Online Appendix for

Congress and Community

Coresidence and Social Influence in the U.S. House of Representatives, 1801–1861

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Contents

Descriptive Statistics

Table A1: Descriptive Statistics (All Dyads)

Variable	Mean	SD	Min	Max	% Missing
Coresidence	0.03	0.16	0	1	7.6
Lagged Coresidence	0.03	0.16	0	1	46.8
Agreement	55	22	0	100	0.4
Same Party	0.49	0.50	0	1	0.0
Same State	0.07	0.25	0	1	0.0
Same Region	0.57	0.49	0	1	0.0
Same Occupation	0.45	0.50	0	1	0.0
Both New Members	0.10	0.30	0	1	0.0
Both in Previous Session	0.59	0.49	0	1	0.0
Neither in Previous Session	0.14	0.35	0	1	0.0
Both College	0.22	0.41	0	1	0.0
Neither College	0.29	0.45	0	1	0.0
Both Military	0.08	0.27	0	1	0.0
Neither Military	0.53	0.50	0	1	0.0
Age Difference	−0.0	1.0	−1	6	0.0
Seniority Difference	0.0	1.0	−1	9	0.0
Lagged Agreement	−0.0	1.0	−2	2	40.1
Lagged Coabsence	0.0	1.0	−1	14	39.9

n dyads = 1322497

Logistic Regressions of *Coresidence* with Dyadic-Robust Standard Errors

Table A2: Logistic Regression Models of ***Coresidence*** with Dyadic-Robust Standard Errors

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged Coresidence	3.00***	3.17***	3.48***	3.12***	2.79***
	(0.21)	(0.17)	(0.15)	(0.12)	(0.14)
Lagged Agreement	0.33***	0.11*	0.35***	0.28***	0.22***
	(0.09)	(0.05)	(0.06)	(0.06)	(0.06)
Lagged Coabsence	−0.02	0.04	−0.01	0.03	0.06
	(0.03)	(0.04)	(0.03)	(0.02)	(0.04)
Same Party	1.89***	0.52***	0.76***	1.36***	0.26***
	(0.22)	(0.09)	(0.09)	(0.11)	(0.08)
Same State	0.78***	1.04***	1.11***	1.10***	0.79***
	(0.08)	(0.09)	(0.07)	(0.06)	(0.09)
Same Region	0.53***	0.27**	0.33***	0.55***	0.48***
	(0.10)	(0.10)	(0.09)	(0.08)	(0.11)
Same Occupation	0.13*	0.05	0.05	0.02	0.02
	(0.06)	(0.06)	(0.06)	(0.04)	(0.08)
Age Difference	−0.04	−0.07	−0.09***	−0.03	−0.06
	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Seniority Difference	−0.00	−0.10	0.03	0.02	−0.14*
	(0.03)	(0.06)	(0.04)	(0.03)	(0.06)
Both First Session	0.05	0.12	0.36*	0.22*	0.09
	(0.12)	(0.14)	(0.16)	(0.09)	(0.15)
Both in Previous Session	−0.14	−0.06	−0.16	−0.05	−0.08
	(0.11)	(0.15)	(0.12)	(0.10)	(0.16)
Neither in Previous Session	0.30*	−0.06	−0.16	0.10	0.00
	(0.13)	(0.16)	(0.15)	(0.09)	(0.15)
Both College	0.16	0.14*	0.05	0.02	0.09
	(0.10)	(0.07)	(0.06)	(0.06)	(0.07)
Neither College	0.10	−0.04	0.30***	0.16**	−0.04
	(0.07)	(0.06)	(0.08)	(0.05)	(0.08)
Both Military	−0.02	0.15	−0.12	−0.03	0.40**
	(0.11)	(0.09)	(0.07)	(0.07)	(0.13)
Neither Military	0.09	0.03	0.00	−0.01	0.06
	(0.07)	(0.07)	(0.04)	(0.05)	(0.08)
n Dyads	111000	134522	274565	384410	317086
n Legislators	521	485	728	898	890
n Sessions	11	8	12	15	12

The table presents the results of logistic regression models for each era. Standard errors are cluster-robust for dyadic data (Aronow, Samii and Assenova 2015). Models also include indicators for each congressional session and for availability of covariates.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table A3: Logit Models of Repeated ***Coresidence*** with Dyadic-Robust Standard Errors

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged Agreement	0.30	0.33^{***}	0.35^{**}	0.52^{***}	0.19
	(0.20)	(0.09)	(0.12)	(0.13)	(0.14)
Lagged Coabsence	-0.08	0.10	-0.28^{***}	-0.07	-0.10
	(0.07)	(0.11)	(0.07)	(0.05)	(0.08)
Same Party	0.52^{*}	0.15	-0.13	0.62^{***}	0.15
	(0.26)	(0.20)	(0.17)	(0.18)	(0.21)
Same State	-0.03	0.14	0.14	0.09	0.03
	(0.08)	(0.10)	(0.08)	(0.06)	(0.10)
Same Region	0.36^{*}	0.19	0.49^{***}	0.24^{*}	0.40^{**}
	(0.15)	(0.15)	(0.14)	(0.12)	(0.15)
Same Occupation	0.17	0.34	-0.03	-0.14	-0.16
	(0.57)	(0.18)	(0.19)	(0.26)	(0.24)
Age Difference	0.22^{***}	0.02	-0.04	0.01	-0.06
	(0.06)	(0.06)	(0.07)	(0.07)	(0.08)
Seniority Difference	-0.15	-0.18	-0.09	-0.31^{**}	-0.00
	(0.18)	(0.15)	(0.13)	(0.10)	(0.15)
Both College	0.10	-0.03	-0.21	-0.07	-0.07
	(0.22)	(0.18)	(0.14)	(0.12)	(0.16)
Neither College	-0.08	0.15	0.38^{*}	0.13	0.09
	(0.20)	(0.15)	(0.16)	(0.11)	(0.13)
Both Military	0.06	0.40^{*}	-0.14	-0.46^{**}	0.23
	(0.20)	(0.19)	(0.15)	(0.14)	(0.27)
Neither Military	-0.09	-0.06	-0.02	0.10	0.05
	(0.21)	(0.16)	(0.12)	(0.12)	(0.16)
n Dyads	1881	2543	3857	5175	4706
n Legislators	341	422	646	791	675
n Sessions	7	8	12	15	11

The table presents the results of logistic regression models for each era, on the subsamples of dyads who were coresidents in the previous time period. Sessions are excluded when no evidence exists for the relevant prior session. Standard errors are cluster-robust for dyadic data (Aronow, Samii and Assenova 2015). Models also include indicators for each congressional session and for availability of covariates.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Linear Probability Models of *Coresidence* with Legislator Fixed Effects

