A Internet Appendix

A.1 Auxiliary tables and figures

- Auxiliary figures:
  - Figure IA.1 shows differences in saving, saving-income sensitivity, leverage, payout, and SEO activity measures between firms with and without lines of credit headquartered in election and non-election states for nine quarters around elections.
  - Figure IA.2 shows differences in debt raised and retired and SEO activity measures between firms with high and low equity-focused constraints according to Hoberg and Maksimovic (2015) headquartered in election and non-election states for nine quarters around elections.
  - Figure IA.3 shows differences in debt raised and retired and SEO activity measures between firms with high and low debt-focused constraints according to Hoberg and Maksimovic (2015) headquartered in election and non-election states for nine quarters around elections.
- Expanded results:
  - Table IA.1 provides coefficient estimates on controls for the regressions reported in Table 2.
- Tables with coefficient estimates presented in Figures 2 and 4 in the main text and IA.1:
  - Table IA.2 reports saving-income sensitivity results in subsamples.
  - Table IA.3 reports leverage results in subsamples.
  - Table IA.4 reports SEO incidence results in subsamples.
  - Table IA.5 reports SEO size results in subsamples.
- Auxiliary repurchases, dividends, debt raised, and debt retired results for the bond rating and lines of credit subsamples and coefficients plotted in Figure 3:
  - Table IA.6 provides coefficient estimates for repurchases regressions in the whole sample and subsamples.
  - Table IA.7 provides coefficient estimates for dividend regressions in the whole sample and subsamples.
  - Table IA.8 provides coefficient estimates for debt raised and debt retired regressions in subsamples.
- Auxiliary results:
  - Table IA.9 presents summary statistics for cash burn rates calculated according to Denis and McKeon (2021).
  - Table IA.10 re-estimates the main cash holdings results for subsamples of seasonal and non-seasonal firms as Fairhurst (2020) defines.

Figure IA.1: Firms with and without lines of credit subsample results. Coefficient estimates for regressions with the dependent variables given in panel titles for firms with and without lines of credit (blue solid and green dashed lines, respectively). Shaded areas are 95% confidence intervals. All regression specifications are described in the caption of Figure 2.

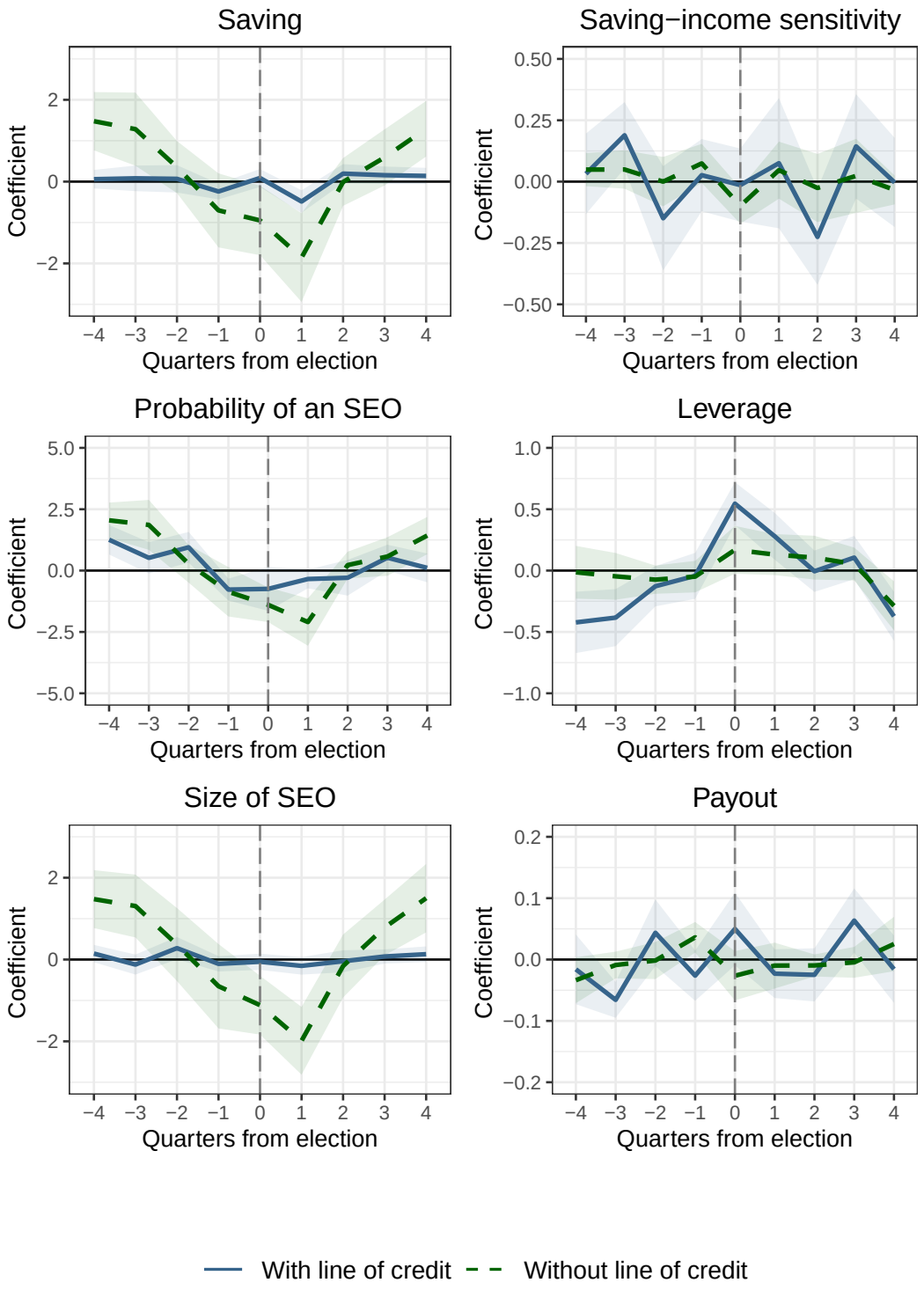

Figure IA.2: Firms with high and low equity-focused constraints. Coefficient estimates for regressions with the dependent variables given in panel titles for firms with high and low equity-focused constraints as Hoberg and Maksimovic (2015) define (blue dashed and purple solid lines, respectively). All regression specifications are described in the caption of Figure 2. The Hobert-Maksimovic data are only available from 1997 to 2015, so these tests include data from only these years.

