

Online Appendix:
Campaign Finance Legislation and the Supply-Side of the
Revolving Door

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A Effect of *Citizens United* on the Revolving Door: Additional Information and Analyses

A.1 Additional Information on Matching of Politician and Lobbyist Lists

To link the legislator data with the lobbyist information, I use a two-step procedure. First, I employ an automated algorithm that matches names between the lists. I use a measure based on the Levenshtein edit distance, which is a string metric that quantifies the difference between two sequences. Intuitively, it is the minimum number of insertions, deletions, or substitutions required to change one sequence into the other. To make them comparable, I use the following similarity measure: $1 - (d(s1, s2)/\max(A, B))$, where d is the Levenshtein distance function, $s1$ and $s2$ are the two strings, and A and B are their lengths. The procedure is implemented in R using the `agrep` function.

I set a low similarity threshold to minimize the number of false negative matches. This allows me to match cases such as a former New Jersey representative who appears in the election data as “Kamin, Dick” but is registered as a lobbyist under “Kamin, C Richard”. Of course, this creates many false positive results. For example, it matches “Kenny, Bernard F. Jr.” and “Flynn, Bernard M.” In a second step, I therefore check all matches manually. If there are doubts about a match, for example if it is a common name, I use supplementary information from internet searches to confirm that it is indeed the same person.

A.2 Descriptive Statistics

Table A1: Descriptive Statistics, House, All.

	Mean	Std. Dev.	Min	Max
Revolving Door Overall	0.016	0.126	0.000	1.000
Voluntary Revolving Door	0.012	0.107	0.000	1.000
Insurance Revolving Door	0.005	0.067	0.000	1.000
Non-Revolving Door Retirement	0.196	0.397	0.000	1.000
Ban _s × Post-CU _t	0.219	0.414	0.000	1.000
Cooling Off Law	0.522	0.500	0.000	1.000
Term Limit	0.236	0.425	0.000	1.000
Public Campaign Finance	0.111	0.314	0.000	1.000
Years in Office	7.877	6.658	1.000	42.000
Chamber Control Own Party	0.578	0.494	0.000	1.000
Speaker or Leader	0.026	0.159	0.000	1.000

Table A2: Descriptive Statistics, House, Republicans.

	Mean	Std. Dev.	Min	Max
Revolving Door Overall	0.020	0.141	0.000	1.000
Voluntary Revolving Door	0.015	0.120	0.000	1.000
Insurance Revolving Door	0.006	0.075	0.000	1.000
Non-Revolving Door Retirement	0.207	0.405	0.000	1.000
Ban _s × Post-CU _t	0.213	0.409	0.000	1.000
Cooling Off Law	0.496	0.500	0.000	1.000
Term Limit	0.250	0.433	0.000	1.000
Public Campaign Finance	0.103	0.304	0.000	1.000
Years in Office	7.138	5.862	1.000	42.000
Chamber Control Own Party	0.616	0.486	0.000	1.000
Speaker or Leader	0.027	0.161	0.000	1.000

Table A3: Descriptive Statistics, House, Democrats.

	Mean	Std. Dev.	Min	Max
Revolving Door Overall	0.012	0.110	0.000	1.000
Voluntary Revolving Door	0.009	0.094	0.000	1.000
Insurance Revolving Door	0.003	0.059	0.000	1.000
Non-Revolving Door Retirement	0.186	0.389	0.000	1.000
Ban _s × Post-CU _t	0.225	0.417	0.000	1.000
Cooling Off Law	0.547	0.498	0.000	1.000
Term Limit	0.223	0.416	0.000	1.000
Public Campaign Finance	0.117	0.322	0.000	1.000
Years in Office	8.564	7.254	1.000	42.000
Chamber Control Own Party	0.542	0.498	0.000	1.000
Speaker or Leader	0.025	0.157	0.000	1.000

A.3 Identification Assumptions and Pre-Treatment Balance Checks

A major assumption of my research design is that the loosening of campaign finance laws through *Citizens United* is indeed exogenous to legislators. This would be an invalid assumption if the ruling were a response to different developments in the treatment and control states. However, the Supreme Court case pertained to federal law and addressed a matter involving the candidacy of Hillary Clinton for president. The ruling only had an effect on state laws by extension, and state laws were not scrutinized or debated in the Supreme Court case.

The research design could still be problematic if states selected into the treatment in a way that there are different trends between treatment and control states in the factors that lead to the laws' adoption; and these factors are correlated with the outcome variable. This is unlikely. Many of the state laws that were struck down by *Citizens United* were passed decades ago (as early as 1908), at a time when the revolving door was practically non-existent (see La Raja and Schaffner, 2014). Abdul-Razzak, Prato and Wolton (2018) show that the adoption of spending bans was often the result of ballot initiatives or the threat thereof. It is unlikely that the factors that led some states to adopt campaign finance bans decades ago are correlated with today's prevalence of the revolving door, and that in addition these factors developed differently between treatment and control states.

In fact, there are no significant differences between treatment and control states on a large set of variables in both pre-treatment cycles, as Figure A1 demonstrates. It shows the Welch two sample t-test for differences in means between the two groups of states for the dependent and independent variables used in the estimations, as well as a number of important time-invariant features. Values between -1.96 and 1.96 indicate that the difference is not significant at the 95 percent level.