Table A4: Linear Probability Models of ***Coresidence*** with Legislator Fixed Effects

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged Coresidence	0.3780^{***}	0.3429^{***}	0.3743^{***}	0.3497^{***}	0.2868^{***}
	(0.0366)	(0.0301)	(0.0257)	(0.0206)	(0.0260)
Lagged Agreement	-0.0013	0.0031^{*}	0.0038^{***}	0.0002	0.0073^{***}
	(0.0046)	(0.0034)	(0.0013)	(0.0015)	(0.0057)
Both Military	0.0028	0.0064	-0.0025^{*}	-0.0008	0.0149^{**}
	(0.0038)	(0.0063)	(0.0027)	(0.0021)	(0.0085)
Both in Previous Session	0.0073	0.0040	-0.0055^{*}	-0.0001	0.0021
	(0.0053)	(0.0048)	(0.0034)	(0.0020)	(0.0039)
Both First Session	0.0044	0.0023	0.0093^{**}	0.0046^{*}	-0.0019
	(0.0045)	(0.0025)	(0.0020)	(0.0014)	(0.0021)
Both College	0.0100^{*}	0.0028	0.0072^{***}	0.0040^{**}	0.0000
	(0.0012)	(0.0011)	(0.0006)	(0.0004)	(0.0010)
Lagged Coabsence	0.0012	0.0030^{*}	0.0014^{*}	0.0019^{***}	0.0036^{*}
	(0.0011)	(0.0015)	(0.0007)	(0.0005)	(0.0015)
Same Party	0.0565^{***}	0.0181^{***}	0.0163^{***}	0.0250^{***}	0.0117^{***}
	(0.0053)	(0.0034)	(0.0019)	(0.0019)	(0.0021)
Same State	0.0602^{***}	0.0630^{***}	0.0525^{***}	0.0506^{***}	0.0436^{***}
	(0.0071)	(0.0069)	(0.0046)	(0.0038)	(0.0061)
Same Region	0.0100^{***}	0.0080^{*}	0.0051^{**}	0.0091^{***}	0.0195^{***}
	(0.0029)	(0.0032)	(0.0016)	(0.0014)	(0.0046)
Same Occupation	0.0048	0.0050^{**}	0.0047^{**}	0.0019	0.0122^{**}
	(0.0025)	(0.0019)	(0.0018)	(0.0011)	(0.0046)
Age Difference	-0.0025^{*}	-0.0031^{**}	-0.0020^{***}	-0.0005	-0.0024^{*}
	(0.0012)	(0.0011)	(0.0006)	(0.0004)	(0.0010)
Seniority Difference	0.0008	0.0001	-0.0006	-0.0002	-0.0002
	(0.0010)	(0.0017)	(0.0007)	(0.0007)	(0.0015)
Both First Session	0.0044	0.0023	0.0093^{**}	0.0046^{*}	-0.0019
	(0.0053)	(0.0048)	(0.0034)	(0.0020)	(0.0039)
Both in Previous Session	0.0073	0.0040	-0.0055^{*}	-0.0001	0.0021
	(0.0038)	(0.0063)	(0.0027)	(0.0021)	(0.0085)
Neither in Previous Session	0.0125^{*}	0.0029	-0.0037	0.0019	0.0018
	(0.0053)	(0.0047)	(0.0027)	(0.0018)	(0.0033)
Both College	0.0100^{*}	0.0028	0.0072^{***}	0.0040^{**}	0.0000
	(0.0045)	(0.0025)	(0.0020)	(0.0014)	(0.0021)
Both Military	0.0028	0.0064	-0.0025^{*}	-0.0008	0.0149^{**}
	(0.0046)	(0.0034)	(0.0013)	(0.0015)	(0.0057)
n Dyads	111000	134522	274565	384410	317086
n Legislators	521	485	728	898	890
n Sessions	11	8	12	15	12

The table presents the results of linear probability models for each era. Standard errors are cluster-robust for dyadic data (Aronow, Samii and Assenova 2015). Models include fixed effect for congressional sessions and legislators. The covariates *Neither College* and *Neither Military* are subsumed by legislator-level fixed effects.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table A5: Linear Probability Models of Repeated ***Coresidence*** with Legislator Fixed Effects

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged Agreement	0.0331	0.0367*	0.0547**	0.0538**	0.0120
	(0.0391)	(0.0167)	(0.0201)	(0.0191)	(0.0146)
Lagged Coabsence	−0.0170	0.0091	0.0139	0.0143	0.0030
	(0.0183)	(0.0225)	(0.0144)	(0.0134)	(0.0185)
Same Party	0.0885	0.0102	0.0033	0.0118	0.0094
	(0.0822)	(0.0415)	(0.0315)	(0.0483)	(0.0236)
Same State	0.0832*	0.0404	0.0921***	0.0480*	0.0448*
	(0.0332)	(0.0291)	(0.0233)	(0.0229)	(0.0205)
Same Region	0.0605	0.0411	−0.0354	0.0546*	0.0446
	(0.0381)	(0.0426)	(0.0275)	(0.0245)	(0.0264)
Same Occupation	0.0091	−0.0081	0.0018	0.0149	0.0314
	(0.0211)	(0.0225)	(0.0243)	(0.0213)	(0.0191)
Age Difference	0.0095	0.0018	−0.0158	−0.0096	−0.0118
	(0.0102)	(0.0132)	(0.0108)	(0.0089)	(0.0095)
Seniority Difference	0.0165	−0.0444	−0.0068	0.0071	0.0018
	(0.0174)	(0.0266)	(0.0118)	(0.0168)	(0.0135)
Both College	0.0646*	−0.0463	0.0492	0.0139	0.0133
	(0.0329)	(0.0299)	(0.0360)	(0.0241)	(0.0206)
Both Military	0.0303	0.0468	−0.0060	−0.0057	0.0266
	(0.0352)	(0.0467)	(0.0327)	(0.0289)	(0.0280)
n Dyads	1881	2543	3857	5175	4706
n Legislators	341	422	646	791	675
n Sessions	7	8	12	15	11

The table presents the results of logistic regression models for each era, on the subsamples of dyads who were coresidents in the previous time period. Sessions are excluded when no evidence exists for the relevant prior session. Standard errors are cluster-robust for dyadic data (Aronow, Samii and Assenova 2015). Models include fixed effect for congressional sessions and legislators. The covariates *Neither College* and *Neither Military* are subsumed by legislator-level fixed effects.