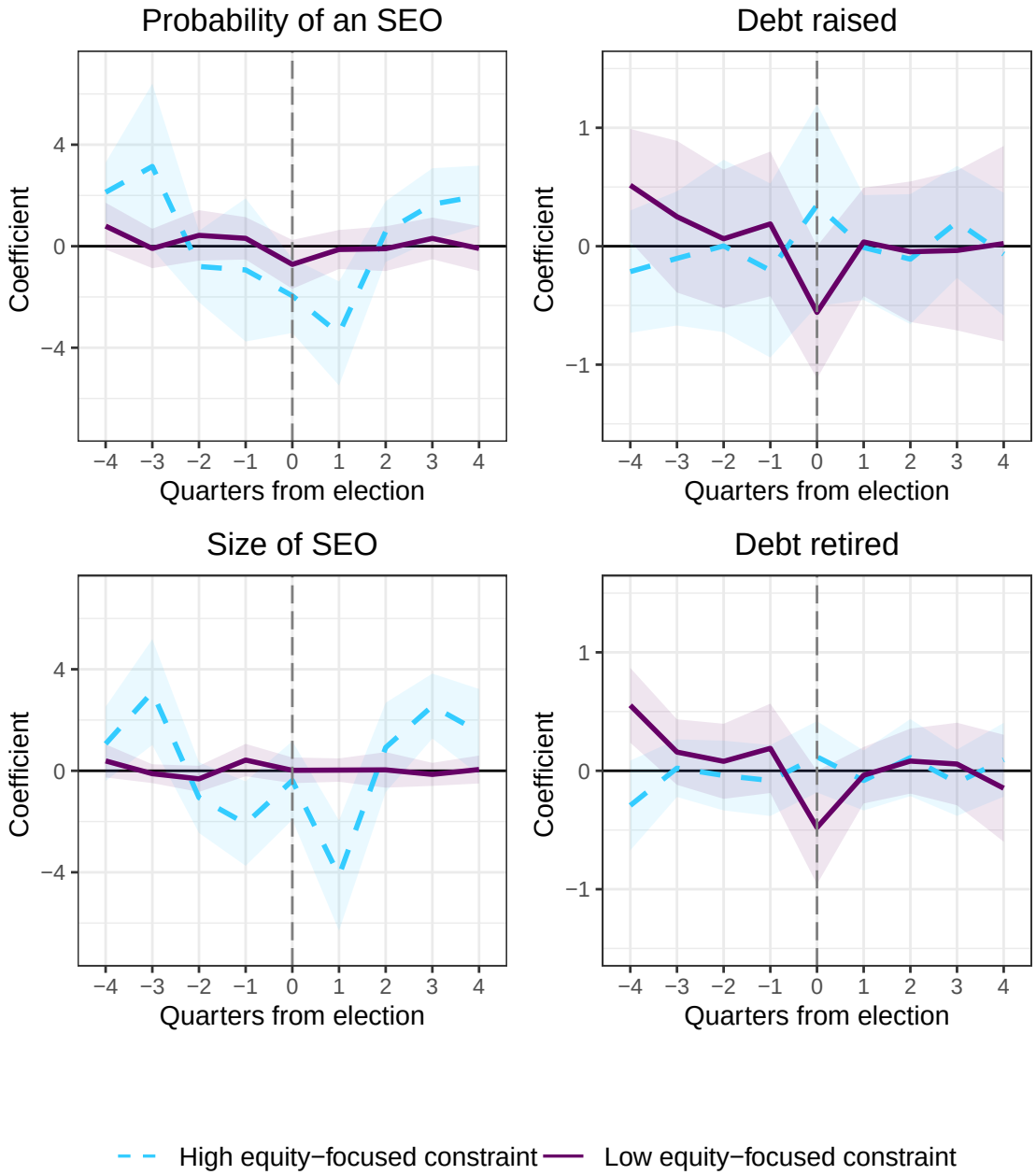

Figure IA.3: Firms with high and low debt-focused constraints. Coefficient estimates for regressions with the dependent variables given in panel titles for firms with high and low debt-focused constraints as Hoberg and Maksimovic (2015) define (grey dashed and pink solid lines, respectively). All regression specifications are described in the caption of Figure 2. The Hobert-Maksimovic data are only available from 1997 to 2015, so these tests include data from only these years.

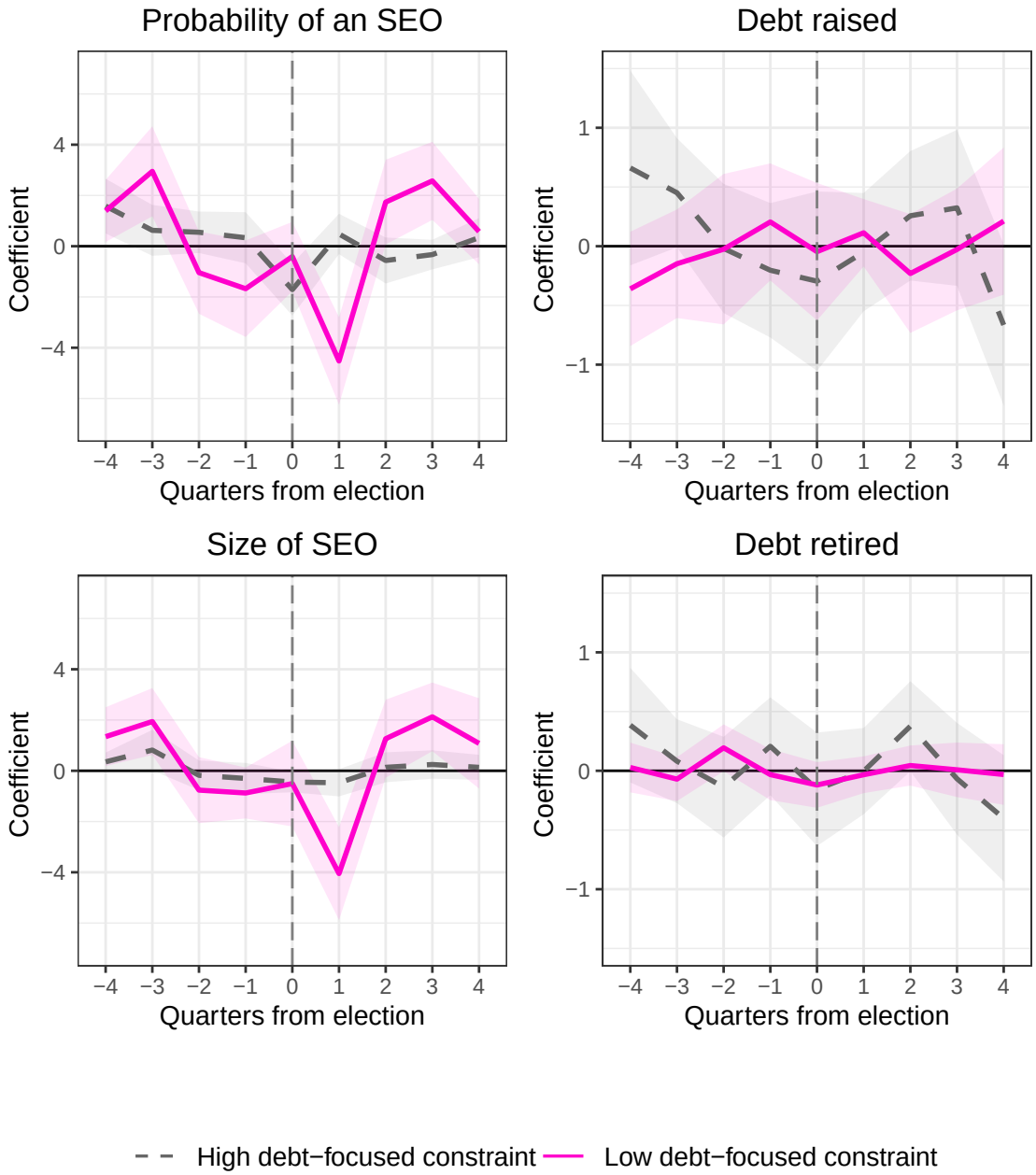

Table IA.1: Expanded table for cash holdings regression results. This table reports regression coefficients for regressions with quarterly cash holdings (cash scaled by total assets) multiplied by 100 as a dependent variable (summarized in Table 2). The estimation is described in the caption of Table 2.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Quarter interaction	0.377***	0.129*	0.206***	−0.025	−0.308***	−0.355***	−0.196***	0.176***	0.368***
	(0.109)	(0.066)	(0.074)	(0.062)	(0.112)	(0.099)	(0.059)	(0.065)	(0.089)
yearbefore	−0.219	−0.061	−0.061	−0.061	−0.062	0.011	0.012	0.012	0.011
	(0.135)	(0.124)	(0.124)	(0.124)	(0.124)	(0.122)	(0.122)	(0.122)	(0.122)
election	−0.144*	−0.033	−0.053	0.005	0.076	−0.186**	−0.186**	−0.185**	−0.187**
	(0.073)	(0.087)	(0.082)	(0.092)	(0.104)	(0.079)	(0.078)	(0.078)	(0.079)
yearafter	−0.124	−0.015	−0.015	−0.015	−0.016	0.040	0.002	−0.091	−0.142
	(0.128)	(0.097)	(0.097)	(0.097)	(0.097)	(0.116)	(0.110)	(0.098)	(0.107)
pres. election year	0.300	−0.141	−0.067	0.055	0.110	−0.689**	−0.677**	−0.727**	−0.877**
	(0.389)	(0.325)	(0.319)	(0.320)	(0.316)	(0.319)	(0.329)	(0.331)	(0.333)
pres. election year	0.316***	0.529***	0.217***	−0.276***	−0.466***	−0.209***	−0.256***	−0.056	0.520***
quarter interaction	(0.089)	(0.132)	(0.047)	(0.054)	(0.128)	(0.070)	(0.069)	(0.055)	(0.093)
pres. election year ×	−0.355	−0.467**	−0.416**	−0.607***	−0.621***	0.288	0.318	0.178	0.133
election.	(0.350)	(0.208)	(0.175)	(0.225)	(0.228)	(0.196)	(0.207)	(0.209)	(0.202)
pres. election year ×	0.653***	−0.247	−0.443*	0.320	0.368*	−0.235	−0.351***	0.207	0.381***
election × quarter int.	(0.200)	(0.207)	(0.236)	(0.267)	(0.219)	(0.176)	(0.077)	(0.165)	(0.072)
Q1	0.138***	−0.021	0.159***	0.138***	0.139***	0.284***	0.158***	0.138***	0.137***
	(0.036)	(0.057)	(0.055)	(0.036)	(0.036)	(0.050)	(0.055)	(0.036)	(0.036)
Q2	–	–	−0.076*	–	–	–	0.140**	–	–
			(0.040)				(0.057)		
Q3	−0.021	−0.021	–	0.047	−0.021	−0.021	–	−0.056	−0.020
	(0.045)	(0.045)		(0.043)	(0.045)	(0.045)		(0.058)	(0.045)
Q4	0.288***	0.463***	0.485***	0.464***	0.649***	0.463***	0.484***	0.464***	0.233***
	(0.062)	(0.034)	(0.043)	(0.034)	(0.055)	(0.034)	(0.043)	(0.034)	(0.057)