For the 2006-2007 (gray) as well as the 2008-2009 period (black), none of the differences in means are significantly different from zero. At the legislator level there are, among others, no significant differences in how common the revolving door is, how frequent incumbents leave office without becoming a lobbyist, how many years they have been in office, or how conservative or liberal they are. At the state level, roughly the same proportion in the treatment and control

groups have term limits, “cooling off” revolving door laws, and a public campaign finance system. The groups are also balanced in terms of their legislative professionalism and partisan composition.

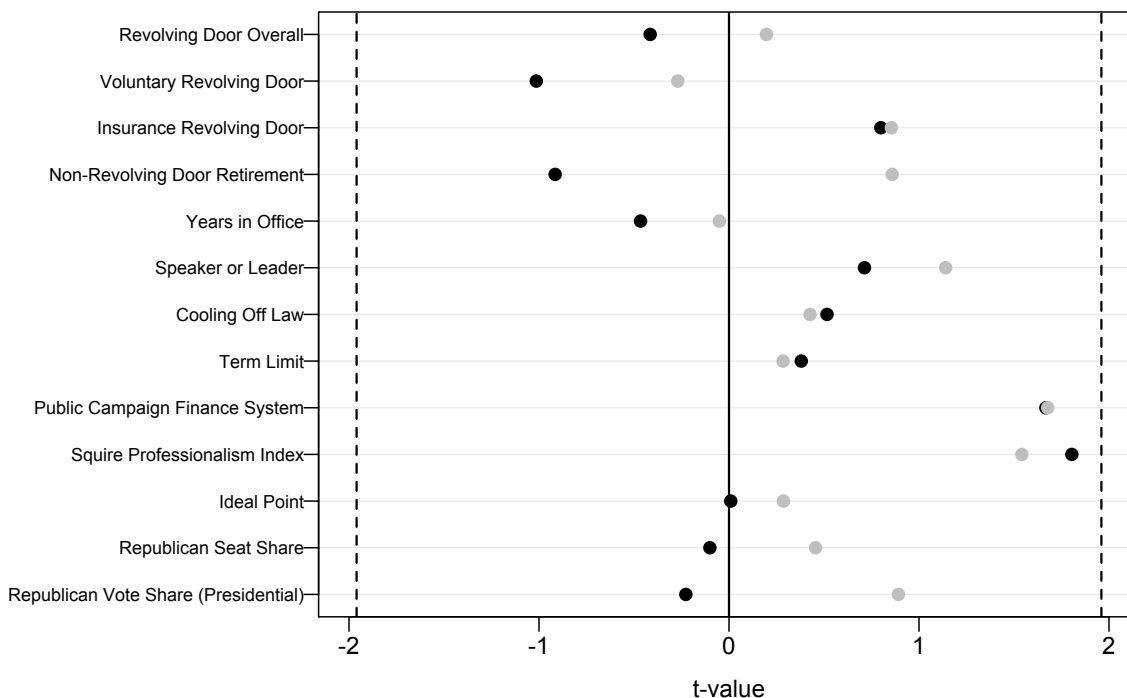


Figure A1: Balance Checks for Pre-*Citizens United* Periods: Values of Welch two sample t-test of difference in means between states affected by the Supreme Court ruling and states not affected. Values between -1.96 and 1.96 indicate that the difference is not significant at the 95 percent level. Black dots: 2006-07 election cycle. Gray dots: 2008-09 election cycle.

Another assumption of the standard difference-in-differences design is that of parallel trends. It has to be assumed that had *Citizens United* not occurred, the difference in the dependent variable between treatment and control states would have been the same as it was before the ruling. This is a relatively restrictive assumption that, of course, cannot be empirically verified. A common approach to relax it is to include a set of treatment unit-specific time trends (Angrist and Pischke, 2009), which in my case are the states. They absorb secular developments that trend differently and may affect the outcomes of interest. If there are such developments and the trends are not included, they are partially captured by β , thus leading to unreliably estimated treatment effects. I therefore follow the lead of studies of the direct effect of *Citizens United* (Klumpp, Mialon and Williams, 2016; Abdul-Razzak, Prato and Wolton, 2018; Petrova, Simonov and Snyder, 2019) and include them in all specifications.

A.4 Information on Time-Variant Controls

I include three time-variant controls at the state-level into the estimations. Here, I provide further information on their construction.

First, during the period of observation, several states introduced or abolished “cooling off” laws that impose waiting times on elected officials before they can become lobbyists. To determine the beginning and end dates of these policies, I used information on the laws provided by the *National Conference of State Legislatures* and researched their history.¹ Following the convention in the literature, I recorded the starting year of cooling off laws as the year they first affected legislators. Table A4 provides an overview of the data.

Table A4: States with Changes in Revolving Door Legislation. Years in which states introduced or abolished new cooling off legislation (1990-2012).