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Bipartite ERGMs of *Residence*

As discussed in the text, one potential objection to our method is that legislators could not select coresidents *per se*; rather, they could choose where they lived. That line of reasoning suggests that rather than model ***Coresidence*** choices, we should model ***Residence*** choices. The result is that we have a bipartite, or two-mode, network that links legislators to residences, rather than legislators to legislators. The evidence in the main text could be seen as the one-mode projection of this two-mode network.

To vet the robustness of our findings to this analytic choice, we fit a series of bipartite exponential random graph models (ERGM, Wang, Pattison and Robbins 2017). Doing so means limiting attention to residences that could accommodate more than one legislator, and therefore we focus on boardinghouses and hotels, dropping private residences. These models account for homophily with statistics that count the number of residents at each location who shared some trait of a particular legislator, or take an average of a continuous covariate over other residents. Furthermore, ERGM coefficients can be expressed in terms of conditional log-odds (Hunter, Goodreau and Handcock 2013). The result is that, for example, the coefficient on ***Same Party*** can be interpreted as the increase in the linear predictor of a legislator choosing a residence with one additional member of his party. We also can now include indicator variables for ***Hotel*** and ***Boardinghouse***, so as to adjust for the relative popularity of these two different types of residence. Finally, we include a geometrically weighted term at the residence level, b2nodematch (Bomiriya 2014), which further controls for the endogenous popularity of different locations. For inference, we used the bootstrapped temporal ERGM approach (Leifeld, Cranmer and Desmarais 2018), in which we resampled over slices of the network, in each case resampling 1000 times.

The results appear in Table A6, and they broadly support the results shown in the text. In fact, the bipartite ERGMs present stronger statistical significance across the board. We conclude that the analytic choice we made, to focus on ***Coresidence*** rather than ***Residence***, did not alter the inferences we made. Moreover, because our final goal is to create legislator-dyad-level probabilities of ***Coresidence***, we prefer the models presented in the main text.

Table A6: Bipartite ERGMs of ***Residence***

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged Coresidence	4.11^{***}	4.93^{***}	4.72^{***}	4.40^{***}	3.73^{***}
	$[3.44, 4.76]$	$[4.44, 5.29]$	$[4.35, 5.05]$	$[4.02, 4.75]$	$[3.19, 4.25]$
Lagged Agreement	0.67^{***}	0.55^{***}	0.72^{***}	0.74^{***}	0.68^{***}
	$[0.49, 0.93]$	$[0.35, 0.78]$	$[0.53, 0.89]$	$[0.59, 0.91]$	$[0.55, 0.86]$
Lagged Coabsence	0.07	0.24^{***}	0.12^{***}	0.13^{***}	0.12^{**}
	$[-0.05, 0.22]$	$[0.11, 0.35]$	$[0.06, 0.19]$	$[0.06, 0.19]$	$[0.05, 0.18]$
Same Party	0.16^{***}	0.08^{***}	0.12^{***}	0.16^{***}	0.05^{***}
	$[0.13, 0.18]$	$[0.06, 0.11]$	$[0.09, 0.17]$	$[0.10, 0.20]$	$[0.02, 0.10]$
Same State	0.41^{***}	0.38^{***}	0.38^{***}	0.38^{***}	0.21^{***}
	$[0.33, 0.48]$	$[0.32, 0.43]$	$[0.34, 0.42]$	$[0.31, 0.44]$	$[0.16, 0.26]$
Same Region	0.03^{*}	0.02	-0.00	0.02	0.02^{***}
	$[0.00, 0.05]$	$[-0.01, 0.04]$	$[-0.03, 0.03]$	$[-0.00, 0.04]$	$[0.01, 0.04]$
Same Occupation	0.01	0.02	0.03^{*}	0.01	0.02^{*}
	$[-0.03, 0.04]$	$[-0.00, 0.04]$	$[0.00, 0.06]$	$[-0.01, 0.03]$	$[0.00, 0.04]$
Seniority Difference	-0.12^{*}	-0.13^{**}	0.03	0.03	-0.17^{***}
	$[-0.23, -0.01]$	$[-0.22, -0.04]$	$[-0.05, 0.12]$	$[-0.04, 0.09]$	$[-0.22, -0.10]$
First Session	-0.07^{**}	-0.02	-0.04	-0.06^{*}	-0.00
	$[-0.11, -0.04]$	$[-0.06, 0.03]$	$[-0.10, 0.00]$	$[-0.16, -0.00]$	$[-0.06, 0.04]$
Age Difference	-0.13	-0.15^{***}	-0.18^{**}	-0.11^{***}	-0.17^{***}
	$[-0.32, 0.05]$	$[-0.22, -0.08]$	$[-0.28, -0.07]$	$[-0.16, -0.05]$	$[-0.24, -0.09]$
In Prev. Session	0.01	0.04	0.04	0.06^{**}	0.01
	$[-0.09, 0.12]$	$[-0.02, 0.09]$	$[-0.05, 0.14]$	$[0.02, 0.17]$	$[-0.04, 0.08]$
Not in Prev. Session	-0.00	-0.02	-0.06^{*}	-0.03	-0.01
	$[-0.05, 0.06]$	$[-0.06, 0.02]$	$[-0.11, -0.02]$	$[-0.06, 0.01]$	$[-0.05, 0.02]$
College Grad.	-0.00	0.01	0.06^{*}	0.03	-0.00
	$[-0.06, 0.06]$	$[-0.05, 0.05]$	$[0.01, 0.10]$	$[-0.03, 0.07]$	$[-0.04, 0.02]$
No College	-0.04	-0.04	0.04^{*}	-0.00	-0.03
	$[-0.10, 0.01]$	$[-0.09, 0.00]$	$[0.01, 0.07]$	$[-0.04, 0.03]$	$[-0.08, 0.01]$
Military	-0.02	0.01	-0.23^{*}	-0.19^{***}	0.03
	$[-0.15, 0.08]$	$[-0.10, 0.08]$	$[-0.44, -0.02]$	$[-0.35, -0.06]$	$[-0.03, 0.05]$
Not Military	-0.04	-0.01	-0.09^{*}	-0.08^{***}	-0.00
	$[-0.10, 0.02]$	$[-0.06, 0.03]$	$[-0.15, -0.02]$	$[-0.12, -0.04]$	$[-0.02, 0.01]$
Boardinghouse	0.86^{***}	1.52^{***}	1.51^{***}	1.73^{***}	2.54^{***}
	$[0.44, 1.19]$	$[1.01, 2.12]$	$[0.99, 1.99]$	$[1.19, 2.30]$	$[1.75, 3.29]$
Hotel	1.23^{***}	1.71^{***}	2.09^{***}	2.28^{***}	3.26^{***}
	$[0.70, 1.64]$	$[1.11, 2.41]$	$[1.56, 2.58]$	$[1.79, 2.79]$	$[2.46, 4.08]$
GW-B2-Degree	-1.81^{*}	-1.42^{**}	-3.25^{***}	-2.82^{***}	-3.31^{***}
	$[-3.24, -0.42]$	$[-2.09, -0.53]$	$[-4.11, -2.45]$	$[-3.52, -2.18]$	$[-4.67, -2.17]$
Edges	-4.96^{***}	-5.67^{***}	-5.64^{***}	-5.99^{***}	-6.95^{***}
	$[-5.41, -4.46]$	$[-6.29, -5.19]$	$[-6.16, -5.02]$	$[-6.58, -5.40]$	$[-7.81, -6.04]$
n Residences	374	335	722	1046	1107
n Legislators	1534	1467	2567	3376	2762
n Sessions	11	8	12	15	12

The table presents the results of bootstrapped TERGM models of bipartite residence networks. 95% confidence intervals are based on 1000 bootstraps over networks. Models also include indicators for each session and for availability of covariate.