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recession	−0.687***	−0.659***	−0.681***	−0.674***	−0.695***	−0.647***	−0.655***	−0.681***	−0.626***
	(0.085)	(0.079)	(0.082)	(0.077)	(0.084)	(0.078)	(0.083)	(0.078)	(0.083)
unemp	−0.054	−0.058	−0.058	−0.059	−0.058	−0.056	−0.055	−0.054	−0.057
	(0.057)	(0.057)	(0.056)	(0.057)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)
leverage	−0.315***	−0.315***	−0.315***	−0.315***	−0.315***	−0.315***	−0.315***	−0.315***	−0.315***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
size	−1.804***	−1.804***	−1.803***	−1.803***	−1.804***	−1.804***	−1.803***	−1.803***	−1.804***
	(0.158)	(0.158)	(0.158)	(0.158)	(0.158)	(0.158)	(0.158)	(0.158)	(0.158)
capital expenditures	0.012	0.012	0.012	0.012	0.012	0.012	0.012	0.012	0.012
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
market-to-book	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
ROA	−0.011***	−0.011***	−0.011***	−0.011***	−0.011***	−0.011***	−0.011***	−0.011***	−0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
working capital	−0.351***	−0.351***	−0.351***	−0.351***	−0.351***	−0.351***	−0.351***	−0.351***	−0.351***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Observations	319,093	319,093	319,093	319,093	319,093	319,093	319,093	319,093	319,093
R^2	0.798	0.798	0.798	0.798	0.798	0.798	0.798	0.798	0.798

Note: *p<0.1; **p<0.05; ***p<0.01

Table IA.2: Saving-income sensitivity regression results in subsamples. Regressions of quarterly saving (change in cash scaled by total assets) multiplied by 100 using the regression framework specified in the caption of Table 3 in subsamples detailed by the italicized headers. The regression augments the saving regression with three interactions: the included quarter dummy and ROA, the included election-cycle indicator (*yearbefore*, *election*, or *yearafter*) and ROA, and a triple interaction between the quarter dummy, election-cycle indicator, and ROA. Firms with high and low Hadlock-Pierce index measures (Panel A) are in the biggest and smallest tercile, respectively. Standard errors are clustered at the state-level.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures									
low Hadlock-Pierce	0.026	−0.007	−0.072	0.064	0.013	0.034	−0.098	0.005	0.051
	(0.069)	(0.072)	(0.078)	(0.052)	(0.068)	(0.074)	(0.063)	(0.047)	(0.072)
high Hadlock-Pierce	0.089*	0.074	−0.019	0.084	−0.145*	0.009	0.017	0.008	−0.048
	(0.050)	(0.046)	(0.041)	(0.055)	(0.074)	(0.055)	(0.049)	(0.061)	(0.037)
Panel B: Firms with and without bond ratings									
With bond rating	0.229	−0.082	0.140	0.063	−0.091	0.139*	0.017	−0.027	−0.093**
	(0.146)	(0.156)	(0.089)	(0.105)	(0.105)	(0.075)	(0.100)	(0.074)	(0.040)
Without bond rating	0.074*	0.049	0.031	0.076**	−0.125***	0.052	−0.060	0.053	−0.035
	(0.043)	(0.043)	(0.047)	(0.037)	(0.039)	(0.062)	(0.071)	(0.071)	(0.029)
Panel C: Firms with and without lines of credit									
With line of credit	0.032	0.189***	−0.149	0.026	−0.014	0.075	−0.225**	0.144	−0.004
	(0.084)	(0.069)	(0.108)	(0.075)	(0.076)	(0.135)	(0.099)	(0.108)	(0.093)
Without line of credit –	−0.206*	−0.100	−0.179	0.230	0.086	0.253**	−0.044	−0.290***	−0.036
firms in Dealscan only	(0.115)	(0.154)	(0.231)	(0.143)	(0.118)	(0.121)	(0.138)	(0.098)	(0.102)
Without line of credit –	0.049	0.049	−0.000	0.074*	−0.099***	0.047	−0.026	0.023	−0.034
all firms	(0.034)	(0.040)	(0.051)	(0.041)	(0.037)	(0.059)	(0.071)	(0.077)	(0.030)

Note: *p<0.1; **p<0.05; ***p<0.01

Table IA.3: Leverage regression results in subsamples. Regressions of quarterly leverage (the sum of long-term and short-term debt scaled by total assets) multiplied by 100 using the regression framework specified in the caption of Table 3 in subsamples detailed by the italicized headers. Only the coefficients estimated for *quarter interaction* are presented. Controls are given in the caption of Figure 2. Firms with high and low Hadlock-Pierce index measures (Panel A) are in the biggest and smallest tercile, respectively. Standard errors are clustered at the state-level.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures									
low Hadlock-Pierce	−0.179*	−0.190*	−0.110	0.077	0.253**	0.064	−0.091	0.096	−0.073
	(0.103)	(0.097)	(0.074)	(0.071)	(0.102)	(0.085)	(0.065)	(0.084)	(0.079)
high Hadlock-Pierce	0.009	−0.443*	−0.057	0.100	0.443**	0.460**	0.214	−0.048	−0.675***
	(0.270)	(0.228)	(0.144)	(0.152)	(0.211)	(0.183)	(0.131)	(0.161)	(0.172)
Panel B: Firms with and without bond ratings									
With bond rating	−0.113	−0.259	−0.095	0.088	0.253*	0.004	0.100	0.057	−0.153
	(0.152)	(0.168)	(0.092)	(0.129)	(0.153)	(0.134)	(0.102)	(0.108)	(0.125)
Without bond rating	−0.205**	−0.137	−0.219***	−0.016	0.364***	0.177*	0.094	0.072	−0.336***
	(0.087)	(0.118)	(0.058)	(0.078)	(0.096)	(0.101)	(0.065)	(0.074)	(0.111)
Panel C: Firms with and without lines of credit									
With line of credit	−0.394***	−0.361***	−0.158*	0.017	0.493***	0.249***	0.007	0.060	−0.308***
	(0.121)	(0.132)	(0.084)	(0.101)	(0.099)	(0.092)	(0.097)	(0.095)	(0.104)
Without line of credit –	−0.232*	−0.288***	−0.089	0.039	0.320**	0.140	0.061	0.136	−0.324**
firms in Dealscan only	(0.119)	(0.111)	(0.074)	(0.088)	(0.147)	(0.126)	(0.076)	(0.104)	(0.129)
Without line of credit –	−0.037	−0.162*	−0.071	−0.018	0.242**	0.212**	0.133*	0.050	−0.385***
all firms	(0.105)	(0.090)	(0.062)	(0.064)	(0.096)	(0.087)	(0.080)	(0.065)	(0.099)

Note: *p<0.1; **p<0.05; ***p<0.01

Table IA.4: SEO regression results in subsamples. Regressions of a binary variable equal to one if a firm had an SEO in a quarter using the regression framework specified in the caption of Table 3 for Panel C in subsamples detailed by the italicized headers. Firms with high and low Hadlock-Pierce index measures (Panel A) are in the biggest and smallest tercile, respectively. We define an SEO as occuring if sale of stock as a percentage of assets is greater than 5%. For ease of interpretation, the dependent variable is multiplied by 100. Only the coefficients estimated for *quarter interaction* are presented. Included firm controls are: lagged log of total assets, lagged market-to-book ratio, lagged leverage, working capital, ROA, and capital expenditures. Firm control variables are defined in the caption of Table 1. Included macroeconomic controls are: *recession* and *unemp*. Macroeconomic controls are also defined in the caption of Table 1. Year, firm, and quarter indicator variables are included in each estimation, along with a binary variable equal to one if a year is a presidential election year and interactions between the presidential election variable and quarter variables. Standard errors are clustered at the state-level.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures									
low Hadlock-Pierce	0.219	0.188	0.265	−0.307*	−0.205	−0.026	0.155	−0.158	0.023
	(0.170)	(0.143)	(0.208)	(0.158)	(0.182)	(0.180)	(0.220)	(0.195)	(0.217)
high Hadlock-Pierce	2.167***	1.624**	0.313	−0.955	−1.226**	−2.201***	−0.256	1.099*	1.685**
	(0.559)	(0.747)	(0.554)	(0.794)	(0.509)	(0.613)	(0.575)	(0.625)	(0.656)
Panel B: Firms with and without bond ratings									
With bond rating	1.018***	0.546	0.475	−0.380	−0.671***	−0.380	0.382	0.073	−0.089
	(0.299)	(0.352)	(0.445)	(0.314)	(0.228)	(0.392)	(0.400)	(0.333)	(0.333)
Without bond rating	1.851***	1.757***	0.489	−1.308***	−1.043***	−1.831***	−0.208	0.873***	1.276***
	(0.338)	(0.418)	(0.334)	(0.488)	(0.358)	(0.438)	(0.233)	(0.332)	(0.356)
Panel C: Firms with and without lines of credit									
With line of credit	1.257***	0.519	0.949***	−0.775***	−0.747	−0.343*	−0.293	0.525*	0.110
	(0.307)	(0.325)	(0.320)	(0.227)	(0.456)	(0.194)	(0.371)	(0.270)	(0.299)
Without line of credit –	1.540***	1.693***	−0.442	−0.446	−0.877**	−1.673***	0.413	−0.096	1.459***
firms in Dealscan only	(0.367)	(0.395)	(0.368)	(0.299)	(0.403)	(0.503)	(0.362)	(0.356)	(0.399)
Without line of credit –	2.047***	1.862***	0.274	−0.865*	−1.395***	−2.097***	0.224	0.573	1.427***
all firms	(0.369)	(0.519)	(0.391)	(0.514)	(0.360)	(0.493)	(0.279)	(0.402)	(0.387)