Arkansas	2011	Montana	2008
California	1991	North Carolina	2005
Colorado	2007	New Jersey	2004
Connecticut	1994	New York	2000
Georgia	2007	Ohio	1996-2010
Iowa	1992	Oregon	1998
Indiana	2012	South Carolina	1992
Kentucky	1993	Tennessee	2006
Massachusetts	1999	Utah	2009
Maine	1999	West Virginia	1999
Michigan	1995		

A second time-variant state-level control I include is whether a state had term limits. The data comes from the *National Conference of State Legislatures*.²

Finally, I control for whether the state had a public campaign finance system. A list of laws is provided by the *National Conference of State Legislatures*.³ I researched the history of these laws to determine their starting date and also conducted research on potential states with lapsed laws. Table A5 provides a list of states.

¹<http://www.ncsl.org/research/ethics/50-state-table-revolving-door-prohibitions.aspx>.

²Source: <http://www.ncsl.org/research/about-state-legislatures/chart-of-term-limits-states.aspx>.

³<http://www.ncsl.org/research/elections-and-campaigns/public-financing-of-campaigns-overview.aspx>.

Table A5: States with Changes in Public Campaign Finance. Years in which states introduced public campaign finance systems (1990-2012).

Arizona	2000	Maine	2000
Connecticut	2008		

A.5 Additional Analyses and Robustness Checks

Due to space constraints, the manuscript only presents the main results in detail and summarizes a number of additional analyses and robustness checks. In this section, I report their results in detail.

A.5.1 Permutation Tests

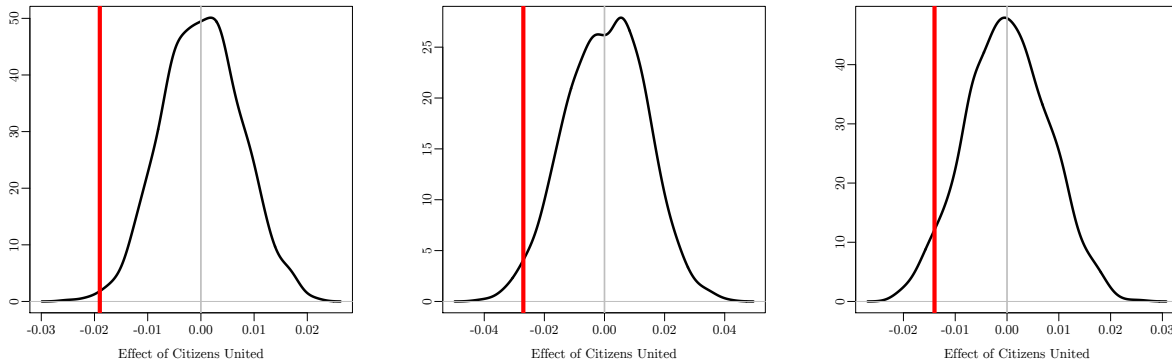
To make sure that the results are unlikely to have arisen by chance, I conduct permutation tests for all specifications I have presented in the article. To do so, I run 1000 regressions in which treatment status is randomly assigned to 21 states and record the estimated “treatment” effects. This provides the distribution of the treatment effect under the null hypothesis. The graphs below show the densities of those distributions as well as the actual treatment effect (red bar).

Figure A2 shows the permutation tests for Table 2 in the manuscript. The treatment effects for the overall effect are far in the tail of the permutation distributions, indicating a small probability that they occurred by chance.

Figure A3 shows the permutation tests for Table 3 in the manuscript. The treatment effect on the voluntary revolving door for reelection is in the margins of the null distribution. For the insurance revolving door for reelection, there was no significant treatment effect in Table 3. Consistent with this, the red bars are in the center of the null distribution.

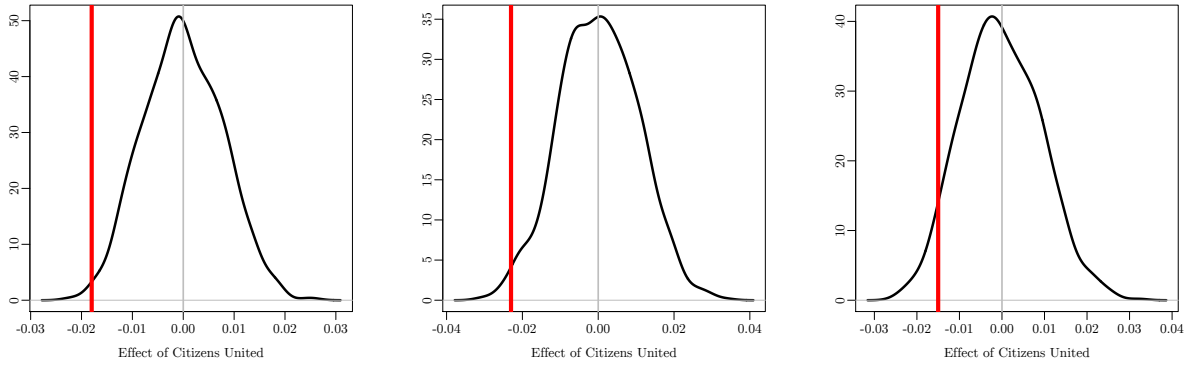
Figure A4 shows the permutation tests for Table 4 in the manuscript, which looks at non-revolving door retirement. There was no significant treatment effect for the pooled sample as well as Republicans, and the red bars are near the center of the distribution under the null. The positive treatment effect of *Citizens United* on Democrats is reflected by being relatively far in the right tail of the null distribution.