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Covariate Balance

To assess balance, we estimated regressed each covariate on ***Coresidence***, once without weighting and again, weighting by the inverse probability of treatment. We calculated predicted probabilities of treatment by estimating a logistic regression model of ***Coresidence*** for each era. These models not only included the covariates shown in Tables 1 and 2 from the main text, they also included all second and third order multiplicative interactions, less those that were subsumed by collinearity. Finally, we calculated dyadic robust standard errors for each model and covariate, which we used to calculate standardized coefficients. These coefficients can be interpreted as measures of imbalance. In general, standardized coefficients with larger magnitudes imply worse balance, as they signify large covariate differences between treatment and control.

Figure A1 graphically displays the improvement in covariate balance that we achieved by weighting. Each line depicts a covariate, comparing its standardized coefficient without weighting, on the left, to that after weighting, on the right. To aid visual comparison, the light gray box depicts two standard deviations, and the dark gray box depicts one.

Of the 135 covariate-era pairs we consider, weighting improved balance in 128 cases, and in the remaining seven cases, the standardized coefficient after weighting had magnitude less than 0.7. Without weighting, balance was substantially worse. The absolute value of standardized coefficients was larger than 2 in over 50% of cases, and more than 1 in over 70%.

More importantly, balance on ***Lagged Agreement*** is substantially improved. Before weighting, the standardized coefficients ranged from 2.3 up to 17.5. After weighting, the such standardized coefficient with the largest magnitude was 0.4. We therefore conclude that our weighting strategy has adequately adjusted for selection based on these observables.

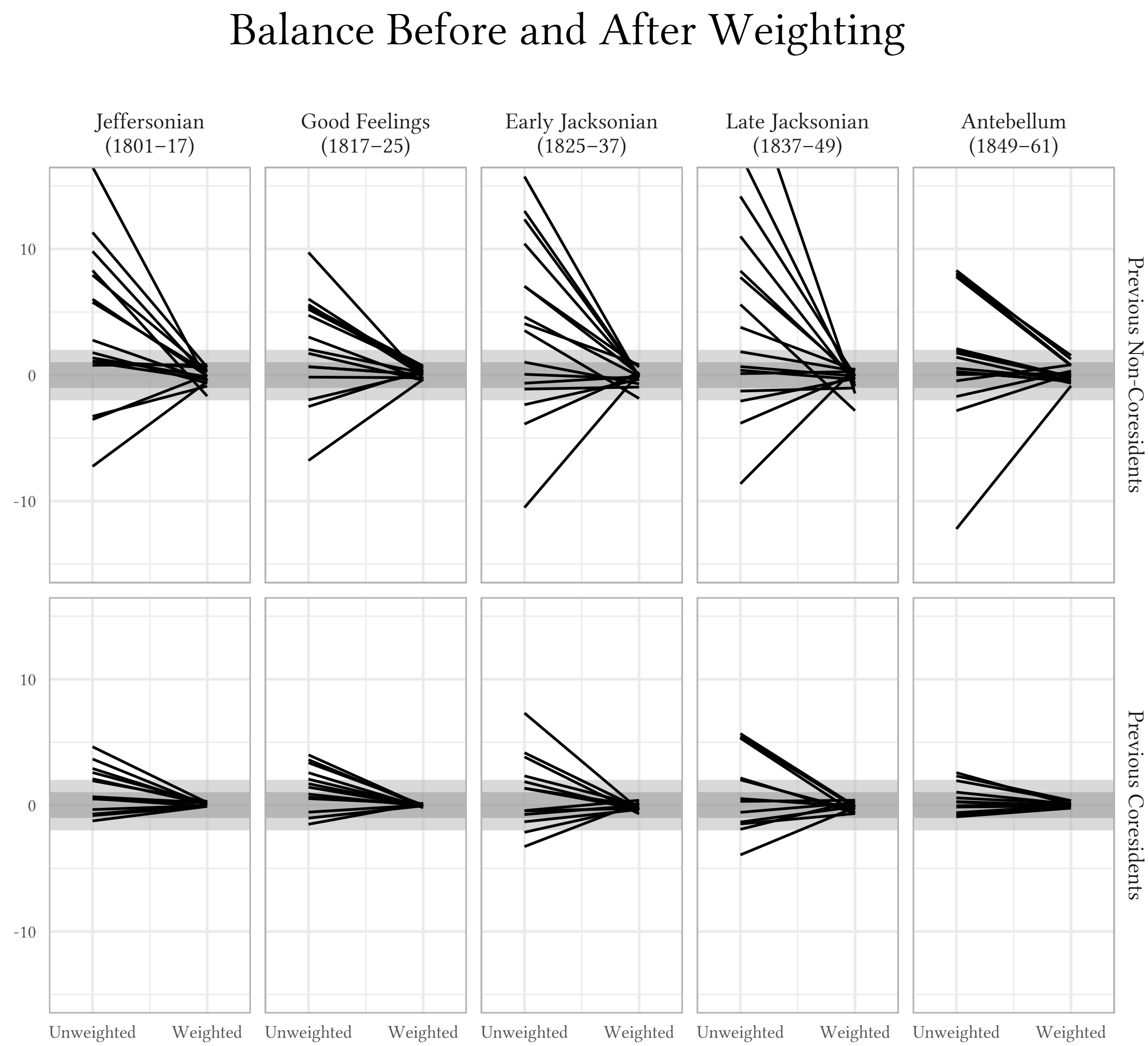

Figure A1: The figure presents balance statistics for each era, covariate, and sample from the inverse probability of treatment weighting design. Each panel contrasts standardized coefficients from regressions of the covariate labeled on ***Coresidence***, unweighted on the left and weighted on the right. Gray rectangles indicate the range of the Normal distribution's 95% interval. In almost every case, balance is improved by weighting, and in most cases, the improvement reduces dramatic levels of imbalance to levels that justify causal comparisons, given sequential ignorability.

