Appendix: Unorthodox Lawmaking and the Value of Committee Assignments

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1 Data

Figure A1 shows the formula grant programs administered by the Department of Education included in our analyses. This encompasses the universe of formula grant programs administered by the U.S. Department of Education from 1980 to 2020. The figure also show when each program was reauthorized and whether the reauthorization bypassed committee or did not have a formal conference ("unorthodox lawmaking").

Figure A1: Department of Education Formula Grant Program Reauthorizations

We also include affordable housing and energy programs in our analyses. These programs and their reauthorizations are listed in Figure A2. Note that the Housing Opportunities for Persons with AIDS and HOME Investment Partnerships Program are reauthorized by the same bills.

Figure A2: Housing and Energy Formula Grant Program Reauthorizations

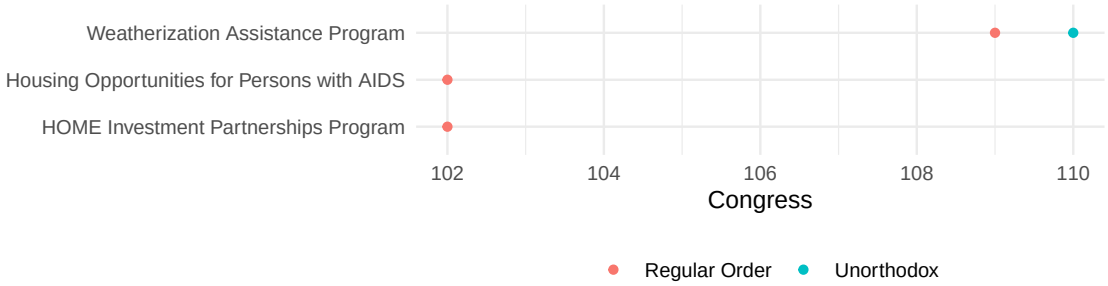


Figure A3 shows the formula grant programs administered by the Department of Agriculture included in the analysis. The figure also shows the reauthorizations for each program.

Figure A3: Department of Agriculture Formula Grant Program Reauthorizations

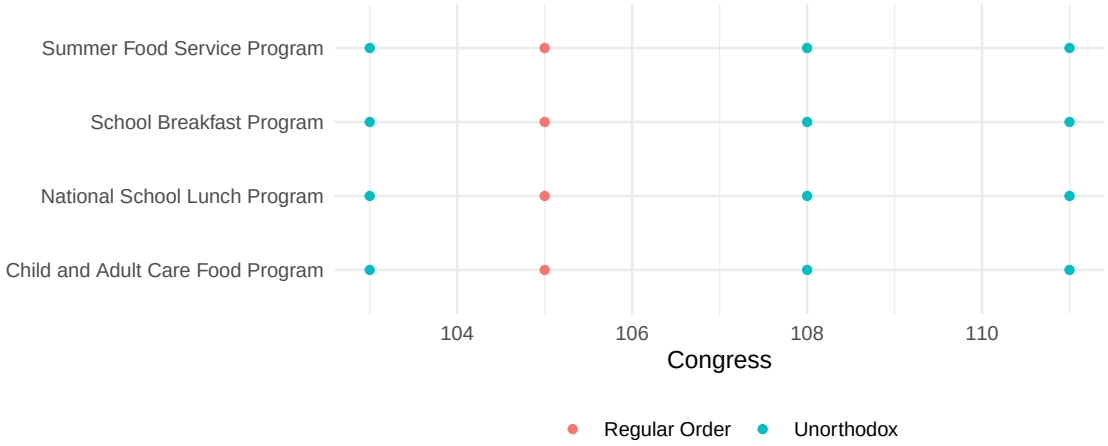


Figure A4 shows the formula grant programs administered by the Department of Health and Human Services included in the analysis. The figure also shows the reauthorizations for each program.

Figure A4: Department of Health and Human Services Formula Grant Program Reauthorizations