Figure A5 shows the permutation tests for Table 5 in the manuscript, which looks the effect of *Citizens United* on lobbying demand overall. For all three indicators there was no significant treatment effect. Consistent with this, the red bars are near the center of the null distributions.

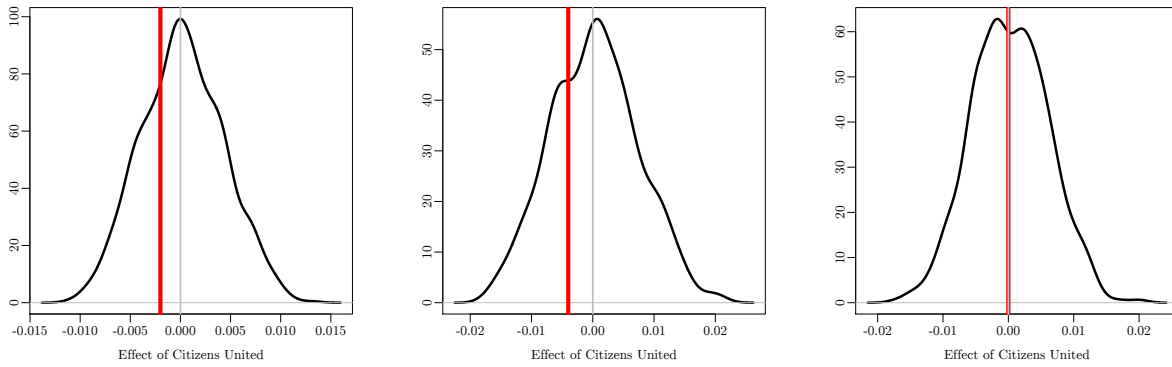


(a) Revolving Door Overall, All (0.004) (b) Revolving Door Overall, Republican (0.018) (c) Revolving Door Overall, Democrat (0.040)

Figure A2: Permutation test for Table 2 in manuscript. Density of effect sizes from 1000 simulations with 21 states randomly assigned treatment status. The red bar shows the treatment effect of *Citizens United* from Table 2 in the manuscript. The numbers in parentheses give the proportion of simulations that produced a more extreme negative coefficient than the actual treatment effect.

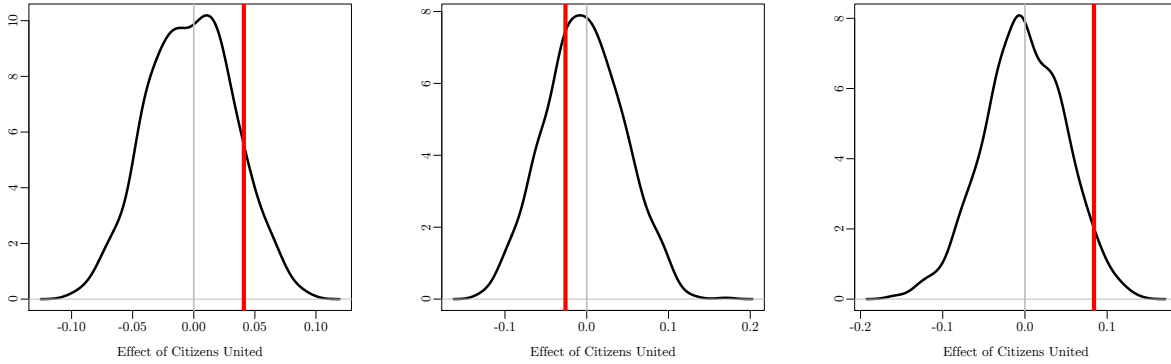


(a) Voluntary Revolving Door, All (0.005) (b) Voluntary Revolving Door, Republican (0.011) (c) Voluntary Revolving Door, Democrat (0.049)



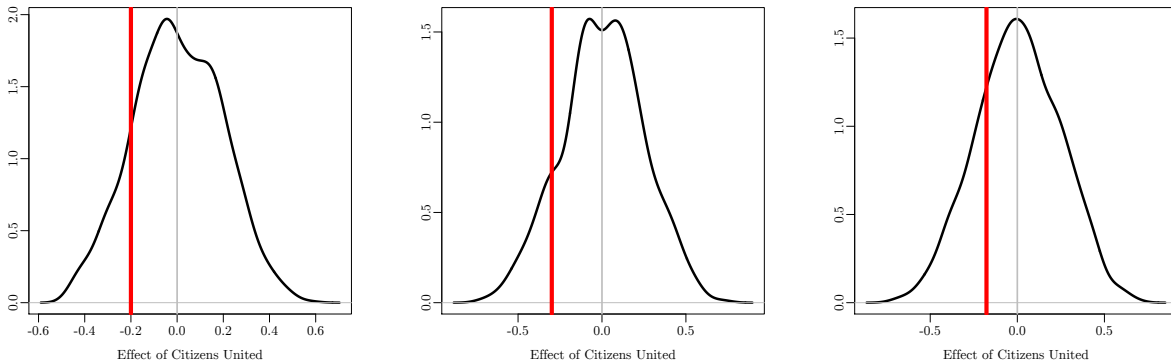
(d) Insurance Revolving Door, All (0.302) (e) Insurance Revolving Door, Republican (0.296) (f) Insurance Revolving Door, Democrat (0.492)

Figure A3: Permutation test for Table 3 in manuscript. Density of effect sizes from 1000 simulations with 21 states randomly assigned treatment status. The red bar shows the treatment effect of *Citizens United* from Table 3 in the manuscript. The numbers in parentheses give the proportion of simulations that produced a more extreme negative coefficient than the actual treatment effect.



