> *[ ]%.*

A.3.2.6 Questions on donors' perception of PAC governance

I then asked the known PAC donors a series of open-ended questions on their perceptions of PAC governance.

> *Regarding people in your organization who participate in the management or oversight of your PAC in some official capacity (e.g. being a representative on the PAC board), what have they done, if any, to get feedback from the rest of the organization on which candidates your PAC should or should not contribute to?*
> *And how often do they incorporate such feedback into into your PAC's formal decisionmaking?*

Regarding people in your organization who do not participate in the management or oversight of your PAC in any official capacity, how often do they try to persuade the PAC management on which candidates the PAC should or should not contribute to?
And what are the common reasons that they cite to support or oppose a candidate?

A.3.2.7 Question on donors' involvement in PAC governance

Finally, I ask each known PAC donor the following question:

Which of the following statement is accurate?

- *I participate in the management or oversight of my PAC in some official capacity.*
- *I do not participate in the management or oversight of my PAC in any official capacity.*

A.3.3 External validity of respondents' knowledge of PAC contributions

While my survey demonstrates that respondents from the Compustat sampling frame are informed about how their respective PACs allocate contributions across parties, one might wonder about external validity of this finding. Here I address two important concerns in this regard: selection into participating in the survey and respondent motivation.

One concern is that if respondents tend to be more informed about their PACs' activities than non-respondents in my sampling frame, my survey result could overstate how well informed a representative PAC donor is about her PAC's contribution patterns. While plausible in theory, such selection bias is likely to be modest in my survey for two reasons.

First, as shown in Section A.3.2.1, the sample recruitment letter never mentions PACs, interest groups, or anything else that could lead sampled donors to select into responding to the survey based on pre-existing knowledge of or interest in their PACs' activities.

Second, relative to rank-and-file PAC donors, respondents do not seem to have any more privileged access to information about their PACs' activities. 90.3% of respondents who are PAC donors stated in the survey that they do not participate in the management or oversight of their PACs in any official capacity. Moreover, as detailed in the paper, the channels through which respondents learn about their PACs' choices of candidates closely resemble a typical information environment for employees in politically active corporations (Hertel-Fernandez 2016).

Another concern is that respondents could be motivated to seek out information about their PACs' activities when asked about them in the survey. If such behavior is systematic, my survey could overstate how informed PAC donors are in more realistic settings (i.e., in the absence of any motivation effects from the survey).

While I am not able to directly track this type of behavior,[6] I estimate that it is rare for respondents to have looked up the correct answers. More importantly, such behavior barely changes my conclusion regarding the extent to which PAC donors are informed about their PACs' contribution patterns. Among all respondents' guesses of what percentage of their PACs' contributions went to Republican (as opposed to Democratic) candidates, only 2.37% of these guesses were correct (i.e., equal to the exact actual percentages). This puts an upper bound on the fraction of respondents who might have looked up the correct answers online or elsewhere. Even if we make the extreme assumption that none of the respondents who answered correctly did so on their own merit, the remaining guesses still have a correlation of 0.267 with the actual numbers. This represents only a slight decrease from the overall correlation of 0.297 between all guesses and their

[6]Regrettably, I did not have the foresight to time how long respondents took to answer this question when designing the survey. I also verified with Qualtrics' support team that such data cannot be recovered *ex post*.

corresponding answers.

A.3.4 Summary statistics for the experiment

Table A.17 reports the summary statistics of all variables used in my analysis of the survey experiment. The number of observations are smaller than 1,760 because I do not include respondents who skipped the questions for this experiment (attrition will be addressed shortly). Experimental results shown in the paper are estimated using a binary measure of respondent partisanship, where $Indiv\_R_i \in \{-1/2, 1/2\}$ is negative if and only if the respondent is Democratic-leaning. In my pre-analysis plan I also consider an alternative, continuous measure of $Indiv\_R_i$ that equals respondent i's CFscore divided by 4 (so as to roughly fall within the $[-1/2, 1/2]$ range). The more negative this CFscore-based $Indiv\_R_i$, the more liberal the respondent (Bonica 2016b).

Table A.17: Summary Statistics for Survey Experiment

Statistic	N	Mean	St. Dev.	Min	Max
PAC_R_{i1}	1,623	0.056	0.272	−0.278	0.389
PAC_R_{i2}	1,623	0.055	0.272	−0.278	0.389
$Amount_{i1}$	1,487	277.642	838.559	0	5,000
$Amount_{i2}$	1,483	279.055	837.385	0	5,000
$\log(Amount_{i1} + 1)$	1,487	1.960	2.830	0.000	8.517
$\log(Amount_{i2} + 1)$	1,483	2.013	2.838	0.000	8.517
$I(Amount_{i1} \geq 200)$	1,487	0.210	0.407	0	1
$I(Amount_{i2} \geq 200)$	1,483	0.213	0.410	0	1
$I(Amount_{i1} > 0)$	1,487	0.352	0.478	0	1
$I(Amount_{i2} > 0)$	1,483	0.365	0.482	0	1
$Indiv\_R_i$ (binary)	1,623	−0.191	0.462	−0.500	0.500
$Indiv\_R_i$ (CFscore-based)	1,623	−0.146	0.316	−0.708	1.220

Tables A.18 and A.19 report summary statistics separately for respondents who are known to be PAC donors versus those who are not.