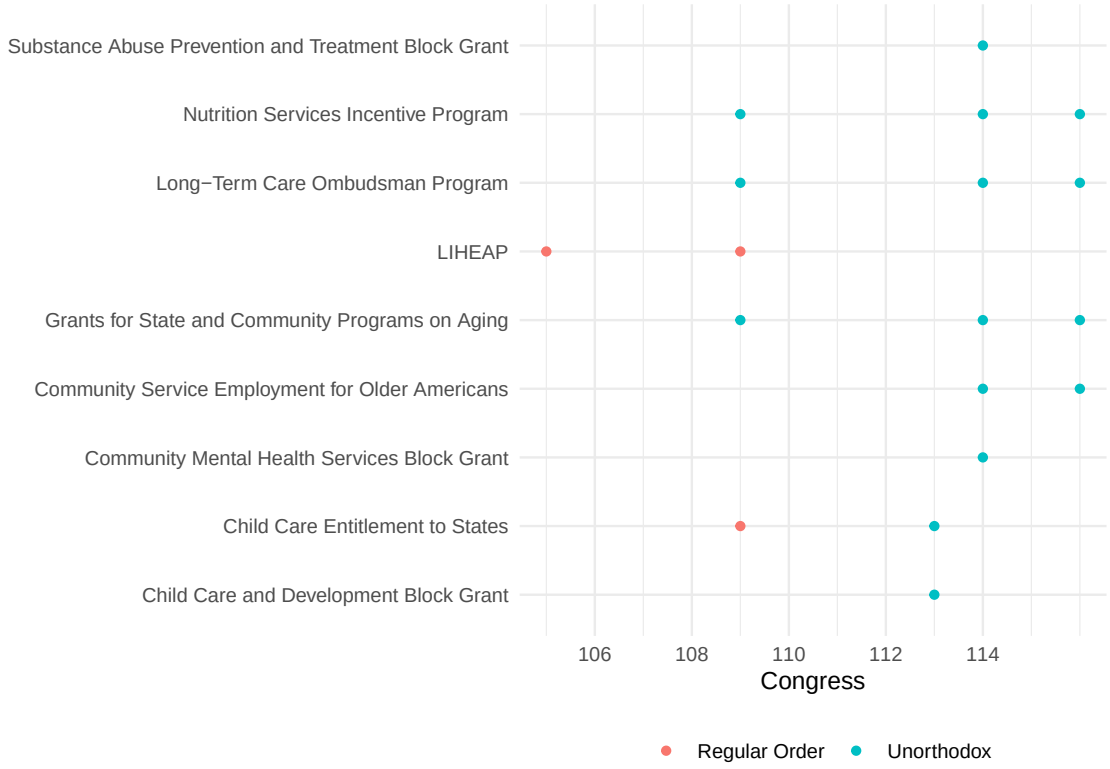
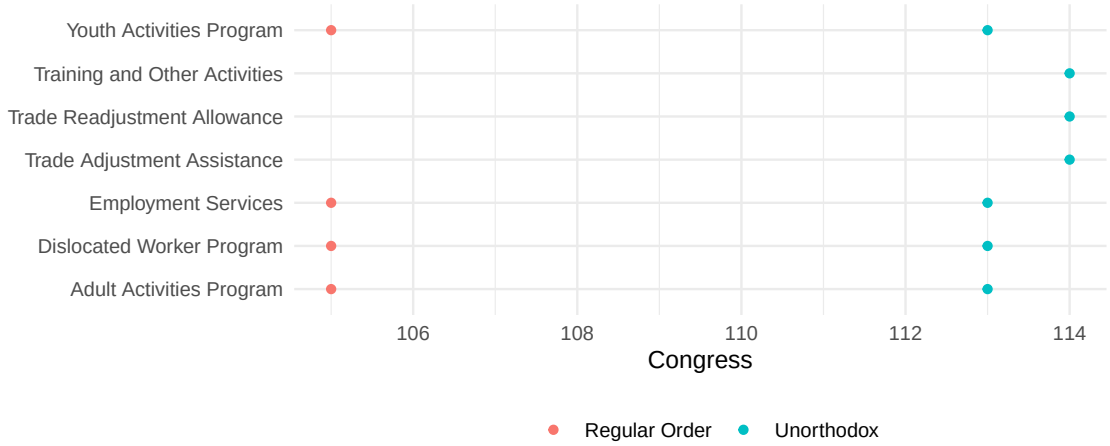


Figure A5 shows the Department of Transportation programs included in the analysis as well as the reauthorizations for each program. Note that all but the Airport Improvement Program are reauthorized by the same bills.

Figure A5: Department of Transportation Formula Grant Program Reauthorizations

Figure A6 lists the Department of Labor formula grant programs included in the analyses. The figure also shows the reauthorizations for each program. Note that all of the trade programs—Training and Other Activities, Trade Readjustment Allowance, and Trade Adjustment Assistance—are reauthorized by the same bills.

