Appendix: The Distributive Politics of Grants-in-Aid

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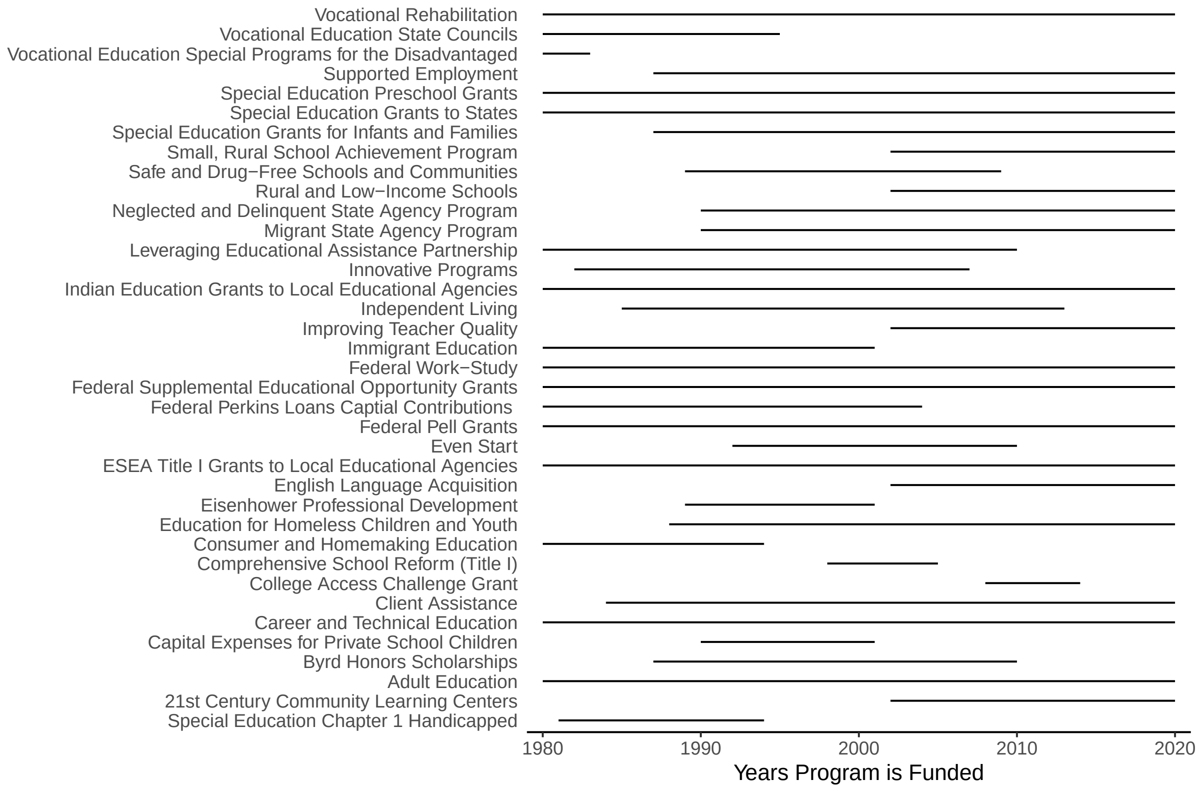
A Data

Figure A1 summarizes which programs are funded in each year in this dataset. The lines on the figure denote what years the program was funded. Programs are only included in the analysis when they receive funding.

Figure A1: Formula Grants Administered by ED, FY1980 to FY2020



Not all of the program reauthorizations follow the same policymaking process. Table A1 shows how common conference committees and omnibus legislation are in the data. In just over a fifth of cases, Congress did not use a formal conference committee to resolve House and Senate differences. And, in two cases, the reauthorization was packaged in an omnibus bill.