Table A.18: Summary Statistics for Survey Experiment (Known PAC Donors)

Statistic	N	Mean	St. Dev.	Min	Max
PAC_R_{i1}	589	0.051	0.276	−0.278	0.389
PAC_R_{i2}	589	0.060	0.267	−0.278	0.389
$Amount_{i1}$	568	395.968	948.240	0	5,000
$Amount_{i2}$	568	370.996	896.139	0	5,000
$\log(Amount_{i1} + 1)$	568	2.768	3.066	0.000	8.517
$\log(Amount_{i2} + 1)$	568	2.680	3.054	0.000	8.517
$I(Amount_{i1} \geq 200)$	568	0.322	0.468	0	1
$I(Amount_{i2} \geq 200)$	568	0.315	0.465	0	1
$I(Amount_{i1} > 0)$	568	0.486	0.500	0	1
$I(Amount_{i2} > 0)$	568	0.470	0.500	0	1
$Indiv\_R_i$ (binary)	589	−0.094	0.491	−0.500	0.500
$Indiv\_R_i$ (CFscore-based)	589	−0.075	0.333	−0.708	0.520

Table A.19: Summary Statistics for Survey Experiment (Other Donors)

Statistic	N	Mean	St. Dev.	Min	Max
PAC_R_{i1}	1,034	0.059	0.270	−0.278	0.389
PAC_R_{i2}	1,034	0.052	0.275	−0.278	0.389
$Amount_{i1}$	919	204.508	754.244	0	5,000
$Amount_{i2}$	915	221.980	793.904	0	5,000
$\log(Amount_{i1} + 1)$	919	1.460	2.550	0.000	8.517
$\log(Amount_{i2} + 1)$	915	1.599	2.613	0.000	8.517
$I(Amount_{i1} \geq 200)$	919	0.140	0.348	0	1
$I(Amount_{i2} \geq 200)$	915	0.150	0.357	0	1
$I(Amount_{i1} > 0)$	919	0.270	0.444	0	1
$I(Amount_{i2} > 0)$	915	0.301	0.459	0	1
$Indiv\_R_i$ (binary)	1,034	−0.247	0.435	−0.500	0.500
$Indiv\_R_i$ (CFscore-based)	1,034	−0.186	0.299	−0.595	1.220

A.3.5 Tests for differential attrition

Since not every respondent answered the questions in the experiment, I verify in this section that attrition does not depend on treatment assignment.

Because I ask every respondent to report her willingness to donate after seeing each of the two letters randomly assigned to her, attrition for each letter is coded in the following way: 0 if she indicated her willingness to give for said letter; 1 if she did not indicate her willingness to give for said letter *and* did not drop out of the survey before reaching the experimental section; and NA if she dropped out of the survey before receiving her treatment assignment. To clarify, my survey was constructed in Qualtrics such that treatment conditions were randomly assigned to respondents at the beginning of the survey (i.e., as hidden embedded values), but respondents do not observe their assignment (i.e., two PAC solicitation letters) until they reached the experimental section of the survey. While treatment conditions were assigned to these "dropouts", I could verify in Qualtrics that they never saw the screening page before the experiment, so attrition of these dropouts could not have been affected by their assigned letters. These dropouts represent 11.8% of all participants who did not respond to the first question in the experiment and 11.4% of all those who did not respond to the second question in the experiment. All dropouts have been removed from my attrition analysis.

Recall that every respondent was randomly assigned to one of the six treatments, defined in terms of the number of Republican candidates in each of the two letters that a respondent received in sequence: $(2,5)$, $(2,8)$, $(5,2)$, $(5,8)$, $(8,2)$, $(8,5)$. Because respondents were asked to report their willingness to donate following each randomly assigned letters (and that they were not allowed to revise their responses once they answered a question), a respondent only observed whether her first letter had 2, 5, or 8 Republican candidates when asked to indicate her willingness to donate upon seeing this letter. In comparison, by the time a respondent was asked for her willingness to donate following her second letter, she had observed the full treatment condition assigned to her. As a result, I analyze attrition for the first letter by whether it contains 2, 5, or 8 Republican candidates, whereas I analyze attrition for the second letter by whether the permutation of Republican candidates is $(2,5)$, $(2,8)$, $(5,2)$, $(5,8)$, $(8,2)$, $(8,5)$.

First, I verify that attrition of either letter does not depend on treatment assignment. Table A.20 reports attrition rates for the first letter with respect to whether it contained 2 (the excluded category), 5, or 8 Republicans. The p-value for the corresponding heteroskedasticity-robust F-test is 0.465.

Table A.20: Heteroskedasticity-Robust F-Tests for Attrition for Letter 1

	Indicator for Attrition for Letter 1
I(first letter has 5 R's)	-0.017
	(0.016)
I(first letter has 8 R's)	-0.017
	(0.016)
Constant	0.085^{***}
	(0.012)
Observations	1,623
Note:	*p<0.05; **p<0.01; ***p<0.001
	Robust standard errors in parentheses

Similarly, Table A.21 reports attrition rates for the second letter with respect to full treatment assignment: $(2,5)$ (the excluded category), $(2,8)$, $(5,2)$, $(5,8)$, $(8,2)$, $(8,5)$. The p-value for the corresponding heteroskedasticity-robust F-test is 0.821.