Figure A6: Department of Labor Formula Grant Program Reauthorizations



2 Methodology

We 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) of state i for program p at time t. We estimate the value of committee seats j years after a reauthorization for $j \in \{0, 1, 2\}$ using

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

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

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

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 account for the fact that the same observation may be used in the control group for multiple observations in the treatment group (matching with replacement), we estimate standard errors using a weighted bootstrap (Otsu and Rai 2017). Specifically, we treat the weights as covariates and do not re-estimate them within each bootstrap iteration. Following Imai, Kim and Wang (2019), we use a block bootstrap procedure to sample state-program units to accommodate the panel nature of my data.

Tables A1 and A2 show the number of treated and control units (state-program-reauthorizations) in each analysis.

Table A1: Count of Treated and Control Units, Table 1 Analyses

	Treated	Control	Total
All Legislation	743	1,253	1,996
Unorthodox Only	214	550	764
Regular Order Only	520	998	1,518

Table A2: Count of Treated and Control Units, Table 2 Analyses

	Treated	Control	Total
No Conference	130	284	414
Not Reported by Committee	179	141	320
Combined Measure	122	326	448

3 Assumptions and Robustness Checks

The assumption required for identification is that, absent the reauthorization of the program, treated and control units would have followed the same trends. To examine this assumption, Figure A7 compares the average state grant amounts for states represented by committee members

(treated) and not represented by committee members (control). Note that averages are weighted so each treated unit is matched to its control set. The trends for both groups are similar, suggesting the parallel trends assumption may be reasonable in this case.

Figure A7: State Grants, Committee Members Versus Non-Committee Members

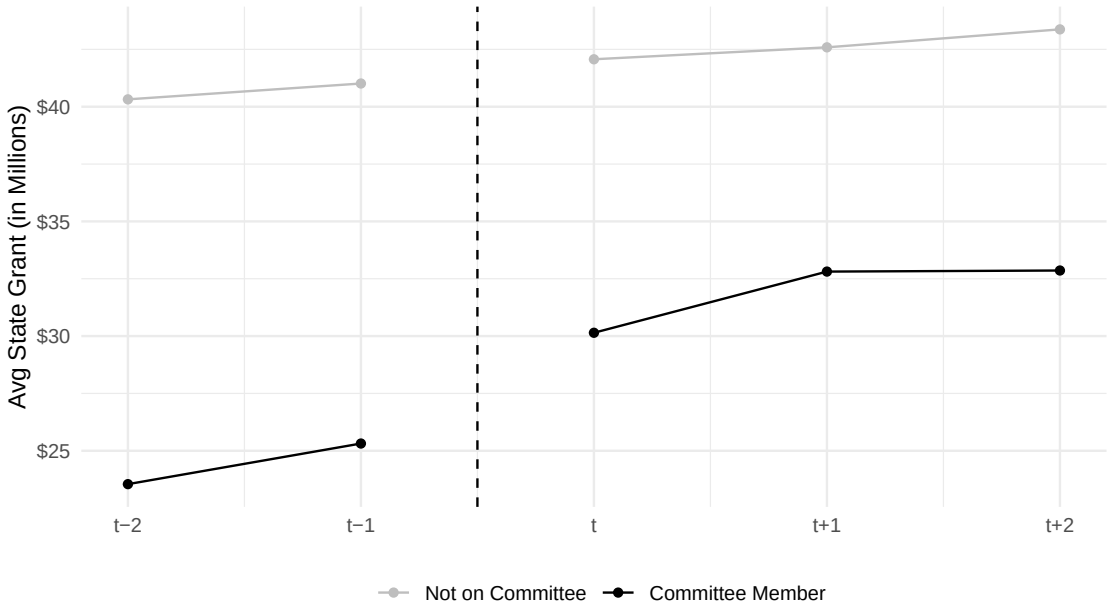


Table A3 examines whether there is any difference between treated units and control units prior to treatment by examining the difference-in-differences at $t-2$. We find no significant difference between these two groups.

An underlying assumption in our first set of analyses is that members of Congress are not strategically choosing to reauthorize certain programs based on their grant amounts under those programs. In other words, a program's formula is unrelated to whether it gets reauthorized. We believe this assumption is reasonable because these formulas live in large bills containing multiple programs and provisions unrelated to the formulas. Thus, for example, the decision to reauthorize an education program might be related to standardized testing requirements as opposed to the formula. Additionally, if members were strategically choosing which programs to authorize based on the status quo formulas then we would see significant differences in the pre-reauthorization trends between our treatment and control groups. As Table A3 and Figure A7 show, this is not the case. Moreover, our second set of analyses compares programs that have been reauthorized in the

Table A3: Impact of Committee Membership on Grant Funding Pre-Trends, Diff-in-Diff Estimates