Table A1: Unorthodox Lawmaking

	Conference		Omnibus	
	Yes	No	Yes	No
Count of Program Reauthorizations	103	29	2	130

B Estimating the Committee Advantage

To estimate the committee advantage I use a difference-in-differences design where each treated observation is matched with control observations from the same state in the same time period. Let $D_{ipt} \in \{0, 1\}$ represent the treatment status (committee member/committee chair/similar to committee chair) of state i for program p at time t. I estimate the committee advantage j years after a reauthorization for $j \in \{0, 1, 2, 3\}$ using

$$\hat{\tau}_j = \frac{\sum_{i \in S} \sum_{t \in T} \sum_{p \in P} W_{ipt} \left(Y_{ipt+j} - Y_{ipt-1}\right)}{\sum_{i \in S} \sum_{t \in T} \sum_{p \in P} D_{ipt} \times W_{ipt}} \tag{1}$$

where Y_{ipt+j} is state i's grant amount under program p at time $t + j$; and

$$W_{ipt} = \begin{cases} \frac{-\sum_{p' \in P} \prod_{j'=1}^{3}(1 - D_{ip't-j'}) \prod_{j'=0}^{3} D_{ip't+j'}}{\sum_{p' \in P} \prod_{j'=-3}^{3}(1 - D_{ip't+j'})} & \text{if } D_{ipt+j'} = D_{ipt-j'} = 0 \ \forall \ j' \in \{0, 1, 2, 3\} \\ 1 & \text{if } \prod_{j'=0}^{3} D_{ipt+j'} = \prod_{j'=1}^{3}(1 - D_{ipt-j'}) = 1; \\ & \text{ and } \sum_{p' \in P} \prod_{j'=-3}^{3}(1 - D_{ip't+j'}) > 0 \\ 0 & \text{Otherwise} \end{cases}$$

Note that τ is the average treatment effect on the treated (ATT). The denominator reflects the number of treated observations that have at least one control observation in their matched sets. The numerator is equivalent to taking the change in a state's grant amount for treated observations that have a matched set and subtracting it from the average change in that state's grant amounts over the same time period for programs that have yet to be reauthorized. To achieve this, treated observations with a matched control set receive a weight (W_{ipt}) of 1 and control observations receive a weight based on the number of treated observations they are matched to and the number of other control observations in the matched set. To estimate standard errors, I use the weighted bootstrap procedure proposed by Otsu and Rai (2017). Specifically, I treat the weights as covariates and do not re-estimate them within each bootstrap iteration. Following Imai, Kim, and Wang (2020), I use a block bootstrap procedure to sample state-program units to accommodate the panel nature of my data.

The assumption required for identification is that, absent program reauthorization, both treated and control units would have continued along the same pre-treatment trajectories. Provided this assumption is satisfied, I can compare differences in the means of state grant amounts before and after reauthorization among treated and control units, and this estimate represents the effect solely attributable to committee membership or similarity to the committee chair. To test this assumption, I estimate the impact of committee membership prior to reauthorization. Table A2 presents the results of this analysis. I do not find a significant effect of being represented by a committee member or the committee chair. This suggests that treated and control units are similar prior to program reauthorization. This provides further support for the parallel trends assumption.

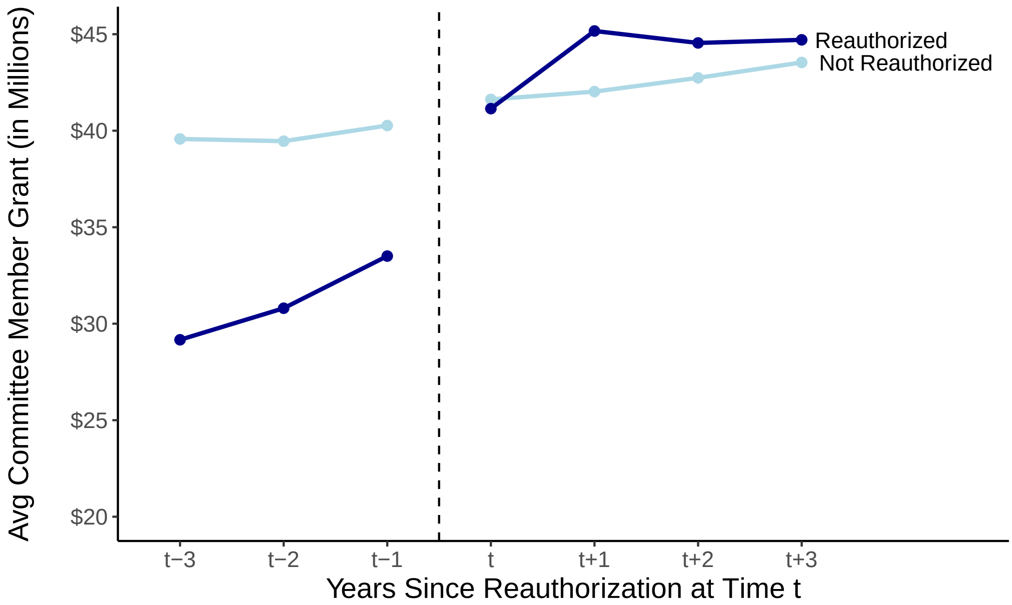
Table A2: Committee Pre-Trends, Diff-in-Diff Estimates