Table A.21: Heteroskedasticity-Robust F-Tests for Attrition for Letter 2

	Indicator for Attrition for Letter 2
treatment (2,8)	0.023
	(0.024)
treatment (5,2)	−0.008
	(0.022)
treatment (5,8)	0.004
	(0.023)
treatment (8,2)	−0.003
	(0.022)
treatment (8,5)	−0.0003
	(0.022)
Constant	0.074^{***}
	(0.016)
Observations	1,623
Note:	*p<0.05; **p<0.01; ***p<0.001
	Robust standard errors in parentheses

Second, I verify that attrition of either letter does not depend on treatment assignment interacted with respondents' partisanship. Table A.22 reports attrition rates for the first letter, controlling for treatment assignment, respondents' partisan leanings, and their interactions. Column 1 uses the binary measure of partisanship, while column 2 uses the CFscore-based measure. In either case, there appears to be no differential attrition. The p-values for the heteroskedasticity-robust F-test on the interaction terms are 0.212 for column 1 and 0.184 for column 2.

Table A.22: Heteroskedasticity-Robust F-Tests for Attrition for Letter 1

	Indicator for Attrition for Letter 1	
	(1)	(2)
I(first letter has 5 R's)	-0.028	-0.029
	(0.019)	(0.019)
I(first letter has 8 R's)	-0.019	-0.018
	(0.019)	(0.020)
$Indiv\_R_i$	0.059^{*}	0.082^{*}
	(0.029)	(0.042)
I(first letter has 5 R's) $\times Indiv\_R_i$	-0.056	-0.081
	(0.038)	(0.054)
I(first letter has 8 R's) $\times Indiv\_R_i$	-0.003	0.001
	(0.039)	(0.056)
Constant	0.097^{***}	0.097^{***}
	(0.014)	(0.015)
Measure of $Indiv\_R_i$	binary	CFscore-based
Observations	1,623	1,623
Note:	*p<0.05; **p<0.01; ***p<0.001	
	Robust standard errors in parentheses	

Similarly, Table A.23 reports attrition rates for the second letter, controlling for treatment assignment, respondents' partisan leaning, and their interactions. As before, column 1 uses the binary measure of partisanship, while column 2 uses the CFscore-based measure. In both cases, it appears that Republican-leaning respondents were marginally less likely to attrite from the second letter if the permutation of the numbers of Republicans in their assigned letters was either $(2, 8)$ or $(5, 8)$. Nonetheless, the p-values for the heteroskedasticity-robust F-test for the coefficient on all interaction terms are 0.206 for column 1 and 0.209 for column 2, failing to reject the null hypothesis that there was no differential attrition by treatment assignment interacted with respondents' partisanship.

Table A.23: Heteroskedasticity-Robust F-Tests for Attrition for Letter 2

	Indicator for Attrition for Letter 2	
	(1)	(2)
treatment (2,8)	-0.00005	-0.001
	(0.028)	(0.029)
treatment (5,2)	-0.026	-0.025
	(0.027)	(0.028)
treatment (5,8)	-0.018	-0.019
	(0.027)	(0.027)
treatment (8,2)	-0.014	-0.013
	(0.028)	(0.029)
treatment (8,5)	-0.012	-0.010
	(0.027)	(0.028)
$Indiv\_R_i$	0.102^{*}	0.135^{*}
	(0.042)	(0.058)
treatment (2,8) $\times Indiv\_R_i$	-0.120^{*}	-0.164^{*}
	(0.056)	(0.080)
treatment (5,2) $\times Indiv\_R_i$	-0.095	-0.122
	(0.055)	(0.076)
treatment (5,8) $\times Indiv\_R_i$	-0.113^{*}	-0.155^{*}
	(0.054)	(0.075)
treatment (8,2) $\times Indiv\_R_i$	-0.060	-0.072
	(0.056)	(0.079)
treatment (8,5) $\times Indiv\_R_i$	-0.043	-0.045
	(0.055)	(0.078)
Constant	0.094^{***}	0.094^{***}
	(0.021)	(0.021)
Measure of $Indiv\_R_i$	binary	CFscore-based
Observations	1,623	1,623
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses	

A.3.6 Miscellaneous pre-registered primary hypothesis tests

A.3.6.1 Fixed-effects models with continuous measure of partisanship

To test my primary hypothesis that survey respondents report higher willingness to give when receiving solicitation letters with a higher share of co-partisan candidates, the paper uses the binary version of $Indiv\_R_i$ to estimate Specification 3 in the paper. In comparison, here I estimate the same regression

$$O(Amount_{it}) = \beta_i + \tau_t + \beta_1 PAC\_R_{it} + \beta_2 (PAC\_R_{it} \times Indiv\_R_i) + \zeta_{it} \tag{A.18}$$

but instead using a CFscore-based $Indiv\_R_i$. Notwithstanding this different measure of respondent partisanship, I continue to expect that $\beta_2 > 0$.