	DV: Grant Amount (Log)	
	$t-2$	N
All Legislation	-0.042	1,996
	(0.035)	
Unorthodox Only	-0.008	764
	(0.016)	
Regular Order Only	-0.06	1,518
	(0.051)	

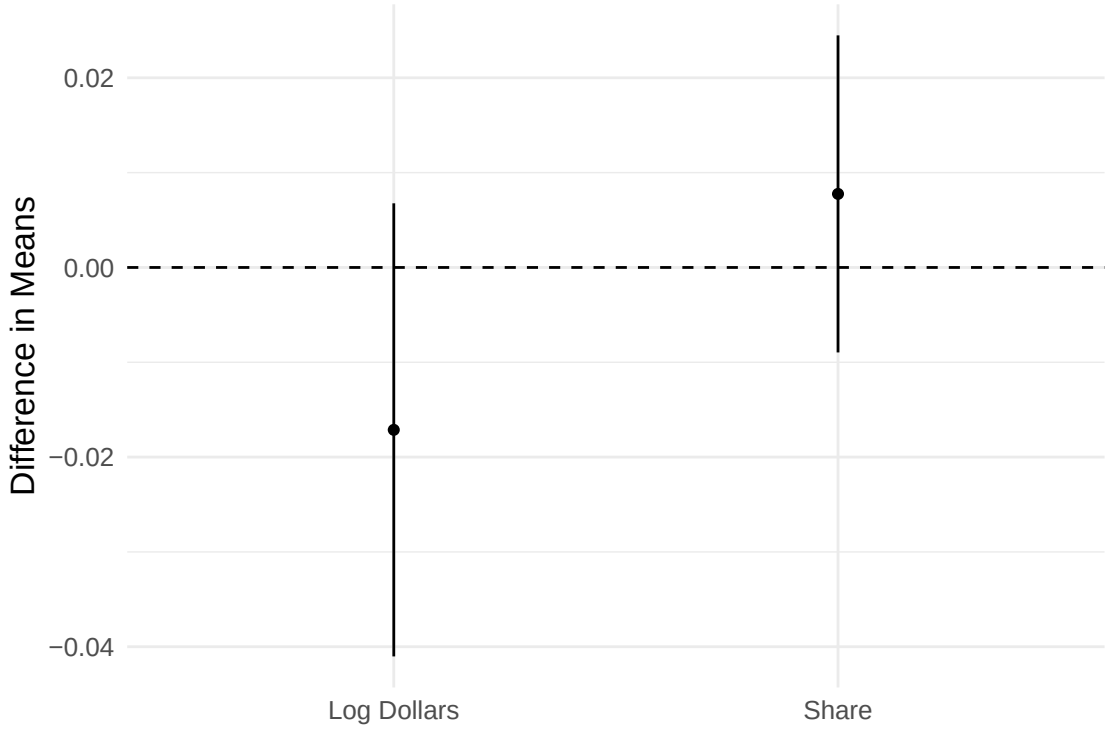
Note: *p<0.05; **p<0.01; ***p<0.001. Standard errors computed based on 1,000 weighted bootstrap samples in parentheses. Count of observations refers to unique number of state-program reauthorization dyads in each analysis. Each cell represents a different regression.

same congress. Thus, grant sizes getting bigger over time or different time tables for reauthorization would not explain the results in Table 2.

Another concern is that party leaders may use unorthodox procedures where they expect committees would otherwise make the biggest gains. Relatedly, changes made via an unorthodox process might be smaller than changes made through a regular order process. Figure A8 presents the difference in means between unorthodox lawmaking and regular order.[1] We do not find significant differences in the size of formula changes. Moreover, leaders strategically using unorthodox procedures where the committee would otherwise make big gains does not explain the results in Table 1.

[1]In this analysis we only look at the in the change in grant amount the first year after a program is reauthorized. Unlike the analyses included in the main body of the paper, there is no matching design. Additionally, we exclude states not receiving any grant funding under a program as this may cause us to underestimate the average size of the change.

Figure A8: Average Change in Grant Amount, Unorthodox Lawmaking vs Regular Order



Has the impact of unorthodox lawmaking changed over time? To examine this question, we re-reestimate the analysis in Table 2 for each congress. These results are displayed in Table A4. In general, we find similar results over time. However, not that some congresses have very few observations. We are therefore hesitant to over-interpret these analyses.