Note: *p<0.1; **p<0.05; ***p<0.01.

Table IA.5: Size of SEO regression results in subsamples. Regressions of sale of stock as a percentage of total assets if that percentage is greater than 5% and zero otherwise using the regression framework specified in the caption of Table 3 for Panel C in subsamples detailed by the italicized headers. Firms with high and low Hadlock-Pierce index measures (Panel A) are in the biggest and smallest tercile, respectively. For ease of interpretation, the dependent variable is multiplied by 100. Only the coefficients estimated for *quarter interaction* are presented. Included firm controls are: lagged log of total assets, lagged market-to-book ratio, lagged leverage, working capital, ROA, and capital expenditures. Firm control variables are defined in the caption of Table 1. Included macroeconomic controls are: *recession* and *unemp*. Macroeconomic controls are also defined in the caption of Table 1. Year, firm, and quarter indicator variables are included in each estimation, along with a binary variable equal to one if a year is a presidential election year and interactions between the presidential election variable and quarter variables. Standard errors are clustered at the state-level.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures									
low Hadlock-Pierce	0.034	−0.003	−0.002	0.004	0.003	0.022	0.045	−0.094**	0.022
	(0.037)	(0.033)	(0.063)	(0.044)	(0.042)	(0.048)	(0.053)	(0.045)	(0.041)
high Hadlock-Pierce	1.199*	1.220*	0.238	−1.012	−0.634	−2.538***	−0.357	1.264**	2.013***
	(0.618)	(0.632)	(0.636)	(0.668)	(0.589)	(0.731)	(0.714)	(0.586)	(0.669)
Panel B: Firms with and without bond ratings									
With bond rating	0.267***	−0.009	0.080	0.065	−0.139*	−0.022	0.112	−0.062	−0.036
	(0.101)	(0.067)	(0.105)	(0.071)	(0.076)	(0.105)	(0.112)	(0.075)	(0.065)
Without bond rating	1.084***	1.082***	0.267	−0.707*	−0.703***	−1.677***	−0.194	0.726***	1.254***
	(0.293)	(0.354)	(0.310)	(0.422)	(0.211)	(0.430)	(0.352)	(0.266)	(0.353)
Panel C: Firms with and without lines of credit									
With line of credit	0.146	−0.123	0.279**	−0.105	−0.055	−0.155	−0.042	0.074	0.131
	(0.108)	(0.125)	(0.139)	(0.095)	(0.101)	(0.109)	(0.137)	(0.088)	(0.097)
Without line of credit –	1.010***	1.002***	−0.169	−0.381	−0.499*	−1.119***	−0.012	0.350	0.855***
firms in Dealscan only	(0.314)	(0.250)	(0.256)	(0.234)	(0.277)	(0.331)	(0.309)	(0.269)	(0.268)
Without line of credit –	1.477***	1.308***	0.367	−0.653	−1.115***	−1.987***	−0.158	0.784**	1.500***
all firms	(0.361)	(0.392)	(0.456)	(0.527)	(0.367)	(0.423)	(0.393)	(0.348)	(0.427)

Note: *p<0.1; **p<0.05; ***p<0.01.

Table IA.6: Repurchases regression results in subsamples. Regressions of repurchases as a percentage of total assets using the regression framework specified in the caption of Table 3 for Panel C in the whole sample (Panel A) and subsamples (Panels B through D) detailed by the italicized headers. Firms with high and low Hadlock-Pierce index measures (Panel B) are in the biggest and smallest tercile, respectively. For ease of interpretation, the dependent variable is multiplied by 100. Only the coefficients estimated for *quarter interaction* are presented. Included firm controls are: lagged log of total assets, lagged market-to-book ratio, lagged leverage, working capital, ROA, and capital expenditures. Firm control variables are defined in the caption of Table 1. Included macroeconomic controls are: *recession* and *unemp*. Macroeconomic controls are also defined in the caption of Table 1. Year, firm, and quarter indicator variables are included in each estimation, along with a binary variable equal to one if a year is a presidential election year and interactions between the presidential election variable and quarter variables. Standard errors are clustered at the state-level.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Repurchases as a dependent variable – Whole sample									
Quarter interaction	−0.005	−0.028***	0.018*	0.016*	−0.006	−0.007	−0.025***	0.013	0.020
	(0.015)	(0.009)	(0.010)	(0.009)	(0.014)	(0.009)	(0.009)	(0.011)	(0.016)
Observations	287,036	287,036	287,036	287,036	287,036	287,036	287,036	287,036	287,036
R^2	0.163	0.163	0.163	0.163	0.163	0.163	0.163	0.163	0.163
Panel B: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures									
low Hadlock-Pierce	−0.026	−0.042*	0.026	−0.001	0.022	−0.044**	−0.009	0.015	0.048
	(0.025)	(0.023)	(0.019)	(0.020)	(0.028)	(0.022)	(0.018)	(0.021)	(0.031)
high Hadlock-Pierce	−0.017	−0.010	−0.008	0.014	0.006	0.015	0.006	−0.007	−0.017
	(0.015)	(0.012)	(0.019)	(0.013)	(0.019)	(0.013)	(0.019)	(0.012)	(0.017)
Panel C: Firms with and without bond ratings									
With bond rating	−0.011	−0.053**	0.057*	0.006	−0.010	−0.030	−0.031	0.006	0.055*
	(0.028)	(0.027)	(0.030)	(0.023)	(0.029)	(0.022)	(0.025)	(0.025)	(0.028)
Without bond rating	0.008	−0.027*	0.006	0.046***	−0.025	−0.011	−0.027***	0.007	0.032*
	(0.015)	(0.014)	(0.014)	(0.010)	(0.017)	(0.012)	(0.010)	(0.011)	(0.019)
Panel D: Firms with and without lines of credit									
With line of credit	−0.015	−0.062***	0.046**	−0.012	0.028	0.001	−0.031	0.037	−0.008
	(0.025)	(0.012)	(0.018)	(0.017)	(0.022)	(0.016)	(0.020)	(0.024)	(0.025)
Without line of credit –	−0.003	−0.013	0.012	0.071***	−0.072***	−0.021	−0.031**	−0.029	0.084***
firms in Dealscan only	(0.022)	(0.020)	(0.022)	(0.019)	(0.022)	(0.018)	(0.012)	(0.018)	(0.023)
Without line of credit –	−0.003	−0.006	0.000	0.034***	−0.029**	−0.007	−0.021***	−0.007	0.037**
all firms	(0.013)	(0.010)	(0.011)	(0.009)	(0.013)	(0.011)	(0.007)	(0.011)	(0.015)