(a) Non-Revolving Door Retirement, All (0.109) (b) Non-Revolving Door Retirement, Republican (0.321) (c) Non-Revolving Door Retirement, Democrat (0.042)

Figure A4: Permutation test for Table 4 in manuscript. Density of effect sizes from 1000 simulations with 21 states randomly assigned treatment status. The red bar shows the treatment effect of *Citizens United* from Table 4 in the manuscript. The numbers in parentheses give the proportion of simulations that produced a more extreme positive coefficient (for a and c) or more extreme negative effect (for b) than the actual treatment effect.



(a) Lobbyist Demand, Lobbyists (0.141) (b) Lobbyist Demand, Clients (0.120) (c) Lobbyist Demand, Lobbyist-Clients (0.229)

Figure A5: Permutation test for Table 5 in manuscript. Density of effect sizes from 1000 simulations with 21 states randomly assigned treatment status. The red bar shows the treatment effect of *Citizens United* from Table 5 in the manuscript. The numbers in parentheses give the proportion of simulations that produced a more extreme negative effect than the actual treatment effect.

A.5.2 Results Separating States with and without “Cooling Off” Laws

In the main specification, I have included a control variable for whether a state has a “cooling off” law preventing legislators from registering as lobbyists for a specified amount of time. Table A4 shows the states that have cooling off laws and the timing of their introduction. As a check, the following tables present estimations separately for states that do not have a cooling off law (Tables A6 to A8) and states that do (Tables A9 to A11). As one may expect, the results are driven by states free of restrictions in post-office employment in the lobbying sector. Because of the lower number of observations the standard errors are somewhat larger, but the point estimates are in fact bigger in magnitude than in the main specification.

Table A6: Table 2 from manuscript for states *without* cooling off laws only.

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.028** (0.012)	-0.043* (0.021)	-0.014 (0.012)
N	8,768	4,458	4,310

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Term Limit Law, Public Campaign Finance. Individual-level controls: Years in Office, Chamber Control Own Party, Speaker or Leader. Robust standard errors clustered by state in parentheses.

Table A7: Table 3 from manuscript for states *without* cooling off laws only.

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.026** (0.013)	-0.032* (0.016)	-0.019 (0.014)	-0.001 (0.007)	-0.011 (0.012)	0.005 (0.011)
N	8,768	4,458	4,310	8,768	4,458	4,310

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Term Limit Law, Public Campaign Finance. Individual-level controls: Years in Office, Chamber Control Own Party, Speaker or Leader. Robust standard errors clustered by state in parentheses.

Table A8: Table 4 from manuscript for states *without* cooling off laws only.

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	0.029 (0.037)	-0.019 (0.061)	0.039 (0.058)
N	8,768	4,458	4,310

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Term Limit Law, Public Campaign Finance. Individual-level controls: Years in Office, Chamber Control Own Party, Speaker or Leader. Robust standard errors clustered by state in parentheses.

Table A9: Table 2 from manuscript for states *with* cooling off laws only.

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.010 (0.007)	-0.009 (0.011)	-0.010 (0.008)
N	9,590	4,393	5,197

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Term Limit Law, Public Campaign Finance. Individual-level controls: Years in Office, Chamber Control Own Party, Speaker or Leader. Robust standard errors clustered by state in parentheses.

Table A10: Table 3 from manuscript for states *with* cooling off laws only.

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.009 (0.007)	-0.015 (0.009)	-0.004 (0.008)	-0.001 (0.004)	0.006 (0.006)	-0.006 (0.006)
N	9,590	4,393	5,197	9,590	4,393	5,197

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Term Limit Law, Public Campaign Finance. Individual-level controls: Years in Office, Chamber Control Own Party, Speaker or Leader. Robust standard errors clustered by state in parentheses.

Table A11: Table 4 from manuscript for states *with* cooling off laws only.

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	0.083 (0.053)	0.012 (0.061)	0.141** (0.069)
N	9,590	4,393	5,197

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Term Limit Law, Public Campaign Finance. Individual-level controls: Years in Office, Chamber Control Own Party, Speaker or Leader. Robust standard errors clustered by state in parentheses.

A.5.3 Discussion of Treatment Status Codings and Robustness to Alternative Codings

Table 1 of the manuscript provides a list of 21 states that were affected by the *Citizens United* ruling. The basis for this coding is a list provided by the *National Conference of State Legislatures*.⁴ Three states for which laws are mentioned are not coded as treated in my data. For Alabama, the law mentioned by the NCSL applies to referenda, but not to candidates. Accordingly, other studies of the impact of *Citizens United* at the state-level do not count Alabama as treated (see Spencer and Wood, 2014; La Raja and Schaffner, 2014; Werner and Coleman, 2015; Klumpp, Mialon and Williams, 2016; Abdul-Razzak, Prato and Wolton, 2018).