Table A4: Unorthodox Lawmaking and the Value of Committee Assignments Over Time, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
114th Congress	-0.105	-0.109	-0.112	160
	(0.082)	(0.087)	(0.078)	
109th Congress	0.028	0.069***	0.121***	22
	(0.015)	(0.012)	(0.016)	
105th Congress	0.019	0.084	-0.036	124
	(0.012)	(0.091)	(0.044)	
99th Congress	0.827	0.43*	0.877	44
	(0.505)	(0.212)	(0.543)	
98th Congress	-0.034	-0.019	-0.037	57
	(0.022)	(0.038)	(0.047)	

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 reauthorization dyads in each analysis. Each cell represents a different regression. Unorthodox lawmaking is defined as no conference or no Senate committee report.

Table A5 re-estimates the analysis in Table 1 but defines unorthodox lawmaking as either no committee report, no conference, or a post-committee adjustment in the Senate. Table A6 re-estimates the analyses in Table 2, but defines unorthodox lawmaking as no Senate committee report, no conference, or a Senate post-committee adjustment.

Table A5: Impact of Committee Membership on Grant Funding, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
Unorthodox Only	0.035	0.047	0.049	1,091
	(0.029)	(0.032)	(0.034)	
Regular Order Only	0.091	0.061	0.227***	1,148
	(0.05)	(0.074)	(0.068)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Table A6: Unorthodox Lawmaking and the Value of Committee Assignments, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
Senate Post-Committee Adjustment	-0.104*	0.062	0.004	586
	(0.051)	(0.042)	(0.044)	
Combined Measure	-0.047	0.06	-0.011	356
	(0.039)	(0.057)	(0.045)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Do the results vary for different committee members? Tables A7 and A8 show the committee chair advantage and the difference in the chair's benefit between unorthodox lawmaking and a regular order process.[2] Tables A9 and A10 re-estimate the analyses for only majority party committee members. Tables A11 and A12 re-estimate the analyses for only minority party committee members. These analyses suggest that there is no significant different between the two different types of lawmaking when looking at subsets of committee members.

[2]Table A7 compares a state when it is represented by the chair to when it is not represented by the chair.

Table A7: Impact of Serving as Committee Chair on Grant Funding, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
All Legislation	0.204	0.102*	0.101**	272
	(0.132)	(0.044)	(0.038)	
Unorthodox Only	0.085*	0.141	0.097***	139
	(0.033)	(0.09)	(0.028)	
Regular Order Only	0.254	0.086*	0.103*	198
	(0.178)	(0.035)	(0.048)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Table A8: Unorthodox Lawmaking and the Value of Committee Chairs, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
No Conference	-0.102	-0.108	-0.125	47
	(0.103)	(0.107)	(0.112)	
Not Reported by Committee	0.353	0.544	0.596	47
	(0.426)	(0.405)	(0.413)	
Combined Measure	-0.021	0.00	-0.014	50
	(0.027)	(0.039)	(0.046)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Table A9: Impact of Majority Party Committee Membership on Grant Funding, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
All Legislation	0.104***	0.118***	0.173***	492
	(0.03)	(0.034)	(0.037)	
Unorthodox Only	0.041***	0.078***	0.09***	132
	(0.006)	(0.008)	(0.01)	
Regular Order Only	0.127**	0.128**	0.198***	374
	(0.041)	(0.047)	(0.052)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Table A10: Unorthodox Lawmaking and the Value of Majority Party Committee Seats, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
No Conference	0.03	-0.022	-0.03	260
	(0.049)	(0.039)	(0.037)	
Not Reported by Committee	-0.284*	-0.038	0.035	239
	(0.14)	(0.056)	(0.024)	
Combined Measure	-0.044	-0.026	-0.032	309
	(0.037)	(0.04)	(0.039)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Table A11: Impact of Minority Party Committee Membership on Grant Funding, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
All Legislation	0.079***	0.117***	0.148***	251
	(0.012)	(0.014)	(0.023)	
Unorthodox Only	0.045**	0.074***	0.057	82
	(0.017)	(0.012)	(0.039)	
Regular Order Only	0.093***	0.133***	0.187***	184
	(0.016)	(0.02)	(0.029)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

Table A12: Unorthodox Lawmaking and the Value of Minority Party Committee Seats, Diff-in-Diff Estimates

	DV: Grant Amount (Log)			
	t	$t+1$	$t+2$	N
No Conference	0.024	-0.012	0.005	124
	(0.032)	(0.053)	(0.061)	
Not Reported by Committee	0.095	0.204	0.191	77
	(0.064)	(0.133)	(0.143)	
Combined Measure	0.038	0.05*	0.077	105
	(0.032)	(0.023)	(0.039)	

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 reauthorization dyads in each analysis. Each cell represents a different regression.

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(520):1720–1732.