Note: *p<0.1; **p<0.05; ***p<0.01

Table IA.7: Dividend regression results in subsamples. Regressions of dividends as a percentage of total assets using the regression framework specified in the caption of Table 3 for Panel C in the whole sample (Panel A) and subsamples (Panels B through D) detailed by the italicized headers. Firms with high and low Hadlock-Pierce index measures (Panel B) are in the biggest and smallest tercile, respectively. For ease of interpretation, the dependent variable is multiplied by 100. Only the coefficients estimated for *quarter interaction* are presented. Included firm controls are: lagged log of total assets, lagged market-to-book ratio, lagged leverage, working capital, ROA, and capital expenditures. Firm control variables are defined in the caption of Table 1. Included macroeconomic controls are: *recession* and *unemp*. Macroeconomic controls are also defined in the caption of Table 1. Year, firm, and quarter indicator variables are included in each estimation, along with a binary variable equal to one if a year is a presidential election year and interactions between the presidential election variable and quarter variables. Standard errors are clustered at the state-level.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Dividends as a dependent variable – Whole sample									
Quarter interaction	−0.006*	0.003	0.000	−0.007**	0.004	−0.000	0.001	0.010***	−0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Observations	311,873	311,873	311,873	311,873	311,873	311,873	311,873	311,873	311,873
R^2	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506
Panel B: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures									
low Hadlock-Pierce	−0.014**	0.011**	−0.003	−0.013***	0.003	0.001	0.004	0.008*	−0.014**
	(0.006)	(0.005)	(0.004)	(0.004)	(0.007)	(0.004)	(0.004)	(0.004)	(0.007)
high Hadlock-Pierce	0.000	−0.004	0.002	0.003	−0.001	−0.001	−0.003	0.014***	−0.009*
	(0.007)	(0.005)	(0.005)	(0.006)	(0.007)	(0.005)	(0.004)	(0.005)	(0.005)
Panel C: Firms with and without bond ratings									
With bond rating	−0.007	0.004	−0.007	−0.009*	0.012*	−0.001	0.014***	0.003	−0.016***
	(0.007)	(0.007)	(0.008)	(0.005)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)
Without bond rating	−0.006	0.003	0.000	−0.007**	0.004	0.000	−0.000	0.011***	−0.011***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)	(0.004)
Panel D: Firms with and without lines of credit									
With line of credit	−0.010**	0.003	−0.006	−0.009**	0.012*	−0.000	0.006*	0.010**	−0.016***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)
Without line of credit –	−0.003	0.010	−0.005	−0.003	−0.003	0.006	−0.002	0.006	−0.011
firms in Dealscan only	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.004)	(0.006)	(0.007)
Without line of credit –	−0.004	0.003	0.004	−0.005	−0.002	0.001	−0.003	0.009***	−0.008*
all firms	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)

Note: *p<0.1; **p<0.05; ***p<0.01

Table IA.8: Retired/raised regression results in subsamples. Regressions of quarterly debt raised or retired, scaled by assets, and multiplied by 100 using the regression framework specified in the caption of Table 3 in subsamples detailed by the italicized headers. Only the coefficients estimated for *quarter interaction* are presented. Firms with high and low Hadlock-Pierce index measures (Panel A) are in the biggest and smallest tercile, respectively. Standard errors are clustered at the state-level.

subsample	dep var	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Firms with high (constrained) and low (unconstrained) Hadlock-Pierce measures										
low Hadlock-Pierce	Debt raised	0.155	0.269*	−0.118	−0.146	−0.056	−0.218*	0.012	0.295	−0.049
		(0.141)	(0.150)	(0.169)	(0.131)	(0.198)	(0.128)	(0.153)	(0.202)	(0.198)
low Hadlock-Pierce	Debt retired	0.151	0.178*	0.057	−0.096	−0.183	−0.077	0.067	0.070	−0.056
		(0.099)	(0.108)	(0.092)	(0.091)	(0.118)	(0.085)	(0.086)	(0.075)	(0.103)
high Hadlock-Pierce	Debt raised	−0.258	0.224	−0.157	−0.149	0.061	−0.120	−0.182	0.252	0.082
		(0.208)	(0.214)	(0.215)	(0.207)	(0.250)	(0.158)	(0.204)	(0.180)	(0.156)
high Hadlock-Pierce	Debt retired	−0.101	−0.098	0.075	0.095	−0.063	0.027	0.012	0.007	−0.051
		(0.088)	(0.077)	(0.065)	(0.093)	(0.101)	(0.096)	(0.096)	(0.086)	(0.089)
Panel B: Firms with and without bond ratings										
With bond rating	Debt raised	0.448*	0.180	−0.012	−0.131	−0.047	−0.266	0.404*	−0.014	−0.127
		(0.249)	(0.238)	(0.292)	(0.315)	(0.214)	(0.255)	(0.208)	(0.256)	(0.196)
With bond rating	Debt retired	0.282*	0.175	0.263**	−0.098	−0.350**	−0.059	0.085	0.015	−0.048
		(0.158)	(0.114)	(0.131)	(0.161)	(0.144)	(0.138)	(0.117)	(0.150)	(0.138)
Without bond rating	Debt raised	0.072	0.290**	−0.011	−0.252**	−0.043	−0.214*	−0.155	0.279***	0.103
		(0.131)	(0.125)	(0.109)	(0.112)	(0.124)	(0.110)	(0.099)	(0.091)	(0.176)
Without bond rating	Debt retired	0.125	0.104**	0.095	−0.038	−0.171**	−0.119**	0.016	0.082**	0.025
		(0.080)	(0.045)	(0.069)	(0.045)	(0.076)	(0.047)	(0.048)	(0.041)	(0.072)
Panel C: Firms with and without lines of credit										
With line of credit	Debt raised	0.257	0.491***	−0.072	−0.335*	−0.115	−0.252*	0.044	0.337**	−0.132
		(0.177)	(0.163)	(0.159)	(0.179)	(0.211)	(0.129)	(0.172)	(0.162)	(0.209)
With line of credit	Debt retired	0.330***	0.221***	0.165*	−0.119	−0.282**	−0.114	0.016	0.174*	−0.086
		(0.120)	(0.074)	(0.093)	(0.096)	(0.138)	(0.084)	(0.104)	(0.090)	(0.119)
Without line of credit – firms in Dealscan only	Debt raised	0.208	0.057	0.185	−0.168	−0.082	−0.026	−0.073	0.100	−0.000
		(0.194)	(0.117)	(0.192)	(0.141)	(0.165)	(0.121)	(0.160)	(0.183)	(0.166)
Without line of credit – firms in Dealscan only	Debt retired	0.063	0.138**	0.072	−0.074	−0.147	−0.074	0.047	0.010	0.019
		(0.096)	(0.068)	(0.071)	(0.063)	(0.098)	(0.076)	(0.060)	(0.076)	(0.100)
Without line of credit – all firms	Debt raised	0.048	0.142	0.114	−0.290*	0.025	−0.075	−0.160	0.235**	0.003
		(0.149)	(0.120)	(0.139)	(0.156)	(0.154)	(0.078)	(0.098)	(0.105)	(0.146)
Without line of credit – all firms	Debt retired	0.067	0.040	0.114***	−0.032	−0.130*	−0.016	0.000	0.039	−0.024
		(0.074)	(0.043)	(0.043)	(0.048)	(0.068)	(0.049)	(0.039)	(0.046)	(0.062)

Note: *p<0.1; **p<0.05; ***p<0.01

Table IA.9: Monthly cash burn rate summary statistics. Cash burn rates calculated as percentage of assets (average burn rate) and burn rate as a percentage of cash holdings. Burn rates are calculated following Denis and McKeon (2021): operating cash flow minus dividends minus capital expenditures, scaled by 12. We refer to quarterly cash burn rates in the main text of this paper, or these monthly burn rates times three. Firms with high and low Hadlock-Pierce index measures are in the biggest and smallest tercile, respectively. The final column gives the t-statistic for the difference in means for each row.

	low Hadlock-Pierce	high Hadlock-Pierce	difference	t-statistic
average burn rate – whole sample	0.20%	1.72%	1.53%	[96.44]
burn rate / cash – whole sample	8.79%	22.17%	13.37%	[36.19]
negative cash flow (binary) – whole sample	35.64%	70.91%	35.27%	[85.13]
average burn rate – negative cash flow sample	0.55%	2.53%	1.98%	[92.24]
burn rate / cash – negative cash flow sample	25.37%	32.02%	6.65%	[9.95]

	rated firms	unrated firms	difference	t-statistic
average burn rate – whole sample	0.25%	1.25%	1.00%	[113.07]
burn rate / cash – whole sample	11.40%	18.40%	7.00%	[23.29]
negative cash flow (binary) – whole sample	40.01%	61.30%	21.29%	[55.25]
average burn rate – negative cash flow sample	0.63%	2.12%	1.49%	[101.78]
burn rate / cash – negative cash flow sample	29.44%	30.87%	1.42%	[2.21]