Montana did have a ban on independent corporate expenditures in place and was thus affected, but enforced it nevertheless until 2012, when the Supreme Court declared it unconstitutional in *American Tradition Partnership vs. Bullock*. As a consequence, some accounts do include Montana as treated, while others do not. I count the state as not treated in the main analysis, but present results of the models when including it as treated as a robustness checks below.

New Hampshire had a ban on union expenditure, but not on corporate spending (a previously existing ban was declared unconstitutional in 2000). Following the majority of studies on *Citizens United*, I do not include New Hampshire as a treated state (cf. Spencer and Wood, 2014; La Raja and Schaffner, 2014; Werner and Coleman, 2015). However, some contributions point out that New Hampshire did have a limit of \$5,000 per election cycle on corporate independent spending and therefore include it as a treated state (Klumpp, Mialon and Williams, 2016; Abdul-Razzak, Prato and Wolton, 2018). Note, however, that these studies do not include New York as treated, despite the fact that this state also had a law limiting corporate spending to \$5,000 (see La Raja and Schaffner, 2014).

Thus, there is no clear consensus in the literature about which states should be classified as affected by *Citizens United* and which ones should not. To ensure that my results are not driven by the contentious classification of the states, I re-estimate the models in Tables 2-4 using the following alternative coding schemes:

⁴<http://www.ncsl.org/research/elections-and-campaigns/citizens-united-and-the-states.aspx>, accessed November 10, 2017.

- Because the ban on independent corporate expenditures in Montana took effect in 2012, I include this state as treated.
- Even though New York and New Hampshire did not have a ban on independent corporate expenditures, they set a limit on them. I provide specifications in which they are included as treated states. However, this may be “overcorrecting” since a \$5,000 limit is potentially not that restrictive, especially in New Hampshire, one of the smallest states that has the largest lower house in the country (400 members). Alternatively, I therefore re-estimate the models dropping all observations from these two states.
- A few states that did not have any bans also reacted to *Citizens United* by introducing new laws, mostly related to reporting requirements. The only state in which such a law has been enacted is Washington. I provide a specification in which I drop this state from the analysis.
- Finally, Spencer and Wood (2014) provide a list that has the biggest differences to the list used by me. They include 20 states as treated, which differs from the one used in the manuscript by including Montana but by excluding North Dakota and Rhode Island. I re-estimate all models using their coding.

As the tables on the following pages show, the substantive findings remain intact for all alternative coding schemes.

Table A12: Table 2 from manuscript using alternative treatment status coding 1 (including Montana in treatment states).

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.020*** (0.006)	-0.026** (0.013)	-0.015** (0.007)
N	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A13: Table 3 from manuscript using alternative treatment status coding 1 (including Montana in treatment states).

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.018** (0.007)	-0.022** (0.009)	-0.015* (0.008)	-0.002 (0.004)	-0.004 (0.007)	-0.000 (0.006)
N	18,358	8,851	9,507	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A14: Table 4 from manuscript using alternative treatment status coding 1 (including Montana in treatment states).

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	0.032 (0.032)	-0.032 (0.043)	0.079* (0.046)
N	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A15: Table 2 from manuscript using alternative treatment status coding 2, version 1 (including New York and New Hampshire in treatment states).

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.016** (0.007)	-0.020 (0.012)	-0.012 (0.008)
N	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A16: Table 3 from manuscript using alternative treatment status coding 2, version 1 (including New York and New Hampshire in treatment states).

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.013* (0.007)	-0.015 (0.010)	-0.010 (0.009)	-0.003 (0.004)	-0.004 (0.007)	-0.001 (0.005)
N	18,358	8,851	9,507	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A17: Table 4 from manuscript using alternative treatment status coding 2, version 1 (including New York and New Hampshire in treatment states).

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	0.037 (0.036)	-0.033 (0.046)	0.113** (0.045)
N	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A18: Table 2 from manuscript using alternative treatment status coding 2, version 2 (dropping New York and New Hampshire from the analysis).

	Revolving Door		
	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	-0.019*** (0.007)	-0.025* (0.013)	-0.014* (0.008)
N	16,592	7,953	8,639

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A19: Table 3 from manuscript using alternative treatment status coding 2, version 2 (dropping New York and New Hampshire from the analysis).

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	-0.017** (0.007)	-0.021** (0.010)	-0.013 (0.009)	-0.002 (0.004)	-0.005 (0.007)	-0.001 (0.006)
N	16,592	7,953	8,639	16,592	7,953	8,639

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A20: Table 4 from manuscript using alternative treatment status coding 2, version 2 (dropping New York and New Hampshire from the analysis).

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	0.043 (0.037)	-0.028 (0.047)	0.110** (0.047)
N	16,592	7,953	8,639

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A21: Table 2 from manuscript using alternative treatment status coding 3 (dropping Washington from analysis).

	Revolving Door		
	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	-0.018*** (0.006)	-0.025* (0.013)	-0.013* (0.007)
N	17,924	8,660	9,264

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A22: Table 3 from manuscript using alternative treatment status coding 3 (dropping Washington from analysis).