Attrition

In the IPW study that comprised all dyads between 1801 and 1861, a small number of cases have missing outcomes at the legislator dyad-level, and therefore legislator-level attrition. That is, in a few cases, a dyad of two legislators was eligible to vote on at least one roll call, yet both members did not do so on any roll calls at all. This is typically because one legislator voted while the other did not, often because the latter had yet to arrive in DC.

Specifically, we are missing ***Agreement***, which requires both legislators to have voted on at least one roll call, for 4,450 of cases in which both legislators were eligible to vote, or about 0.3%. Across eras, this legislator-level attrition rate ranges from 0.2% to 0.5%.

We caution that ***Coabsence*** is subtly different from legislator-level attrition. ***Coabsence*** occurs when both members miss a roll call on which they were both eligible to vote, is responsible for attrition only when both legislators were both absent from 100 percent of the votes on which they were eligible. Of the 4,450 cases of attrition, the ***Coabsence*** rate was 100% for only 115 dyads, meaning in just those cases, 115/4450 = 2.6%, do we have attrition due to ***Coabsence***.

To gauge the inferential impact of legislator-level attrition, we imputed two complete datasets with "worst case scenarios" (Manski 1989). That is, we completed (1) one dataset imputing all missing agreement scores to be equal to 0% for coresidents and 100% for non-coresidents, and (2) a second dataset imputing all missing outcomes to be 100% for coresidents and 0% for non-coresidents. We then repeated our analysis (selection model, IP weighting, exposure model) on each.

A larger number of cases have attrition at the roll call vote-level. That is, in any given session, many legislators were absent on many roll call votes, and therefore—as is standard in the literature—our measure of ***Agreement*** drops these votes in its calculation. We therefore refer to this measure as ***Agreement (both Voting)*** when it is necessary to disambiguate it from other possible measures, as in the next section of this appendix.

While the measure of ***Agreement (both Voting)*** is a standard one, we proceeded to perform a "worst case" bounds analysis at this roll call vote-level. As in the case of attrition at the legislator-level, we seek to create two logically possible, complete datasets at the legislator-roll call vote level that will serve as worse case scenarios for measurement of our estimand, the differences in average agreement scores between coresidents and non-coresidents.

Unlike in the case of the legislator-level, we cannot simply impute missing values directly, at the level of the legislator dyad-roll call vote pair, because doing so would result in a logically incoherent set of imputed values. For example, a naive method for creating one of the worst case datasets would be (1) to sweep through all legislator dyad-roll call votes in which one legislator was eligible to vote but absent, (2) in the case that the members of the dyad were coresidents, impute the missing vote to match that of the other member of the dyad, and (3) in the cases that the members of the dyad were not coresidents, impute the missing vote to be the opposite of that of the other member of the dyad. To see why this method results in an incoherent dataset, suppose that a legislator i with the missing vote did not reside with at two other legislators, j and k, and further suppose that j voted Yea while k voted Nay. The imputed dataset would then include an observation in which i's vote was imputed to Nay, and another observation in which i's vote was imputed to Yea. Since legislators cannot simultaneously vote both Yea and Nay on a single vote, the result is a logically incoherent dataset.

Therefore, we must develop a more sophisticated method to create the two logically coherent that would bound the possibly measurable effects of coresidence. To do so, we must effectively optimize over a vector with binary values for entries, and we must do so twice: once for the worst case upper bound and a second time for the worst case lower bound. Unfortunately, the search problem here is of astounding magnitude. For example, in the 1st session of the 7th Congress, there were 2208 missing roll call votes. As each of these could be a Yea or a Nay, we must search for the 2208-length vectors of Yeas or Nays that would yield maximally large or small differences in means between Agreement for coresidents, and that for non-coresidents. The size of the space is therefore 2^{2208}, which is far too large to be solvable in a reasonable amount of time. Moreover, we must do so twice, for each of the 58 sessions in our sample.

To cope with the intractable scale of this search problem, we used a heuristic method to approximate worst case bounds. To begin, we reconceptualized ***Agreement*** so that it can encompass convex combinations of Yea and Nay. That is, we redefined

$$\textbf{\textit{Agreement}} = \sum_{v=1}^{V} p_{iv}p_{jv} + (1 - p_{iv})(1 - p_{jv}),$$

where i, j index over legislators, and $v = 1, \ldots, V$ indexes over roll call votes. We let $p_{iv} = 1$ if i cast a Yea vote (or paired Yea) on v, and $p_{iv} = 0$ if i cast a Nay vote (or paired Nay) on v. This reconceptualization of ***Agreement*** allows us to include uncertainty about whether a vote should be imputed Yea or Nay. Moreover, when there is no attrition at the roll call vote-level, this definition of ***Agreement*** matches ***Agreement (both Voting)***.

To describe our imputation algorithm, first index the missing vector of votes with $k = 1, ..., K$. We denote the missing roll call votes with k-vectors i^{miss} and v^{miss}, where i^{miss} refers to the identity of the legislator with the missing vote, and v^{miss} refers to the identity of the roll call vote. Let $\hat{p}^{\text{miss}}$ denote a k-vector of imputed values corresponding to that index.

To approximate the upper bound, we used the following algorithm. We initialize the missing votes by imputing them to $\hat{p}_0^{\text{miss}} = 0.5$. Then, to define $\hat{p}_{t+1}^{\text{miss}}$, we sweep through $k = 1, \ldots, K$. For each k, we:

1. Create two copies of $\hat{p}_t^{\text{miss}}$, one denoted $\hat{p}_{t,k,1}^{\text{miss}}$ with the k^{th} missing vote imputed to 1, and one denoted $\hat{p}_{t,k,0}^{\text{miss}}$ with the k^{th} missing vote imputed to 0.
2. Populate the legislator dyad-roll call vote dataset with $\hat{p}_{t,k,1}^{\text{miss}}$ and calculate ***Agreement***, denoted $A_{1,k}$.
3. Populate the legislator dyad-roll call vote dataset with $\hat{p}_{t,k,0}^{\text{miss}}$ and calculate ***Agreement***, denoted $A_{0,k}$.
4. Calculate the difference in means in $A_{1,k}$ between coresident dyads and non-coresident dyads, denoted $\Delta_{1,k}$.
5. Calculate the difference in means in $A_{0,k}$ between coresident dyads and non-coresident dyads, denoted $\Delta_{0,k}$.
6. If $\Delta_{1,k} > \Delta_{0,k}$, set the k^{th} value of $\hat{p}_{t+1}^{\text{miss}}$ to be 1, if $\Delta_{1,k} < \Delta_{0,k}$, set the k^{th} value of $\hat{p}_{t+1}^{\text{miss}}$ to be 0, and if $\Delta_{1,k} = \Delta_{0,k}$, set the k^{th} value of $\hat{p}_{t+1}^{\text{miss}}$ to be 0.5.