Tables A.24, A.25, A.26, and A.27 display estimates using specification A.18, where the outcome variables are $Amount_{it}$, $\log(Amount_{it} + 1)$, $I(Amount_{it} \geq 200)$, and $I(Amount_{it} > 0)$, respectively. In each of these tables, the first column is estimated on the sub-sample of respondents who are known to have given to a corporate PAC before, while the second column uses the remaining respondents. All four tables report robust standard errors.

Across Tables A.24, A.25, A.26, and A.27, the interaction term between $Indiv\_R_i$ and PAC_R_{it} is positive and statistically significant. These qualitative results are identical to those shown in the paper.

Table A.24: Fixed-effects Estimates for Dollar Amount of Donation

	$Amount_{it}$	
	(1)	(2)
$PAC\_R_{it} \times Indiv\_R_i$	839.189***	624.030***
	(191.811)	(167.176)
PAC_R_{it}	83.260	21.267
	(69.830)	(57.841)
Sample	known PAC donors	other donors
Observations	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses	

Table A.25: Fixed-effects Estimates for Log of Dollar Amount of Donation

	$\log(Amount_{it})$	
	(1)	(2)
$PAC\_R_{it} \times Indiv\_R_i$	4.600***	4.186***
	(0.685)	(0.546)
PAC_R_{it}	0.017	-0.021
	(0.236)	(0.195)
Sample	known PAC donors	other donors
Observations	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses	

Table A.26: Fixed-effects Estimates for Indicator for Itemized Donation

	$I(Amount_{it} \geq 200)$	
	(1)	(2)
$PAC\_R_{it} \times Indiv\_R_i$	0.355***	0.302***
	(0.070)	(0.053)
PAC_R_{it}	0.006	0.009
	(0.035)	(0.026)
Sample	known PAC donors	other donors
Observations	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001	
	Robust standard errors in parentheses	

Table A.27: Fixed-effects Estimates for Indicator for Positive Donation

	$I(Amount_{it} > 0)$	
	(1)	(2)
$PAC\_R_{it} \times Indiv\_R_i$	0.507***	0.563***
	(0.077)	(0.065)
PAC_R_{it}	−0.007	−0.014
	(0.038)	(0.033)
Sample	known PAC donors	other donors
Observations	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001	
	Robust standard errors in parentheses	

A.3.6.2 Random-effect models

Here I test my primary hypothesis using the following fixed-effect specification:[7]

[7] In my pre-analysis plan, I accidentally omitted $Indiv\_R_i$ from the set of covariates in equation A.19. I apologize for this mistake. Controlling for $Indiv\_R_i$ or not has negligible effect on the point estimates of β_3.

$$O(Amount_{it}) = \beta_i + \tau_t + \beta_1 PAC\_R_{it} + \beta_2 Indiv\_R_i + \beta_3 (PAC\_R_{it} \times Indiv\_R_i) + \zeta_{it} \tag{A.19}$$

where β_i is now a random- (rather than fixed-) effect for each respondent i, and I additionally control for common trend across letters with τ_t. Here, my primary hypothesis would be that $\beta_3 > 0$, i.e., willingness to donate increases in the number of co-partisan candidates in the solicitation letters that one receives.

Tables A.28, A.29, A.30, and A.31 display fixed-effects regression estimates, where the outcome variables are $Amount_{it}$, $\log(Amount_{it} + 1)$, $I(Amount_{it} \geq 200)$, and $I(Amount_{it} > 0)$, respectively. In each of these tables, the first and third columns are estimated on the sub-sample of respondents who are known to have given to a corporate PAC before, while the second and fourth columns use the remaining respondents. Moreover, I use the binary measure of $Indiv\_R_i$ in columns 1 and 2, whereas I use the CFscore-based version in columns 3 and 4. All four tables report robust standard errors.

Across Tables A.28, A.29, A.30, and A.31, the interaction term between $Indiv\_R_i$ and PAC_R_{it} is positive and statistically significant. These qualitative results are identical to those shown in the paper.

Table A.28: Random-effects Estimates for Dollar Amount of Donation

	$Amount_{it}$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	599.607***	417.659***	825.586***	587.649***
	(133.856)	(105.443)	(187.251)	(155.811)
$Indiv\_R_i$	197.368*	155.006*	309.457**	223.905*
	(81.721)	(72.443)	(114.860)	(95.048)
PAC_R_{it}	71.816	22.235	76.085	29.016
	(66.928)	(52.721)	(68.205)	(53.786)
Constant	400.325***	258.977***	405.641***	262.375***
	(40.861)	(36.221)	(41.810)	(35.843)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.29: Random-effects Estimates for Log of Dollar Amount of Donation

	$\log(Amount_{it})$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	2.909***	3.051***	4.298***	4.111***
	(0.457)	(0.354)	(0.665)	(0.517)
$Indiv\_R_i$	0.081	−0.193	0.193	−0.024
	(0.246)	(0.194)	(0.358)	(0.267)
PAC_R_{it}	−0.063	0.010	−0.025	0.019
	(0.229)	(0.177)	(0.229)	(0.185)
Constant	2.737***	1.517***	2.746***	1.564***
	(0.123)	(0.097)	(0.125)	(0.100)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.30: Random-effects Estimates for Indicator for Itemized Donation