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	-0.018** (0.007)	-0.024** (0.010)	-0.014 (0.009)	0.000 (0.004)	-0.002 (0.006)	0.001 (0.006)
N	17,924	8,660	9,264	17,924	8,660	9,264

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A23: Table 4 from manuscript using alternative treatment status coding 3 (dropping Washington from analysis).

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	0.039 (0.033)	-0.031 (0.044)	0.082* (0.047)
N	17,924	8,660	9,264

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A24: Table 2 from manuscript using alternative treatment status coding 4 (using the coding of treatment states according to Spencer and Wood (2014)).

	Revolving Door		
	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	-0.019*** (0.007)	-0.026** (0.013)	-0.014* (0.007)
N	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A25: Table 3 from manuscript using alternative treatment status coding 4 (using the coding of treatment states according to Spencer and Wood (2014)).

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
$Ban_s \times Post-CU_t$	-0.019*** (0.007)	-0.023** (0.010)	-0.017** (0.008)	0.000 (0.004)	-0.004 (0.007)	0.003 (0.006)
N	18,358	8,851	9,507	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A26: Table 4 from manuscript using alternative treatment status coding 4 (using the coding of treatment states according to Spencer and Wood (2014)).

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	0.021 (0.032)	-0.019 (0.043)	0.051 (0.046)
N	18,358	8,851	9,507

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

A.5.4 Including Legislators Not at the End of their Terms

In the manuscript, I only analyze the career decisions of legislators at the end of their terms, that is those who have to run for reelection if they want to stay in office. As a robustness check, I re-estimate the models for Tables 2-4 including legislators who are not up for reelection. The substantive findings remain unaffected, with the exception of the increase of non-revolving door retirement among Democrats. This is not unexpected, as *Citizens United* should have no impact on politicians' decision to retire when they are *not* at the end of their term.

Table A27: Table 2 from manuscript when including legislators *not* at the end of their terms.

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.019*** (0.006)	-0.028** (0.012)	-0.013** (0.006)
N	19,358	9,317	10,041

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A28: Table 3 from manuscript when including legislators *not* at the end of their terms.

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.015** (0.007)	-0.021** (0.009)	-0.011 (0.008)	-0.004 (0.004)	-0.007 (0.007)	-0.002 (0.006)
N	19,358	9,317	10,041	19,358	9,317	10,041

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A29: Table 4 from manuscript when including legislators *not* at the end of their terms.

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.025 (0.043)	-0.070 (0.045)	0.007 (0.055)
N	19,358	9,317	10,041

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

A.5.5 Term-Limited Legislators Only

About a handful of states have term limits, barring legislators from running for reelection after between 8 and 16 years, depending on the state.⁵ For term-limited legislators, the cost-benefit calculation in Equation (1) of the manuscript no longer applies. As a placebo test, I estimate the models for Tables 2 and 4 only with legislators that are term-limited (I do not estimate the models from Table 3, as there is no “insurance” revolving door for term-limited legislators). As expected, *Citizens United* did not have a significant effect on the career paths of term-limited legislators.

Table A30: Table 2 from manuscript with term-limited legislators only.

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.075 (0.119)	-0.087 (0.140)	-0.154 (0.207)
N	540	283	257

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A31: Table 4 from manuscript with term-limited legislators only.

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	0.102 (0.146)	0.218 (0.174)	0.149 (0.248)
N	540	283	257

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

⁵See <http://www.ncsl.org/research/about-state-legislatures/chart-of-term-limits-states.aspx>.