	DV: Grant Amount (Log)		
	$t-3$	$t-2$	N
Committee Chair	0.049	0.049	145
	(0.041)	(0.058)	
Committee Member	-0.008	-0.008	1,179
	(0.042)	(0.04)	

Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Count of observations refers to unique number of state-program reauthorizations in each analysis.

The assumption required for identification is that, absent program reauthorization, both treated and control units would have continued along the same pre-treatment trajectories. Provided this assumption is satisfied, I can compare differences in the means of state grant amounts before and after reauthorization among treated and control units, and this estimate represents the effect solely attributable to committee membership or similarity to the committee chair. To test this assumption, Figure A2 examines the pre-reauthorization trends in state grant amounts for committee members. The trends for the reauthorized and not reauthorized grants are similar. This suggests that the parallel trends assumption may be reasonable in this case.

Figure A2: Committee Members' Grants by Reauthorization Status

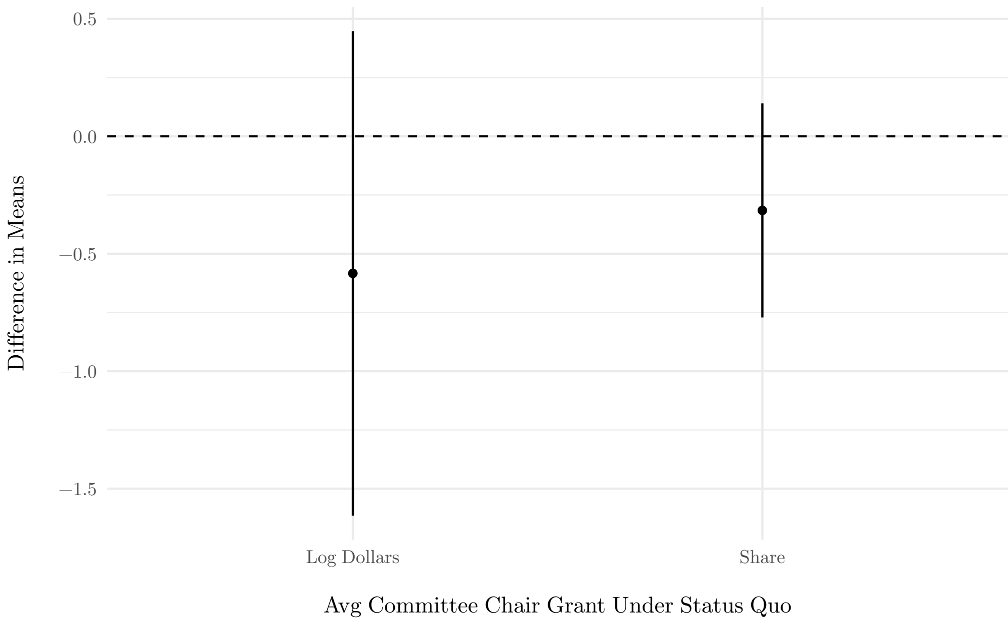


Notes: Averages are weighted so that each treated unit is matched to its control set.

One potential concern with this design is that committee chairs may be strategically selecting which programs to reauthorize. For example, if committee chairs choose to reauthorize programs where their states are doing poorly (and thus have the most room for improvement) then this analysis will overestimate the committee chair benefit. However, a single statute contains multiple formula grant programs as well as other policies. Thus, whether a program gets reauthorized depends on more than just its allocation formula. To empirically test whether committee chairs strategically select programs to reauthorize, I compare chairs' grants under the status quo under programs reauthorized and not reauthorized in a given year. Figure A3 shows the results of this analysis.[1] I find no significant difference between the treatment and control groups. This suggests that chairs are not selecting bills to reauthorize based on how much grant funding their states are receiving under programs included in each bill.