	$I(Amount_{it} \geq 200)$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	0.331***	0.298***	0.477***	0.375***
	(0.068)	(0.049)	(0.098)	(0.070)
$Indiv\_R_i$	-0.0004	0.010	0.009	0.053
	(0.037)	(0.026)	(0.054)	(0.035)
PAC_R_{it}	-0.004	0.014	-0.0001	0.009
	(0.034)	(0.025)	(0.034)	(0.026)
Constant	0.319***	0.151***	0.320***	0.159***
	(0.019)	(0.013)	(0.019)	(0.013)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.31: Random-effects Estimates for Indicator for Positive Donation

	$I(Amount_{it} > 0)$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	0.478***	0.559***	0.718***	0.770***
	(0.075)	(0.062)	(0.110)	(0.091)
$Indiv\_R_i$	-0.014	-0.081^{**}	-0.012	-0.076
	(0.039)	(0.031)	(0.058)	(0.044)
PAC_R_{it}	-0.009	-0.006	-0.002	-0.001
	(0.037)	(0.031)	(0.037)	(0.032)
Constant	0.477***	0.271***	0.478***	0.278***
	(0.020)	(0.015)	(0.020)	(0.016)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

A.3.6.3 Pooled models

Finally, I test my primary hypothesis using pooled regressions of the following form:

$$O(Amount_{it}) = \theta_0 + \theta_1 PAC\_R_{it} + \theta_2 Indiv\_R_i + \theta_3 (PAC\_R_{it} \times Indiv\_R_i) + \epsilon_{it} \quad \text{(A.20)}$$

where I consider both measures of $Indiv\_R_i$. Analogous to the hypothesis test in random-effects regressions, I expect $\theta_3 > 0$ in equation A.20.

Tables A.32, A.33, A.34, and A.35 report pooled regression estimates using specification A.20, where the outcome variables are $Amount_{it}$, $\log(Amount_{it} + 1)$, $I(Amount_{it} \geq 200)$, and $I(Amount_{it} > 0)$, respectively. In each table, the first two columns use the binary version of $Indiv\_R_i$, whereas the latter two use the CFscore-based version. In addition, the first and third columns of each of these tables focus on the known PAC donors, while the second and fourth columns are based on the remaining respondents. All four tables show robust standard errors.

In almost all cases in Tables A.32, A.33, A.34, and A.35, the interaction term between $Indiv\_R_i$ and PAC_R_{it} is positive and statistically significant at the 5% level or lower. The only exceptions are estimates obtained from the other donors (i.e., donors who are not known to be eligible PAC donors) in Table A.32, where the point estimates are positive but do not attain conventional levels of statistical significance.

Table A.32: Pooled Estimates for Dollar Amount of Donation

	$Amount_{it}$			
$PAC\_R_{it} \times Indiv\_R_i$	559.966*	333.515	750.176*	454.484
	(227.493)	(191.749)	(323.524)	(257.155)
$Indiv\_R_i$	207.122***	162.124**	322.266***	233.878**
	(61.377)	(55.908)	(86.573)	(74.293)
PAC_R_{it}	18.749	51.428	23.196	54.087
	(113.746)	(95.874)	(116.368)	(94.739)
Constant	403.719***	257.131***	409.051***	260.689***
	(30.688)	(27.954)	(31.397)	(27.689)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.33: Pooled Estimates for Log of Dollar Amount of Donation

	$\log(Amount_{it})$			
$PAC\_R_{it} \times Indiv\_R_i$	2.228**	2.857***	3.170**	3.834***
	(0.688)	(0.536)	(1.004)	(0.748)
$Indiv\_R_i$	0.137	−0.179	0.282	−0.006
	(0.192)	(0.149)	(0.279)	(0.206)
PAC_R_{it}	−0.213	0.115	−0.189	0.113
	(0.344)	(0.268)	(0.347)	(0.276)
Constant	2.751***	1.507***	2.762***	1.556***
	(0.096)	(0.075)	(0.097)	(0.077)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.34: Pooled Estimates for Indicator for Itemized Donation

	$I(Amount_{it} \geq 200)$			
$PAC\_R_{it} \times Indiv\_R_i$	0.247*	0.284***	0.343*	0.346***
	(0.103)	(0.071)	(0.150)	(0.099)
$Indiv\_R_i$	0.006	0.011	0.019	0.055*
	(0.029)	(0.020)	(0.042)	(0.027)
PAC_R_{it}	−0.039	0.025	−0.037	0.017
	(0.052)	(0.036)	(0.052)	(0.037)
Constant	0.322***	0.150***	0.323***	0.158***
	(0.014)	(0.010)	(0.015)	(0.010)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001			
	Robust standard errors in parentheses			