B Analyses for State Senates

B.1 The Revolving Door in State Senates

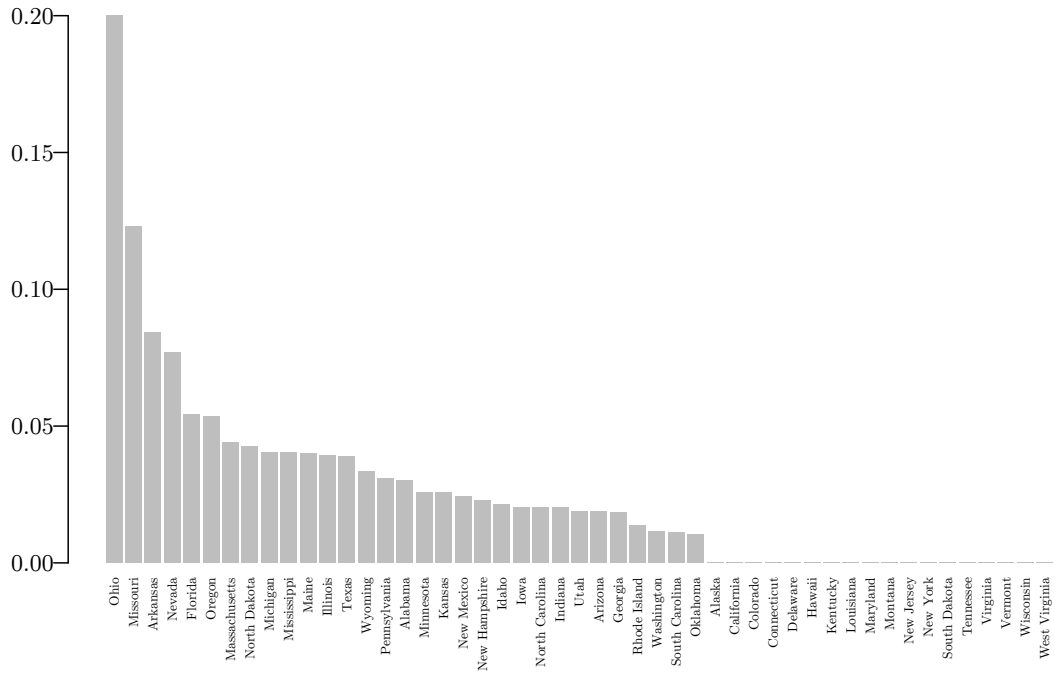


Figure A6: Share of Representatives going through Revolving Door, Senates. Proportion of legislators per election cycle that leave office and are registered as lobbyists in the same or following year.

B.2 Descriptive Statistics

Table A32: Descriptive Statistics, Senate, All.

	Mean	Std. Dev.	Min	Max
Revolving Door Overall	0.023	0.150	0.000	1.000
Voluntary Revolving Door	0.018	0.130	0.000	1.000
Insurance Revolving Door	0.005	0.070	0.000	1.000
Non-Revolving Door Retirement	0.257	0.440	0.000	1.000
Ban _s × Post-CU _t	0.229	0.420	0.000	1.000
Cooling Off Law	0.573	0.490	0.000	1.000
Term Limit	0.236	0.420	0.000	1.000
Public Campaign Finance	0.127	0.330	0.000	1.000
Years in Office	9.280	7.010	1.000	42.000
Chamber Control Own Party	0.601	0.490	0.000	1.000
Speaker or Leader	0.000	0.020	0.000	1.000

Table A33: Descriptive Statistics, Senate, Republicans.

	Mean	Std. Dev.	Min	Max
Revolving Door Overall	0.029	0.170	0.000	1.000
Voluntary Revolving Door	0.022	0.150	0.000	1.000
Insurance Revolving Door	0.006	0.080	0.000	1.000
Non-Revolving Door Retirement	0.274	0.450	0.000	1.000
Ban _s × Post-CU _t	0.222	0.420	0.000	1.000
Cooling Off Law	0.568	0.500	0.000	1.000
Term Limit	0.262	0.440	0.000	1.000
Public Campaign Finance	0.129	0.340	0.000	1.000
Years in Office	8.782	6.660	1.000	42.000
Chamber Control Own Party	0.621	0.490	0.000	1.000
Speaker or Leader	0.000	0.020	0.000	1.000

Table A34: Descriptive Statistics, Senate, Democrats.

	Mean	Std. Dev.	Min	Max
Revolving Door Overall	0.018	0.130	0.000	1.000
Voluntary Revolving Door	0.014	0.120	0.000	1.000
Insurance Revolving Door	0.004	0.060	0.000	1.000
Non-Revolving Door Retirement	0.241	0.430	0.000	1.000
Ban _s × Post-CU _t	0.236	0.420	0.000	1.000
Cooling Off Law	0.578	0.490	0.000	1.000
Term Limit	0.211	0.410	0.000	1.000
Public Campaign Finance	0.126	0.330	0.000	1.000
Years in Office	9.757	7.300	1.000	40.000
Chamber Control Own Party	0.581	0.490	0.000	1.000
Speaker or Leader	0.000	0.020	0.000	1.000

B.3 Analyses

Table A35 shows the results of a difference-in-differences estimation of the effect of *Citizens United* on the revolving door in the state upper chambers. In their analysis of the electoral consequences of the supreme court ruling, Klumpp, Mialon and Williams (2016) do not find any evidence that the ruling had an effect on the senates. In addition, Figure A6 above has shown that in many states, the revolving door in upper chambers is uncommon or even non-existent. Consistent with this, there is no treatment effect of *Citizens United* on the revolving door. All three coefficients are positive and quite far away from statistical significance. The same is true when splitting up the effect between politicians who go through the revolving door not having run for office and those that run for office and take up a lobbying position after having lost (see Table A36). Table A37 shows that *Citizens United* also did not have a significant effect on non-revolving door retirement in the state senates.

Table A35: Table 2 from manuscript for states senates.

	Revolving Door		
	All	Republicans	Democrats
Ban _s × Post-CU _t	0.021 (0.022)	0.036 (0.029)	0.017 (0.031)
N	4,769	2,332	2,437

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A36: Table 3 from manuscript for states senates.

	Voluntary Revolving Door			Insurance Revolving Door		
	All	Republicans	Democrats	All	Republicans	Democrats
Ban _s × Post-CU _t	0.015 (0.019)	0.026 (0.022)	0.011 (0.023)	0.006 (0.011)	0.010 (0.019)	0.006 (0.017)
N	4,769	2,332	2,437	4,769	2,332	2,437

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

Table A37: Table 4 from manuscript for states senates.

	Non-Revolving Door Retirement		
	All	Republicans	Democrats
Ban _s × Post-CU _t	-0.092 (0.087)	-0.195 (0.119)	0.015 (0.105)
N	4,769	2,332	2,437

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions include state-level and individual-level controls, state and year fixed effects, and state-specific time trends. Robust standard errors clustered by state in parentheses.

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