[1]The differences in means were weighted so that each treated unit is matched to its control set.

Figure A3: Chair's Status Quo Grant Balance Between Treatment and Control



Notes: Differences in means are weighted so that each treated unit is matched to its control set. Grant share is measured on a scale from 0 to 100.

C Placebo Test

It is possible that states represented by committee members and the committee chair see an increase in their grant amounts because all states see an increase in their grant amounts following a reauthorization. To account for this, I rerun the analysis for non-committee members and present the results in Table A3. I do not find a significant increase in these states' grant amounts following program reauthorizations.

Table A3: Effect of Committee Position on Formula Grants Placebo Test

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	$t+3$
Not On Committee	-0.057	-0.036	-0.081	-0.04
	(0.042)	(0.031)	(0.047)	(0.039)
Observations	712	712	712	712

Note: *p<0.05; **p<0.01; ***p<0.001; standard errors computed based on 1,000 weighted bootstrap samples in parentheses

D Role of Parties

Are there differences for Democratic versus Republican committee members? Table A4 presents the committee advantage broken out by party. I find that states represented by both Democratic and Republican committee members disproportionately benefit from grants-in-aid.

Table A4: Effect of Committee Position on Formula Grants, Diff-in-Diff Estimates

	DV: Grant Amount (Log)				
	t	$t+1$	$t+2$	$t+3$	N
Dem. Committee Member	0.063***	0.096**	0.126**	0.454	376
	(0.015)	(0.03)	(0.048)	(0.233)	
Rep. Committee Member	0.172**	0.128**	0.208**	0.262***	812
	(0.056)	(0.049)	(0.063)	(0.07)	

Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Count of observations refers to unique number of state-program reauthorizations in each analysis.

E Bicameral Representation

Table A5 examines the role of bicameral committee membership in two ways. The first row compares states represented by both House and Senate committee members to states with no committee representation. The second row compares states with just Senate committee representation to states with no committee representation. I do not find evidence of an additional benefit to bicameral committee representation.

Table A5: Effect of Bicameral Committee Representation on Formula Grants, Diff-in-Diff Estimates

	DV: Grant Amount (Log)				
	t	$t+1$	$t+2$	$t+3$	N
Bicamerial Committee Representation	0.058	0.119*	0.151**	0.1	108
	(0.029)	(0.049)	(0.055)	(0.1)	
Only Senate Committee Member	0.287*	0.133	0.251**	0.289*	319
	(0.131)	(0.098)	(0.096)	(0.129)	

Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Count of observations refers to unique number of state-program reauthorizations in each analysis.

F Role of Conference Committees

Of the program reauthorizations in my data, 103 had a formal conference and 29 did not. Table A6 re-estimates the Senate committee benefit for reauthorizations without a formal conference. I find that committee members' states still benefit from these bills.

Table A6: Effect of Committee Position on Formula Grants in Reauthorizations without a Formal Conference, Diff-in-Diff Estimates

	DV: Grant Amount (Log)				
	t	$t+1$	$t+2$	$t+3$	N
Senate Committee Member	0.07*	0.086**	0.146***	0.247***	714
	(0.03)	(0.03)	(0.042)	(0.062)	

Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Unit of analysis is state program. Units are matched based on state and year.

G State Similarity

Table A7 re-estimates the spillover analysis, but breaks states into five groups instead of six groups. The results do not change substantially when I use five groups. I do find that Senate partisan similarity is significant at times t and $t+1$, although the effects are substantially smaller than demographic similarity.

Table A7: Effect of Committee Chair Similarity on Grants, Diff-in-Diff Estimates

	DV: Grant Amount (Log)				
	t	$t+1$	$t+2$	$t+3$	N
Senate Demographic Similarity	0.064**	0.158***	0.245***	0.178***	208
	(0.022)	(0.031)	(0.057)	(0.044)	
Senate Partisan Similarity	0.03*	0.041*	0.024	0.031	198
	(0.012)	(0.017)	(0.02)	(0.024)	
House Demographic Similarity	-0.005	-0.023	-0.037	0.014	107
	(0.01)	(0.021)	(0.033)	(0.026)	
House Partisan Similarity	-0.22	-0.083	-0.177	-0.169	64
	(0.161)	(0.11)	(0.145)	(0.143)	

Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Unit of analysis is state program.