Table IA.10: Cash holdings regression results for seasonal and non-seasonal firms. A re-estimation of the results in Table 2 for subsamples defined using the Fairhurst (2020) definition of firms with seasonal and non-seasonal cash holdings. See the caption of Table 2 for details on the specification.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: seasonal									
Quarter interaction	0.385***	0.249**	−0.021	0.064	−0.281*	−0.192*	0.045	−0.041	0.177
	(0.099)	(0.111)	(0.115)	(0.108)	(0.148)	(0.115)	(0.095)	(0.084)	(0.131)
Observations	53,759	53,759	53,759	53,759	53,759	53,759	53,759	53,759	53,759
R^2	0.758	0.758	0.758	0.758	0.758	0.758	0.758	0.758	0.758
Panel B: nonseasonal									
Quarter interaction	0.442***	0.153	0.277***	−0.044	−0.384***	−0.294*	−0.240***	0.157*	0.371***
	(0.134)	(0.107)	(0.092)	(0.086)	(0.144)	(0.159)	(0.067)	(0.095)	(0.119)
Observations	204,533	204,533	204,533	204,533	204,533	204,533	204,533	204,533	204,533
R^2	0.777	0.777	0.777	0.777	0.777	0.777	0.777	0.777	0.777
Note:									*p<0.1; **p<0.05; ***p<0.01

A.2 Identification using close elections

We use U.S. gubernatorial election timing as a measure of uncertainty. Election closeness seems, prima facie, a useful measure of uncertainty that could be used alongside or instead of election timing. Election closeness provides gradation in uncertainty, allowing researchers to examine how various levels of uncertainty affect firms. However, elections do not happen in a vacuum. Gubernatorial election closeness is correlated with both the presidential election cycle and state economic conditions. Thus, examining election closeness must be carefully undertaken. For brevity, we leave this discussion to our appendix.

This appendix has three parts. First, we detail the endogeneity issues associated with examining election closeness. This discussion provides important context for our second part, in which we present robustness results examining how election closeness affects firm cash holdings. Consistent with our model predictions and main results, we find that higher uncertainty related to closer elections is associated with stronger evidence of savings behavior. However, because of inherent endogeneity issues, we discuss these results cautiously. Finally, we weigh the benefits of examining election closeness against the costs that come with more complicated identification. While examining close elections may provide more nuanced results than solely examining election timing, we argue that the growing political uncertainty literature has reduced the benefits of examining closeness by rendering such tests and findings redundant across studies.

U.S. gubernatorial election timing is an exogenous measure of uncertainty. Gubernatorial elections are pre-scheduled, rather than brought on by political or economic conditions as can occur in parliamentary systems, so election timing is orthogonal to the business cycle. The exogenous nature of gubernatorial election timing combined with variation in election cycles—at least some elections occur in every year—sets up an ideal difference-in-difference (DD) framework for causal inference.

In contrast, election closeness is correlated with several confounders, including presidential elections and election cycles. Presidential and gubernatorial election outcomes are intertwined in complex ways. Carsey and Wright (1998) show presidential approval ratings affect gubernatorial election voting patterns. Similarly, Simon (1989) concludes evaluations of the president can affect individuals' voting patterns and gubernatorial election outcomes, calling the phenomenon by which all lower-level elections to varying degrees reflect an evaluation of the president, "electoral retribution". Peltzman (1987) notes gubernatorial elections in the year or two following the presidential election gives "voters the opportunity to 'settle up' with the president..." Erikson, Folke, and Snyder Jr. (2015) examine a story with reversed causality and find that gubernatorial elections affect subsequent presidential elections by showing that states that narrowly elected a Republican governor are more likely to be won by a Democratic presidential candidate, and vise versa. Results from these studies suggest gubernatorial election closeness is correlated with evaluations of presidential performance, the closeness of presidential elections, and the presidential election cycle.

The interconnectedness of presidential and gubernatorial election outcomes presents a challenge to using gubernatorial election closeness as a measure of state-level uncertainty. This interconnectedness suggests a serious measurement error problem with gubernatorial election closeness, as closeness also likely reflects voters' degree of satisfaction with the president and his or her party and the timing of the gubernatorial election within the presidential election cycle. It is unclear whether or how the effects of gubernatorial election closeness can be disentangled from these presidential election factors.

The same literature that suggests presidential cycles are endogenous with gubernatorial election closeness also examines how state economic conditions affect gubernatorial election outcomes. To study the effects of state economic conditions on gubernatorial elections, a researcher must control for confounders related to the president and presidential elections, and vice versa. Peltzman (1987) and Carsey and Wright (1998) show incumbent governors perform less well when state economic conditions are worse. These studies suggest a correlation between gubernatorial election closeness and state economic conditions.

The correlation between state economic conditions and gubernatorial election closeness is a more serious challenge to identification than the mismeasurement problem related to presidential approval and election cycles. Correlation between state economic conditions and gubernatorial election closeness results in the need to control for potential reverse causality. State economic conditions, which can be reflected in firms' higher (or lower) cash balances, can result in closer gubernatorial elections. Whereas mismeasurement attenuates coefficient estimates, reverse causality unpredictably biases estimates. Indeed, one reason we use election timing as an exogenous measure of uncertainty is because other measures of uncertainty, like the Baker et al. (2016) economic policy uncertainty index, are correlated with the business cycle. Including election closeness in our study effectively re-introduces this endogeneity problem via a different route.

Our data reflect the same correlations between gubernatorial election closeness, the presidential election cycle, and state economic conditions previous literature documents. The top panel of Fig. IA.2.1 shows the number of gubernatorial elections (height of the blue bars) for each year from 1981 to 2016. The number of close elections within each year is indicated by the height of the red bars. We define close elections following Jens (2017), as elections with vote differentials—the difference in percentage of votes received by the first and second place candidates—in the bottom tercile of the sample. The top panel of Fig. IA.2.1 shows variation in election closeness over time. The bottom panel of Fig. IA.2.1 shows the average vote differential for each year from 1981 to 2019. Consistent with the predictions in Peltzman (1987), there is a rise in election closeness, seen in the figure as a drop in average vote differential, in most years following presidential elections (1981, 1985, 1989, 1993, 1997, 2001, 2005, 2009, 2013, and 2017). With the exception of 1985, vote differential in these years is at or below the average vote differential in the sample, indicated by the dashed horizontal line. The results in Fig. IA.2.1 show a clustering in elections by closeness in the years directly following presidential elections.

Fig. IA.2.2 shows a histogram of the number of elections in bins of election closeness. The height of

the bars shows the number of elections in each range of vote differential. Also indicated are the number of these elections that are in years following presidential elections (height of black bar sections) and years with low GDP (height of dark blue bar sections), which are defined as years in which the change in state GDP was in the bottom 15th percentile of the sample. The figure shows close elections are more likely to occur in years with low GDPs and years following presidential elections. The relation between close elections and these potential confounders also appears to be nonlinear.

Table IA.2.1 provides summary statistics of vote differential, our binary election closeness variable, change in state GDP, and a binary variable for whether a year is the year after a presidential election. We examine three subsamples: the most competitive tercile of elections (Panel A), for which our binary closeness variable is equal to 1, the middle tercile of elections by closeness (Panel B), and the least competitive tercile of elections (Panel C). The percentage of the sample that is a post-presidential election year drops from 5.23% to 4.62% to 1.74% moving from Panel A to Panel B to Panel C. These results support those in Figs. IA.2.1 and IA.2.2 and show a correlation between the timing of the presidential election cycle and gubernatorial election closeness. Additionally, change in state GDP is lowest in the closest election tercile, with an average of 4.09% (median of 4.42%), compared with averages of 5.32% and 5.23%, respectively, (medians of 5.04% and 5.08%, respectively) in the middle and least competitive terciles. The nonlinear relation between change in state GDP and election closeness is also apparent in these results.

We do not make any kind of a causal statement based on our results in Figs. IA.2.1 and IA.2.2 and Table IA.2.1. It is sufficient for our discussion of the problems associated with close elections that these correlations exist. No causal statements are necessary to demonstrate these issues.

Despite the endogeneity issues inherent in examining election closeness, robustness checks examining closeness are commonly requested and performed in the political uncertainty literature. In Table **??**, we follow suit and examine whether firms headquartered in states with close elections have higher cash balances than firms headquartered in states with non-close elections. In our main results, we show firms hold higher cash balances up to four quarters before elections, or starting in Q4 of years before elections. Before close elections, we expect a stronger effect of uncertainty on firm cash holdings. Firms could begin to build up cash balances even earlier than Q4 of years before elections and could build up larger balances than are seen, on average, in election years, or both. Additionally, as uncertainty related to close elections should resolve more slowly than uncertainty related to non-close elections, we could see a more pronounced drawdown in cash following elections, both in magnitude and duration.

To measure the effects of close elections on firm cash holdings, we use the same regression framework as for Table 2 but with a binary election closeness, rather than timing, variable. Following Jens (2017), we define close elections as elections with a vote differential—the difference in percentage of vote received by the first and second place candidate—in the bottom tercile of election closeness. In Panels A, B, and C, we limit our sample to years before elections, years with elections, and years following elections, respectively. This setup decreases sample size and power but allows us to measure an incremental effect of election

closeness on cash holdings. We interpret these coefficients as the difference between cash holdings in firms headquartered in close-election states relative to cash holdings of firms headquartered in non-close-election states.