We iterate this process until at least 99% of missing votes do not change, or until the process has iterated 10 times.

To approximate the lower bound, we used a similar algorithm, except that we altered step (6.) so that if $\Delta_{1,k} > \Delta_{0,k}$, set the k^{th} value of $\hat{p}_{t+1}^{\text{miss}}$ to be 0, and if $\Delta_{1,k} < \Delta_{0,k}$, set the k^{th} value of $\hat{p}_{t+1}^{\text{miss}}$ to be 1.

Once we have the completed worst-case datasets, we then repeated our analysis (selection model, IP weighting, exposure model) on each.

Table A7 reports these worst-case outcomes at both the legislator- and roll call vote-level. In all but one case, the worst case bounds are positive for Previous Non-Coresidents. For Previous Coresidents, the legislator-level bounds are positive, while the roll call-level bounds are positive only in one case.

Table A7: Attrition and Worst-Case Estimates

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Previous Coresidents					
Estimate (no imputation)	2.7	1.9	0.3	1.2	1.6
Lower bound (legislator-level)	3.0	1.9	0.4	1.5	1.8
Upper bound (legislator-level)	2.6	1.9	0.2	1.1	1.4
Lower bound (roll call-level)	−0.7	−0.6	−0.6	−0.2	0.1
Upper bound (roll call-level)	9.1	7.7	9.9	11.7	13.2
Previous Non-Coresidents					
Estimate (no imputation)	4.7	4.3	6.3	5.1	3.6
Lower bound (legislator-level)	5.0	4.6	6.4	5.3	3.8
Upper bound (legislator-level)	4.4	4.1	6.2	4.8	3.5
Lower bound (roll call-level)	0.2	−0.2	1.6	1.2	0.6
Upper bound (roll call-level)	14.2	11.5	15.4	18.1	16.1

Attrition rate = fraction of eligible dyads (i.e., there was at least one roll call for which both legislators were eligible) for which both legislators cast votes on no roll calls.
Estimates with Attrition are those from the paper (e.g., Figure 3).
Estimates with Min. ATE have missing ***Agreement*** scores set to 0% for ***Coresidence*** $= 1$, and 100% for ***Coresidence*** $= 0$.
Estimates with Max. ATE have missing ***Agreement*** scores set to 100% for ***Coresidence*** $= 1$, and 0% for ***Coresidence*** $= 0$.

Alternative Outcomes and Specifications

We conducted several auxiliary analyses to parallel our IPW design-study of the effect of ***Coresidence*** on ***Agreement***. Using the same process—fitting a selection model, calculating inverse probability weights, and estimating causal effects of ***Coresidence***—we also estimated the effects of ***Coresidence*** on (1) ***Coabsence***, which is defined for each dyad as the number of roll call votes in a congressional session (or time period) on which both legislators did not cast a vote, divided by the total number of roll call votes in the congressional term, (2) ***Agreement with Coabsence***, which is defined for each dyad as the number of roll calls on which both legislators voted Yea, or both voted Nay, or both were absent, divided by the total number of roll call votes on which both were eligible, (3) ***Imputed Agreement***, which first imputes missing roll call votes using ideal point estimates to classify those votes as Yea or Nay, (4) ***Agreement (both Voting)***, the outcome variable from the text, but now adjusting for an additional covariate that measures whether both members of a dyad chose to live in a boardinghouse during that session, and (5) ***Agreement (both Voting)***, the outcome variable from the text, now using not only IPW but also including legislator-level fixed effects in the form of dummy variables for each legislator. In this last case, we rely on dyadic-robust standard errors rather than bootstrapping, to cope with issues created by resampling over time slices that do not cleanly coincide with the appearance of legislators.

Table A8 present these results, along with the results from the paper for the sake of comparison. In the first four cases, results closely resemble those presented in the paper, regardless of specification and when the outcome variable is the more conventional ***Agreement*** score familiar from the literature. That is, we still see that the effects of ***Coresidence*** are larger for previous non-coresidents than for previous coresidents. In the final case, we see sporadic effects of ***Coresidence*** on ***Coabsence***. These estimates are insignificant in all but three cases, and are all much smaller than 1% in magnitude.

Finally, the last two rows of Table A8 present similar analyses for two different subgroups: dyads of legislators who lived in boardinghouses, and dyads of legislators who both lived in hotels. In each case, we again conducted an analysis to parallel our IPW design, now including ***Previous Coresidence*** and its interactions as regressors in the selection model.

Comparing these two rows, we see that there was a significant difference between the effects for boardinghouse and hotel residents in both Jacksonian Era, but not for the other three eras. The large confidence interval for the subgroup of hotel residents in the Jeffersonian Era is explained by the small number of legislators in that group (n = 151).