Table A.35: Pooled Estimates for Indicator for Positive Donation

	$I(Amount_{it} > 0)$			
$PAC\_R_{it} \times Indiv\_R_i$	0.382***	0.537***	0.558***	0.744***
	(0.110)	(0.086)	(0.163)	(0.124)
$Indiv\_R_i$	−0.007	−0.081***	−0.0002	−0.076*
	(0.031)	(0.024)	(0.045)	(0.034)
PAC_R_{it}	−0.015	0.010	−0.010	0.014
	(0.055)	(0.043)	(0.055)	(0.045)
Constant	0.478***	0.269***	0.479***	0.276***
	(0.015)	(0.012)	(0.016)	(0.012)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	1,136	1,834	1,136	1,834
Note:	*p<0.05; **p<0.01; ***p<0.001			
	Robust standard errors in parentheses			

A.3.7 *Additional tests for primary hypothesis*

Recall in Section A.3.5 that I do not reject the null hypothesis of no differential attrition for respondents' second letters by treatment conditions interacted with respondents' partisanship: the p-values for the heteroskedasticity-robust F-tests on all interaction terms are 0.206 and 0.209, depending on whether respondents' partisanship is measured as a binary or continuous variable. That being said, Table A.23 shows that the coefficients on either treatment $(2, 8)$ or $(5, 8)$ interacted with $Indiv\_R_i$ are statistically significant, suggesting that on average Republican-leaning respondents were marginally less likely to attrite from their second letters if they were assigned to either of these treatment conditions.

As a robustness check, here I present fixed-effects regressions excluding treatment conditions treatment $(2, 8)$ or $(5, 8)$. Tables A.36, A.37, A.38, and A.39 report pooled regression estimates using specification A.20, where the outcome variables are $Amount_{it}$, $\log(Amount_{it} + 1)$, $I(Amount_{it} \geq 200)$, and $I(Amount_{it} > 0)$, respectively. In each table, the first two columns use the binary version of $Indiv\_R_i$, whereas the latter two use the CFscore-based version. In addition, the first and third columns of each of these tables focus on the known PAC donors, while the second and fourth columns are based on the remaining respondents. All four tables show robust standard errors.

Table A.36: Fixed-effects Estimates for Dollar Amount of Donation

	$Amount_{it}$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	764.071***	494.348***	1,067.933***	683.163***
	(206.783)	(120.646)	(292.510)	(184.475)
PAC_R_{it}	196.423	20.968	207.509*	26.832
	(103.391)	(60.323)	(105.700)	(60.508)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	771	1,225	771	1,225
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.37: Fixed-effects Estimates for Log of Dollar Amount of Donation

	$\log(Amount_{it})$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	3.774^{***}	3.231^{***}	5.579^{***}	4.367^{***}
	(0.628)	(0.468)	(0.916)	(0.661)
PAC_R_{it}	0.393	-0.142	0.468	-0.125
	(0.314)	(0.234)	(0.315)	(0.242)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	771	1,225	771	1,225
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.38: Fixed-effects Estimates for Indicator for Itemized Donation

	$I(Amount_{it} \geq 200)$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	0.437^{***}	0.359^{***}	0.613^{***}	0.441^{***}
	(0.092)	(0.071)	(0.134)	(0.097)
PAC_R_{it}	0.052	0.033	0.059	0.025
	(0.046)	(0.036)	(0.046)	(0.037)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	771	1,225	771	1,225
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.39: Fixed-effects Estimates for Indicator for Positive Donation

	$I(Amount_{it} > 0)$			
	(1)	(2)	(3)	(4)
$PAC\_R_{it} \times Indiv\_R_i$	0.629***	0.554***	0.950***	0.773***
	(0.101)	(0.077)	(0.150)	(0.111)
PAC_R_{it}	0.054	−0.052	0.067	−0.043
	(0.050)	(0.039)	(0.050)	(0.040)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	771	1,225	771	1,225
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Across Tables A.36, A.37, A.38, and A.39, the coefficient on $PAC\_R_{it} \times Indiv\_R_i$ is positive and statistically significant. Moreover, the point estimates are comparable, if not bigger, than the ones obtained without excluding treatment conditions treatment $(2, 8)$ or $(5, 8)$.

A.3.8 Secondary hypothesis tests

As explained in the paper, real campaign finance records suggest that changes in PAC contributions may have persistent effects on donor behavior in the form of entries and exits. Entries occur when eligible but inactive donors start donating to their PAC after an increase in the share of PAC contributions going to co-partisan recipients. And exits happen when active donors stop donating to their PAC altogether after the allocation of PAC contributions to donors' preferred party declines.

Since the survey experiment is only a two-period panel, it is impossible to construct measures of entries and exits that correspond exactly to those defined in the observational study. Nonetheless, insofar as entries and exits demonstrate persistent effects on donor behavior, I can examine whether respondents' treatment condition in the first letter moderates the treatment effect in the second letter in the following ways.

In the survey experiment, define entries as cases where a respondent's willingness to donate was initially high because the first letter she received contained a high number of co-partisan candidates. Analogously, exits are cases where a respondent's willingness to donate was initially low because the number of co-partisan candidates in her first letter was small. I expect an average respondent's willingness to donate to become less sensitive to the treatment condition in her second letter if she either "entered" or "exited" after seeing her first letter.