Results in Table IA.2.2 show evidence of an earlier build-up in cash before elections. Estimated coefficients for quarter interactions for Q1, Q2, and Q3 in the year before the election are positive, indicating relatively higher cash balances in firms headquartered in close election states relative to firms headquartered in non-close election states. In these quarters, we estimate cash holdings of firms headquartered in states with upcoming close elections are 0.002%, 0.105%, and 0.035% higher than the cash holdings of firms headquartered in states with upcoming non-close elections. Although these estimates are not statistically different from zero, they are positive, as predicted, and suggestive of higher cash holdings before close elections than before non-close elections. Generally, we see smaller t-statistics for estimates of pre-election cash buildup in Table IA.2.2 than in Table 2, likely related to the fact that our sample was reduced by approximately 75% to focus on close elections.

Results in Table IA.2.2 also show evidence that close-election firms rely more on stored cash following elections than non-close-election firms. Differences between the cash holdings of close-election and non-close-election firms are small in election years. However, we find that two quarters after the election, firms in close election states have, on average, 0.253% less cash holdings than firms in non-close election states. This finding is consistent with slower resolution of uncertainty, on average, following close elections than non-close elections, so firms rely more on stored cash following close elections. Taken together, these results show that examining close elections provide some evidence of stronger cycles in firm cash holdings when uncertainty is relatively higher. However, given the large economic magnitude of cash savings and dissavings around elections, on average, and that close elections are an imperfect proxy for higher uncertainty, we are not surprised that the marginal effect of examining close elections is small.

The one benefit of examining election closeness as an additional measure of uncertainty is arguably outweighed by the cost of an extra hurdle to identification, particularly given recent advances in the political uncertainty literature. Jens (2017) discusses the importance of examining election closeness, noting that, "...if I do not look at some measure of gradation of political uncertainty above the baseline level of political uncertainty created by elections, I am unable to determine whether my effects are caused by higher political uncertainty associated with elections or the elections themselves." However, there are now a number of papers, including Julio and Yook (2012), Jens (2017), Chen et al. (2023), and Atanassov et al. (2024), who use election timing as an exogenous measure of political uncertainty. In contrast, no study exists using elections to study the effects of elections themselves on firm decisions or clearly identifies any alternative story whereby elections themselves may affect firms. Additionally, there is evidence that volatility, a measure of uncertainty, spikes around elections (Boutchkova, Doshi, Durnev, and Molchanov, 2012; Białkowski, Gottschalk, and Wisniewski, 2008). The fast-growing literature on political uncertainty now contains overwhelming evidence that elections measure political uncertainty,

so examining election closeness constitutes a robustness check that is increasingly unnecessary to demonstrate in empirical studies. Should such a robustness test be required, our placebo analysis effectively demonstrates our results are not driven by spurious correlations without requiring relying on identifying effects from close elections.

In Table IA.2.3, we demonstrate that our main results are robust to removing close elections from the sample. For ease in comparison, Panel A of Table IA.2.3 repeats the results from Table 2 showing the effects of political uncertainty from election cycles on cash holdings. In Panel B, we re-estimate these results omitting close election cycles from the sample, or elections with outcomes in the lowest tercile of closeness. Although removing close election cycles reduces our sample size, we continue to find strong evidence of cycles in firms' cash holdings around elections. The results in Table IA.2.3 show that our main results are not driven by close elections; we also see cycles in firms' cash holdings around non-close elections.

In sum, while election timing is a useful, exogenous measure of political uncertainty, the incremental benefits of examining election closeness are less clear. An important concern in examining the effects of any type of uncertainty is controlling for correlation between the uncertainty measure and business cycle. Gubernatorial election timing is orthogonal to the business cycle. While examining election closeness in addition to timing may provide additional information, election closeness is endogenous. Thus, incorporating tests with election closeness as a variable of interest must be carefully designed and conducted. Ultimately, in a study of political uncertainty proxied by election timing, endogeneity issues outweigh the potential for stronger identification using election closeness, leaving little marginal value from using closeness even as a robustness check.

Figure IA.2.1: Summary of close elections over time. Gubernatorial election data are from 1981 to 2019. Close elections are in the lowest tercile of vote differential. The dashed line in the bottom plot indicates the median vote differential for the sample.

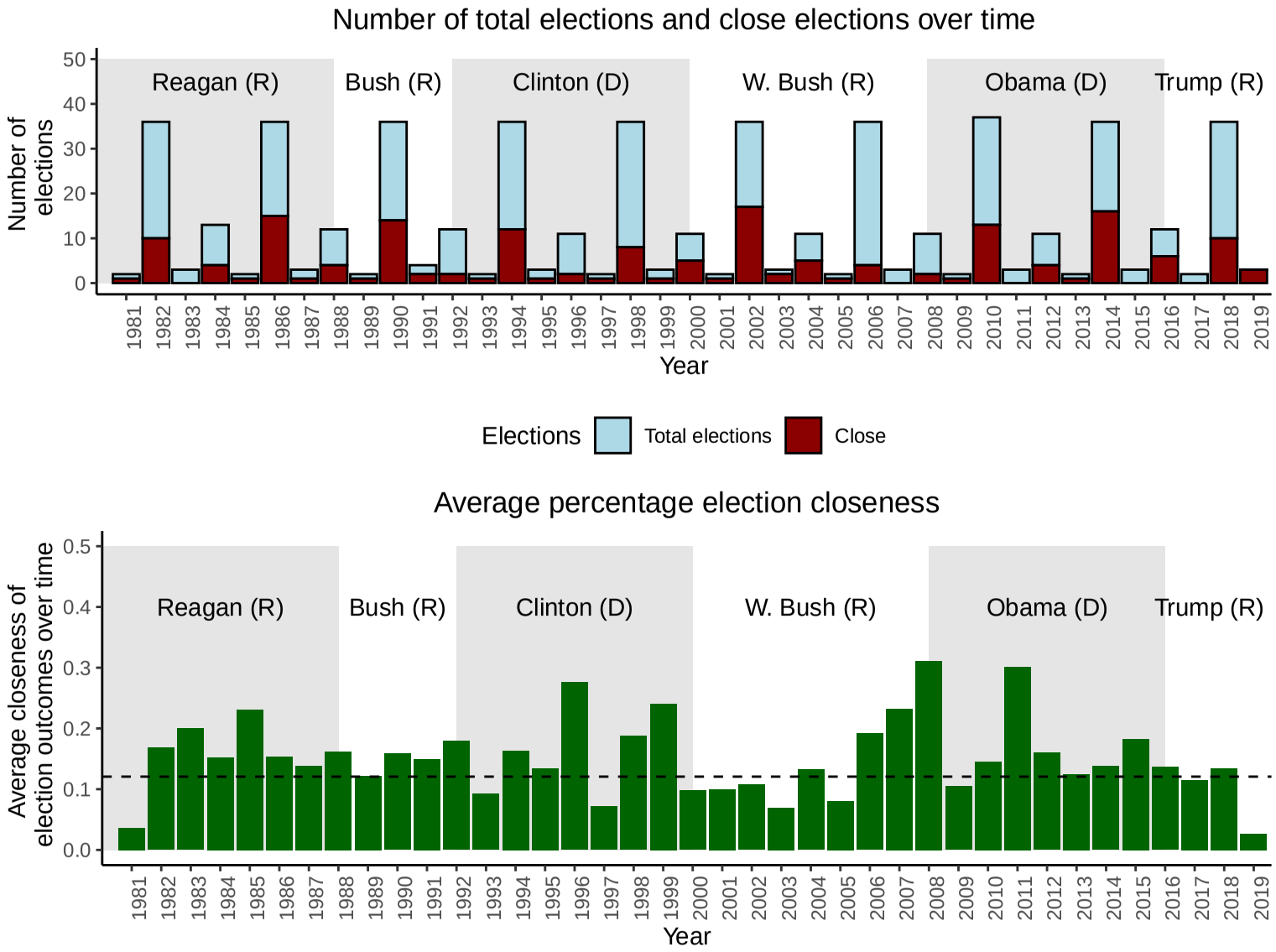

Figure IA.2.2: Histogram of elections in bins of election closeness. The height of the bar indicates the total number of elections in each bin. Also indicated is the number of elections in years following presidential elections (solid black) and years with low GDP (dark blue), defined as years in the bottom 15% of state GDP growth. Gubernatorial election data are from 1981 to 2019.