Table A8: Alternative Outcomes and Specifications

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Previous Coresidents					
Agreement (Both Voting)	2.6	2.2	0.4	1.0	1.6
	[0.8, 4.4]	[1.1, 3.9]	[−0.7, 1.6]	[−0.3, 2.3]	[−0.3, 4.3]
Adjusting for Both Boardinghouse	2.7	2.3	−0.7	0.5	1.6
	[0.3, 4.9]	[0.8, 4.5]	[−3.6, 1.0]	[−1.1, 2.0]	[−0.4, 4.3]
w/ Legislator Fixed Effects	3.0	2.0	0.1	1.8	3.4
	[0.7, 5.2]	[−1.3, 5.4]	[−1.9, 2.2]	[0.4, 3.3]	[0.5, 6.4]
Agreement (With Coabsence)	2.1	1.8	2.2	1.9	2.2
	[−0.3, 4.0]	[0.6, 3.3]	[1.2, 3.4]	[0.9, 2.7]	[0.5, 4.2]
Agreement (Imputed)	1.9	1.5	−0.8	1.3	2.5
	[−0.1, 3.4]	[−0.4, 3.0]	[−2.5, 1.1]	[0.1, 2.5]	[0.3, 5.7]
Coabsence	−0.0	0.2	0.0	−0.3	−0.0
	[−0.6, 0.4]	[−0.3, 0.9]	[−0.6, 0.6]	[−0.8, 0.3]	[−1.2, 1.0]
Previous Non-Coresidents					
Agreement (Both Voting)	4.0	4.2	6.3	5.0	3.7
	[1.3, 5.8]	[2.4, 6.1]	[5.0, 7.6]	[3.2, 7.2]	[2.3, 5.3]
Adjusting for Both Boardinghouse	2.2	4.3	5.5	2.9	4.4
	[−2.4, 4.9]	[2.5, 6.3]	[4.4, 6.8]	[0.9, 5.0]	[3.0, 6.0]
w/ Legislator Fixed Effects	5.3	4.5	6.4	4.2	7.2
	[0.7, 9.9]	[2.1, 6.8]	[4.2, 8.5]	[0.9, 7.5]	[4.6, 9.8]
Agreement (With Coabsence)	3.8	2.6	4.8	4.0	2.6
	[1.4, 5.2]	[1.6, 3.9]	[3.9, 5.8]	[2.7, 5.9]	[1.7, 3.5]
Agreement (Imputed)	2.6	2.6	6.2	5.5	4.4
	[−1.3, 5.5]	[1.0, 4.4]	[4.1, 8.3]	[3.0, 8.6]	[2.6, 6.6]
Coabsence	−0.0	0.4	0.6	0.5	0.3
	[−0.6, 0.6]	[0.2, 0.7]	[0.0, 1.1]	[0.1, 1.0]	[−0.0, 0.8]
Both Boardinghouse Residents					
Agreement (Both Voting)	5.4	3.9	7.0	6.6	3.4
	[2.7, 7.6]	[2.2, 5.8]	[5.4, 8.6]	[4.1, 9.2]	[0.6, 6.0]
Both Hotel Residents					
Agreement (Both Voting)	1.6	4.3	1.8	1.5	3.4
	[−27.0, 19.8]	[1.8, 8.7]	[−3.1, 6.8]	[0.1, 3.5]	[1.8, 5.4]

Deceased Legislator Residence Study

In this section, we describe the protocol we used in the Deceased Legislator Residence Study, and report regression tables and robustness checks. First, we describe the protocol:

1. Identify all deceased legislators who died within a congressional term, and had coresidents before their deaths. Eliminate cases in which both deceased legislators died in the same boardinghouse in the same term. This identifies 60 cases.
2. For each deceased legislator, take the congressional term in which they died, and build the following roll call dataset:
   (a) Identify the surviving coresidents of the deceased legislator, and split their roll call records into two parts, one from before the decedent died, and another after the decedent died.
   (b) Append the decedent's roll call record, and those of all other legislators below these two sets of surviving coresidents' roll call records.
3. Estimate 2 dimensional ideal points using the ideal function from the pscl package (Jackman 2017), with 5000 burnin iterations and 1000 simulations.
4. Post-process so that the mean ideal point has Euclidean distance 1 from the origin.
5. Calculate the Euclidean distance between each surviving coresident's pre-death ideal point and the decedent's ideal point. Similarly, calculate the distance between the surviving coresident's post-death ideal point and the decedent's ideal point. Do so for each simulation, and then take the median over all simulations. Log that median to get a measure of ***log Ideal Point Distance***, which will be the primary outcome variable.
6. Build a dataset in which each decedent-surviving coresident dyad has two observations, one from before death and one from after death. The resulting dataset has 730 observations: 365 decedent-surviving coresident dyads before death, and 365 after.
7. To create the control cases, identify all 967 residence-congressional term pairs in which no residents died. For each first-session resident, repeat steps 2–5, using that resident as a "control" decedent marking their "death" at the end of the first session. The resulting dataset has 56,724 observations: 28,362 control dyads before "death", and 28,362 after.
8. Combine the two datasets, including an indicator variable name ***Resident Death***, which is 1 if a member of a dyad actually died, and 0 otherwise. Take first differences of the outcome variables, and then take averages of the resulting changes at the residence-level. Record the number of dyads included in each residence. The resulting residence-level dataset has 1027 observations, equal to the 60 residences in which a resident died, plus the 967 in which no residents died.
9. Estimate the weighted linear regression model

$$\text{mean change in } \boldsymbol{\textit{log Ideal Point Distance}}_{rt} = \boldsymbol{\textit{Resident Death}}_{rt} + a_t,$$

   where r indexes residences, t indexes congressional term, and a_t is an era-level fixed effect. Weights are given by the number of dyads in each residence.
10. Bootstrap by resampling over residences, stratifying by residences with decedents and those with controls. Repeat 1000 times.
11. For hypothesis tests, use two-sided bootstrap p-values equal to twice the frequency of bootstrap estimates greater than 0, or twice the frequency less than 0, whichever is smaller.

Table A9: Deceased Legislator Study Models and Alternative Outcomes and Specifications

	[1]	[2]	[3]	[4]	[5]
Resident Death	0.072*	0.042	0.029	0.066	0.088*
	(0.035)	(0.035)	(0.067)	(0.043)	(0.044)
Boardinghouse			0.028		
			(0.023)		
Residence Size				−0.003	
				(0.005)	
Within Session					−0.035
					(0.069)
Resident Death × ***Boardinghouse***			0.068		
			(0.080)		
Resident Death × ***Residence Size***				0.004	
				(0.028)	

Columns [1], [3], [4], and [5] present weighted least squares models of mean change in ***log Ideal Point Distance*** between members of each dyad, averaged at the residence level ($n = 1027$). Observations are weighted by number of dyads included from each residence. Column [2] presents a similar model of mean change in (unlogged) ***Ideal Point Distance***. ***Residence Size*** is mean centered and scaled to have standard deviation 1. Estimates are given by bootstrap means. All columns include fixed effects for eras and bootstrapped standard errors, with resamples by residence.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table A9, columns [1], presents model the results of the main model specification described in the paper. Column [2] of Table A9 uses a similar design, but with mean change in (unlogged) ***Ideal Point Distance*** as the outcome variable. Although the sign remains positive, the finding is not statistically significant. This result is therefore sensitive to transformation of the outcome variable. However, as we argued in the paper, logging is more appropriate in this case, given that ***Ideal Point Distance*** is positive-valued, and that the ideal points are measured separately by congressional term.

Columns [3], [4], and [5] present models with interaction terms. (There is no main effect in the model in column [5] because control dyads are always split between sessions, so ***Within Session*** only takes the value 1 when ***Resident Death*** equals 1.) In no case was the interaction term significant. Note that, in model [3], we can reject the hypothesis testing whether the sum of the coefficients on ***Resident Death*** and ***Resident Death*** × ***Boardinghouse*** is zero at $p = 0.032$.

To test whether our analyses are robust to the use of classical estimation and inference, we report estimates and heteroscedasticity-consistent standard errors (HCSEs) in Table A10. Results are very similar to those using bootstrap inference, from Table A9. We similarly replicated our placebo test using HCSEs, displayed in Figure A2. Results are very similar to those shown in the paper, with 5% of placebo p values less than or equal to 0.05, and 3.2% of placebo estimates larger than the actual estimate.