To test this secondary hypothesis, I estimate[8]

$$\begin{aligned} O(Amount_{i2}) = \gamma_0 + \gamma_1 PAC\_R_{i2} + \gamma_2 Indiv\_R_i + \gamma_3 (PAC\_R_{i2} \times Indiv\_R_i) \\ + \gamma_4 (PAC\_R_{i2} \times Indiv\_R_i \times I(Entry_i)) \\ + \gamma_5 (PAC\_R_{i2} \times Indiv\_R_i \times I(Exit_i)) + \zeta_{i2} \end{aligned} \tag{A.21}$$

where the indicator $I(Entry_i)$ equals 1 if and only if either $(Indiv\_R_i \leq 0) \& (PAC\_R_{i1} = 2/9 - 1/2 = -5/18)$ or $(Indiv\_R_i > 0) \& (PAC\_R_{i1} = 8/9 - 1/2 = 7/18)$, and indicator $I(Exit_i)$ equals 1 if and only if either $(Indiv\_R_i \leq 0) \& (PAC\_R_{i1} = 8/9 - 1/2 = 7/18)$ or $(Indiv\_R_i > 0) \& (PAC\_R_{i1} = 2/9 - 1/2 = -5/18)$.

Given equation A.21, a weak version of my secondary hypothesis is $(\gamma_3 > 0) \& (\gamma_4 < 0) \& (\gamma_5 < 0)$. In words, this says that, on average, respondents still report higher willingness to give after seeing their respective second letter if said letter contains a high number of co-partisan candidates. However, such effect will be moderated by either entry or exit.

[8] In my pre-analysis plan, I accidentally wrote t where it should be "2". In other words, I forgot to specify that I am only looking at outcomes for the second letters. I apologize for this mistake. The mis-specified regressions in my pre-analysis plan produce substantially similar estimates.

In comparison, a stronger set of predictions is $(\gamma_3 > 0)\&(\gamma_4 < 0)\&(\gamma_5 < 0)\&(\gamma_3 + \gamma_4 = 0)\&(\gamma_3 + \gamma_5 = 0)$, denoting the case where, under either entry or exit, a respondent becomes completely unresponsive to their treatment conditions in the second letter.

Tables A.40, A.41, A.42, and A.43 display regression estimates using specification A.21, where the outcome variables are $Amount_{it}$, $\log(Amount_{it} + 1)$, $I(Amount_{it} \geq 200)$, and $I(Amount_{it} > 0)$, respectively. In each table $Indiv\_R_i$ is binary in the first and second columns, and based on CFscores in the latter two columns. Moreover, the first and third columns examine the known PAC donors, while the second and fourth columns look at the remaining respondents. All four tables display robust standard errors.

Table A.40: Secondary-hypotheses Estimates for Dollar Amount of Donation

	$Amount_{i2}$			
$PAC\_R_{i2} \times Indiv\_R_i$	10.082 (386.660)	366.715 (341.218)	2.287 (493.912)	478.419 (446.423)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Entry_i)$	955.542 (529.341)	772.201* (318.777)	1,503.560* (728.785)	1,053.363* (412.183)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Exit_i)$	1,267.632 (919.679)	375.094 (638.198)	1,796.736 (1,299.014)	579.935 (868.255)
$Indiv\_R_i$	139.927 (80.703)	152.655* (76.632)	231.474* (112.867)	221.254* (106.246)
PAC_R_{i2}	90.485 (143.509)	118.420 (140.698)	95.901 (146.307)	119.595 (140.009)
Constant	378.022*** (49.015)	273.153*** (42.687)	384.533*** (48.931)	274.934*** (42.128)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	568	915	568	915
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.41: Secondary-hypotheses Estimates for Log of Dollar Amount of Donation

	$\log(Amount_{i2})$			
$PAC\_R_{i2} \times Indiv\_R_i$	0.131	2.383*	0.172	2.642*
	(1.388)	(0.961)	(1.967)	(1.337)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Entry_i)$	4.137	3.105*	6.306	4.482*
	(2.341)	(1.285)	(3.469)	(1.761)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Exit_i)$	4.501	1.512	6.561	2.186
	(2.576)	(1.857)	(3.733)	(2.574)
$Indiv\_R_i$	−0.141	−0.236	−0.121	−0.062
	(0.267)	(0.211)	(0.386)	(0.298)
PAC_R_{i2}	0.010	−0.011	0.029	−0.111
	(0.490)	(0.380)	(0.492)	(0.393)
Constant	2.681***	1.611***	2.687***	1.665***
	(0.160)	(0.121)	(0.160)	(0.122)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	568	915	568	915
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.42: Secondary-hypotheses Estimates for Indicator for Itemized Donation

	$I(Amount_{i2} \geq 200)$			
$PAC\_R_{i2} \times Indiv\_R_i$	−0.023	0.207	−0.067	0.173
	(0.211)	(0.128)	(0.299)	(0.175)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Entry_i)$	0.454	0.320	0.691	0.494*
	(0.356)	(0.175)	(0.535)	(0.236)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Exit_i)$	0.606	0.085	0.859	0.124
	(0.398)	(0.257)	(0.578)	(0.348)
$Indiv\_R_i$	−0.031	0.002	−0.031	0.047
	(0.040)	(0.028)	(0.058)	(0.039)
PAC_R_{i2}	0.014	0.007	0.014	−0.012
	(0.074)	(0.050)	(0.074)	(0.053)
Constant	0.310***	0.159***	0.311***	0.169***
	(0.024)	(0.016)	(0.024)	(0.017)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	568	915	568	915
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Table A.43: Secondary-hypotheses Estimates for Indicator for Positive Donation