Are there heterogeneous spillover effects based on level of similarity to the chair? In Table A8, I look at spillovers to states that are one group away from the chair's state ("Moderately Similar to Chair"). Table A8 also re-estimates the spillover effect for states in the chair's group ("Very Similar to Chair"), but excludes states that are one group away from the control group. I find that states that are one group away from the chair also see spillover effects that are similar to states in the same group as the chair. This suggests that there may be a threshold above which there are spillovers and below which there are not spillovers.

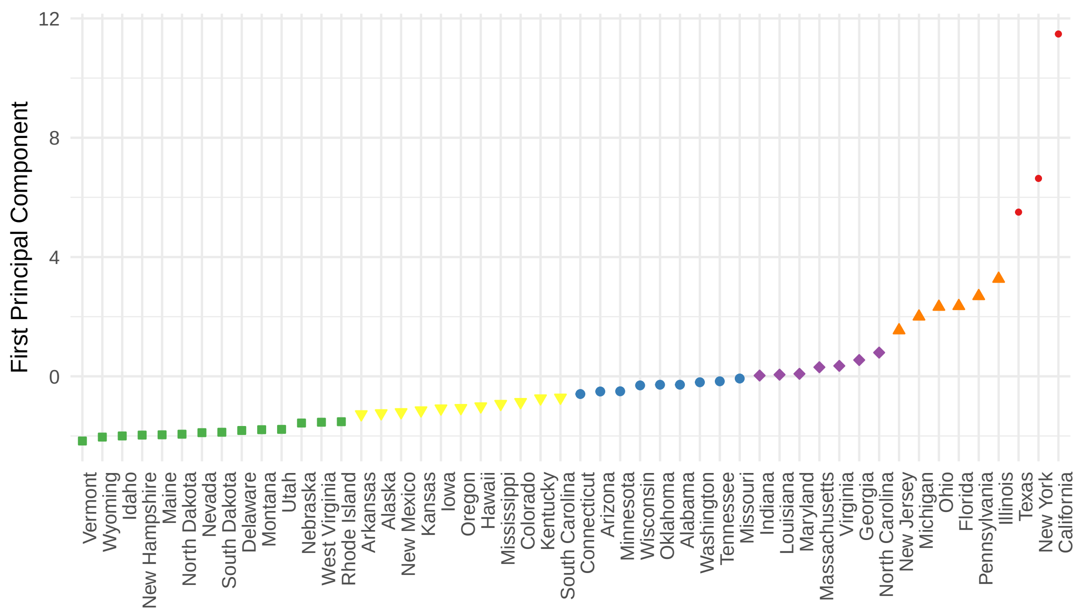
Table A8: Effect of Senate Committee Chair Similarity on Grants, Diff-in-Diff Estimates

	DV: Grant Amount (Log)				
	t	$t+1$	$t+2$	$t+3$	N
Very Similar to Chair	0.096***	0.157***	0.335***	0.234***	139
	(0.026)	(0.037)	(0.09)	(0.054)	
Moderately Similar to Chair	0.164**	0.205*	0.193**	0.181**	48
	(0.057)	(0.077)	(0.072)	(0.063)	

Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Unit of analysis is state program. Units are matched based on state and year. Analyses exclude committee members.

How much does state similarity vary over time? Figure A4 shows the similarity scores for 1980. Compared to 2020 (see Figure 3 in main text), there is some change. In both years, California has the highest score and Vermont has the lowest score. However, there are some changes in the ordering of states and thus the similarity groups. For example, in 1980, New York is in a group with California, but, in 2020, New York is in a group with Florida.

Figure A4: State Similarity, 1980



References

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Otsu, Taisuke, and Yoshiyasu Rai. 2017. “Bootstrap Inference of Matching Estimators for Average Treatment Effects.” *Journal of the American Statistical Association* 112, no. 520 (2, 2017): 1720–1732.