We prefer the main model specification because it emphasizes the focal role of the residence in the design. But for robustness, we also consider the following dyad-level model:

$$\textbf{\textit{log Ideal Point Distance}}_{dt} = \textbf{\textit{After Death}}_{dt} + \textbf{\textit{Resident Death}}_{dt} \times \textbf{\textit{After Death}}_{dt} + \alpha_d,$$

where d indexes dyads, t indexes congressional term, ***After Death*** indicates whether the outcome is measured before (0) or after (1) death/session break, and α_d is a dyad fixed effect. The

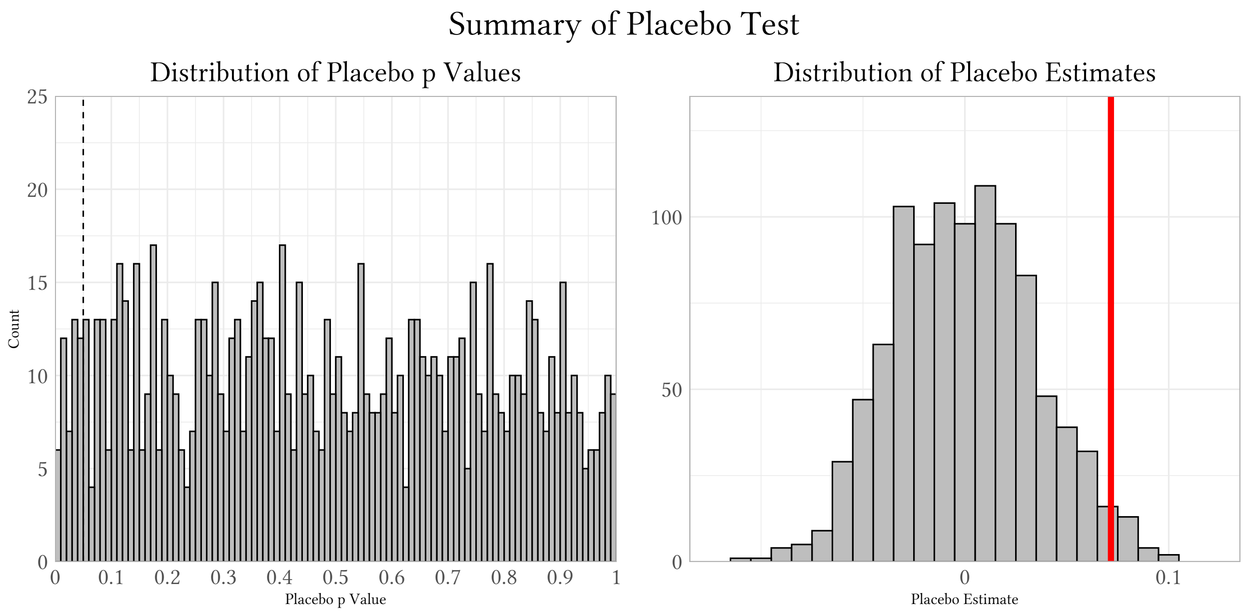

Figure A2: The figure presents the results of 1000 simulated placebo tests using all residences that did not experience a death of a resident. Residences that did experience a death are excluded. The p values and estimates presented here are based on classical estimation and heteroscedasticity-consistent standard errors. The panel on the left displays a histogram of the classical p values from these tests, and the panel on the right displays a histogram of the estimated placebo effects. Consistent with the design assumptions, only 5% of placebo p values are less than or equal to 0.05, and only 3.2% of placebo estimates exceed the actual estimated effect.

Table A10: Deceased Legislator Study Models with Classical Estimates and HCSEs

	[1]	[2]	[3]	[4]	[5]
Resident Death	0.071*	0.044	0.034	0.068	0.089*
	(0.034)	(0.035)	(0.049)	(0.051)	(0.043)
Boardinghouse			0.028		
			(0.023)		
Residence Size				−0.001	
				(0.005)	
Within Session					−0.037
					(0.065)
Resident Death × ***Boardinghouse***			0.058		
			(0.067)		
Resident Death × ***Residence Size***				0.002	
				(0.018)	

Columns [1], [3], [4], and [5] present weighted least squares models of mean change in ***log Ideal Point Distance*** between members of each dyad, averaged at the residence level ($n = 1027$). Observations are weighted by number of dyads included from each residence. Column [2] presents a similar model of mean change in (unlogged) ***Ideal Point Distance***. ***Residence Size*** is mean centered and scaled to have standard deviation 1. All columns include fixed effects for eras and heteroscedasticity consistent standard errors.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

main coefficient for ***Resident Death*** is subsumed by the fixed effects. The coefficients on the interaction ***Resident Death*** × ***After Death*** are comparable to the The design we present in the paper only differs by including fixed effects for era, which would also have been subsumed by the dyad fixed effects. If we omit era fixed effects from the main specification, the two models are identical. We also fit companion models for the others presented in Table A9. Estimates are similar to those from Table A9, although p values are slightly higher.

Table A11: Dyad-Level Models with Cluster-Robust Standard Errors

	[1]	[2]	[3]	[4]	[5]
After Death	0.066^{***}	0.058^{***}	0.031^{*}	0.066^{***}	0.066^{***}
	(0.008)	(0.009)	(0.013)	(0.008)	(0.008)
After Death × ***Resident Death***	$0.060^{\dagger}$	0.032	0.023	$0.061^{\dagger}$	$0.081^{\dagger}$
	(0.034)	(0.033)	(0.048)	(0.034)	(0.043)
After Death × ***Boardinghouse***			0.049^{**}		
			(0.016)		
After Death × ***Resident Death*** × ***Boardinghouse***			0.066		
			(0.066)		
After Death × ***Resident Size***				$-0.014^{\dagger}$	
				(0.008)	
After Death × ***Resident Death*** × ***Residence Size***				−0.008	
				(0.036)	
After Death × ***Within Session***					−0.045
					(0.066)

Columns present models with dyad-level fixed effects. Each observation is for a dyad of coresidents, either before death/session break or after. All omitted main terms are subsumed by the dyad-level fixed effects. Similarly, era-level fixed effects are omitted because they are subsumed by the dyad-level fixed effects. The outcome variable for columns [1], [3], [4], and [5] is ***log Ideal Point Distance***. Column [2] presents a similar model of (unlogged) ***Ideal Point Distance***. ***Residence Size*** is mean centered and scaled to have standard deviation 1. All columns include fixed effects for eras and cluster-robust standard errors at the level of the residence (n = 1027).

$^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

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