	$I(Amount_{i2} > 0)$			
$PAC\_R_{i2} \times Indiv\_R_i$	0.104 (0.228)	0.436** (0.160)	0.152 (0.331)	0.508* (0.232)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Entry_i)$	0.724 (0.389)	0.544* (0.234)	1.088 (0.584)	0.773* (0.330)
$PAC\_R_{i2} \times Indiv\_R_i \times I(Exit_i)$	0.486 (0.413)	0.348 (0.319)	0.742 (0.607)	0.470 (0.452)
$Indiv\_R_i$	−0.048 (0.043)	−0.089** (0.034)	−0.063 (0.064)	−0.084 (0.049)
PAC_R_{i2}	0.001 (0.080)	−0.014 (0.062)	0.004 (0.080)	−0.029 (0.065)
Constant	0.476*** (0.026)	0.289*** (0.020)	0.475*** (0.026)	0.297*** (0.020)
Measure of $Indiv\_R_i$	binary	binary	CFscore-based	CFscore-based
Sample	known PAC donors	other donors	known PAC donors	other donors
Observations	568	915	568	915
Note:	*p<0.05; **p<0.01; ***p<0.001 Robust standard errors in parentheses			

Contrary to predictions based on my secondary hypothesis, when $Indiv\_R_i \times PAC\_R_{i2}$ is interacted with either entry or exit, the coefficients on these interaction terms are always positive, though statistically significant in only some of the cases. In words, when a respondent's first letter contained a large or small number of co-partisan candidates, the resultant high versus low willingness to donate did not persist. If anything, under either of these scenarios a respondent's willingness to donate generally became *more* sensitive to the number of co-partisan candidates listed in the second letter.

One way to rationalize of the results shown in Tables A.40, A.41, A.42, and A.43 is that receiving a hypothetical solicitation letter containing either a high or a low number of co-partisan candidates frames respondents' perception of that same number for the subsequent letter that they each receive. If a respondent's first letter has a high number of co-partisan candidates, she will not only react negatively to her second letter (which by design contains a lower number of co-partisan candidates), but she will react more strongly than someone who has the same partisan preference but received a different first letter. A symmetrical argument could be made about respondents whose first letters contain a low number of co-partisan candidates.

While it appears that the treatment effects induced by respondents' first letters were not persistent, this does not necessary contradict evidence for entries and exits in real campaign finance data. Long-term donor behavior is hard to measure in this survey experiment. And my definitions of exit and entry in the survey do not directly compare to those used in the paper.

A.3.9 Counterfactual fundraising outcomes

Here I demonstrate how one could calculate the counterfactual fundraising outcomes for itemized donations for a given access-seeking PAC j in cycle t corresponding to different hypothetical shares of PAC contributions to Republican politicians, denoted as $\widetilde{PAC\_R}_{jt}$.

First, I summarize, for PAC j in cycle t, the total amount of donations coming from each itemized donor i, or $Amount_{ijt}$. I omit unitemized donations because they are reported only in aggregate.

Second, for any itemized donors whose partisanship I cannot measure (i.e., because they have not made itemized donations to any Republican/Democratic candidates or party committees), I impute these donors' partisan preferences by randomly drawing, with replacements, the set of partisan preference measures $Indiv\_R_i$ for all donors who have both donated to major-party candidates or party committees at some point and gave itemized donations to PAC j in cycle t, under the assumption that unobserved partisan preferences of active donors follow the same distribution as the observed ones within the same cycle.

Third, for each itemized donor i in PAC j in cycle t, I calculate how much she would have given to the PAC, call it $\widetilde{Amount}_{ijt}$, under a given hypothetical $\widetilde{PAC\_R}_{jt}$ using the following formula: $\widetilde{Amount}_{ijt} = (Amount_{ijt} + 1) \times \exp(3.096 \times Indiv\_R_i \times (\widetilde{PAC\_R}_{jt} - PAC\_R_{jt})) - 1$. Note that 3.096 is the coefficient estimate derived from column 1 of Table

6 in the paper.

I choose this particular estimate for a couple of reasons. Relative to results obtained from my analysis of campaign finance records, the survey estimates not only avoid the problem of attenuation bias but also allow for computations of counterfactual amounts of donations (rather than just the binary act of giving). Because donors in the survey tend to under- rather than over-state the amounts of donations they are willing to give relative to their actual donation behavior, calculating counterfactuals using the survey estimates is unlikely to exaggerate the impact of donors' partisan preferences on PAC fundraising. In addition, I choose the log-linear model out of all models estimated on the survey experimental data because this model provides a better fit for dollar amounts of itemized donations, which are right-skewed in both the survey responses and in patterns of real giving.

Fourth, if the counterfactual amount of donation calculated for any donor exceeds \$10,000, I trim it to \$10,000, which is the legal maximum for how much an eligible donor could give to an access-seeking PAC across two years within a federal election cycle.

Finally, I sum up the counterfactual amounts $\widetilde{Amount}_{ijt}$ across all donors i in cycle t.

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