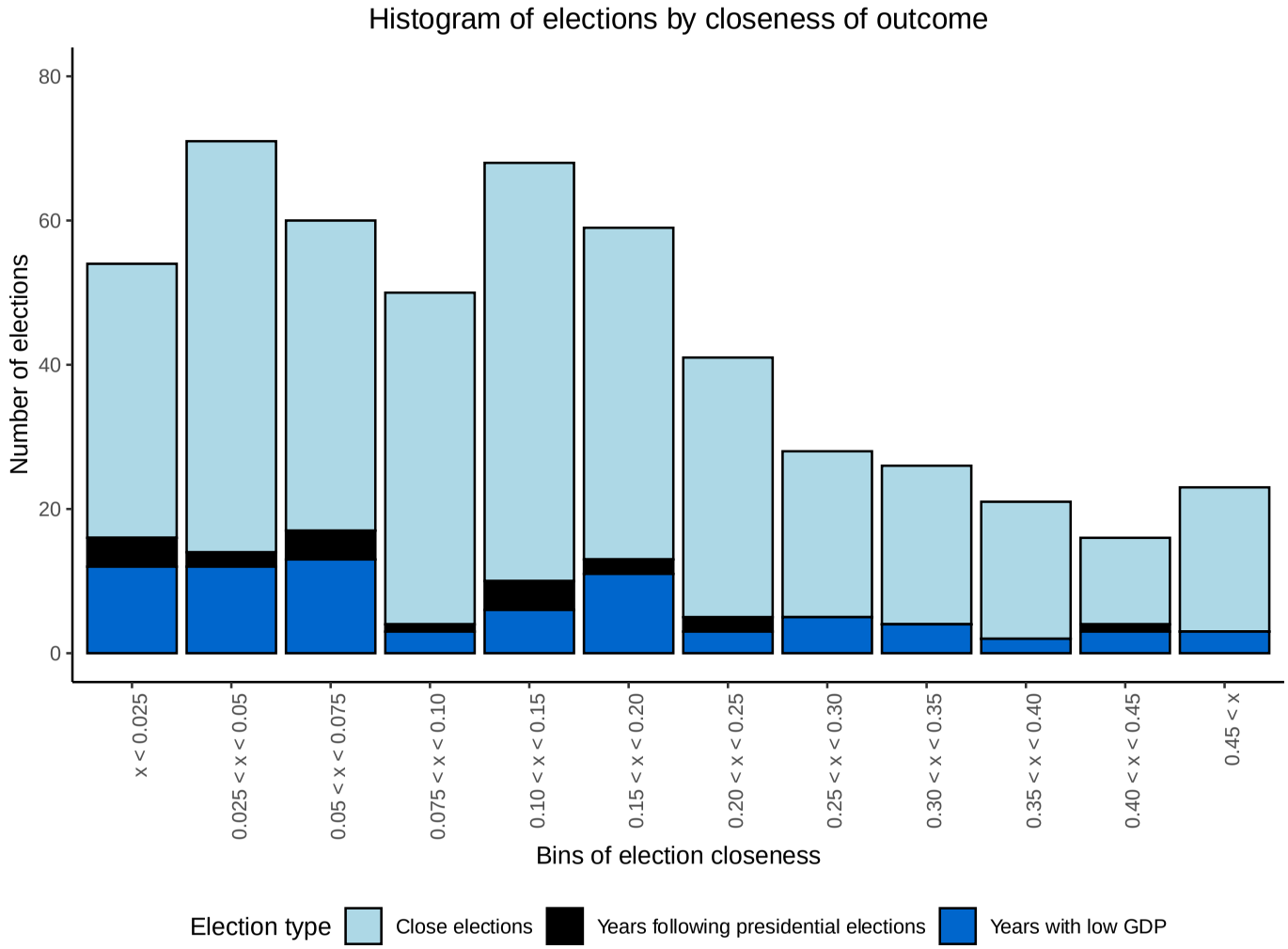

Table IA.2.1: Election closeness summary statistics. Summary statistics for vote differential (difference in percentage of votes received by the first and second place candidates), a binary election closeness variable, change in state GDP, and a binary variable if a year is the year after a presidential election. Samples are defined in the italicized headers. Gubernatorial election data are from 1981 to 2019.

Variable	Min	Q1	Median	Mean	Q3	Max
Panel A: Most competitive (closest) tercile of elections						
vote differential	0.005%	1.77%	3.48%	3.48%	5.09%	6.78%
close election	100.000%	100.00%	100.00%	100.00%	100.00%	100.00%
change GDP	−30.962%	2.75%	4.42%	4.09%	5.67%	12.87%
yearafter presidential election	0.000%	0.00%	0.00%	5.23%	0.00%	100.00%
Panel B: Middle tercile of election closeness						
vote differential	6.884%	8.93%	12.04%	12.13%	14.97%	18.41%
close election	0.000%	0.00%	0.00%	3.47%	0.00%	100.00%
change GDP	−14.838%	3.55%	5.04%	5.32%	6.65%	12.87%
yearafter presidential election	0.000%	0.00%	0.00%	4.62%	0.00%	100.00%
Panel C: Least competitive tercile of elections						
vote differential	18.581%	23.02%	29.85%	31.85%	39.26%	64.74%
close election	0.000%	0.00%	0.00%	0.00%	0.00%	0.00%
change GDP	−7.426%	3.70%	5.08%	5.23%	7.12%	22.02%
yearafter presidential election	0.000%	0.00%	0.00%	1.74%	0.00%	100.00%

Table IA.2.2: Re-estimation of main cash holdings results with election closeness. Results from re-estimations of the regressions in Table 2 with a binary treatment variable for election closeness, rather than election timing. An election is close if the vote differential—the difference in percentage vote received by the first and second place candidates—is in the bottom tercile of the sample. Sample is limited to observations in years before election years, years with elections, and years after election years. The quarter interaction variable is an interaction of *yearbefore*, *election*, and *yearafter*, respectively, with the quarter indicated in the column header. Standard errors are clustered at the state-level and given in parentheses below coefficient estimates.

	−7	−6	−5	−4	−3	−2	−1	election	+1	+2	+3	+4
Quarter interaction	0.002	0.105	0.035	−0.144	0.035	−0.118	0.066	0.019	0.112	−0.253*	0.046	0.107
	(0.164)	(0.118)	(0.099)	(0.138)	(0.152)	(0.087)	(0.092)	(0.145)	(0.161)	(0.136)	(0.084)	(0.248)
Observations	80,008	80,008	80,008	80,008	80,598	80,598	80,598	80,598	81,301	81,301	81,301	81,301
R^2	0.811	0.811	0.811	0.811	0.819	0.819	0.819	0.819	0.817	0.817	0.817	0.817

Note: *p<0.1; **p<0.05; ***p<0.01.

Table IA.2.3: Robustness of cash holdings results to dropping close elections. Regressions of cash holdings (cash scaled by total assets) multiplied by 100 using the regression framework specified in the caption of Table 2. Panel A is a copy of the results in Table 2, while in Panel B we drop all observations from close elections. Standard errors are clustered at the state-level and given in parentheses below coefficient estimates.

	−4	−3	−2	−1	election	+1	+2	+3	+4
Panel A: Full sample									
Quarter interaction	0.377***	0.129*	0.206***	−0.025	−0.308***	−0.355***	−0.196***	0.176***	0.368***
	(0.109)	(0.066)	(0.074)	(0.062)	(0.112)	(0.099)	(0.059)	(0.065)	(0.089)
Observations	319,093	319,093	319,093	319,093	319,093	319,093	319,093	319,093	319,093
R^2	0.798	0.798	0.798	0.798	0.798	0.798	0.798	0.798	0.798
Panel B: Dropping close elections									
Quarter interaction	0.463***	0.160**	0.204***	−0.046	−0.318***	−0.419***	−0.114**	0.179***	0.346***
	(0.121)	(0.077)	(0.067)	(0.065)	(0.108)	(0.086)	(0.058)	(0.064)	(0.069)
Observations	242,112	242,112	242,112	242,112	242,112	242,112	242,112	242,112	242,112
R^2	0.808	0.808	0.808	0.808	0.808	0.808	0.808	0.808	0.808

Note: *p<0.1; **p<0.05; ***p<0.01

Appendix B.1

Dependent Variable	Firm-level Controls	Macro Controls	Fixed Effects
Cash holdings	Log of assets	Recession dummy	State
	Market-to-book ratio	State unemployment	Industry
	Leverage		Year
	Working capital		Year
	ROA		Year
	Credit rating dummy		Year
Cash savings	Lag log of assets	Recession dummy	State
Payout	Lag market-to-book ratio	State unemployment	Industry
Equity issuance	Lag leverage		Year
	Lag working capital		Year
	Investment		Year
	ROA		Year
	Credit rating dummy		Year
Investment	Lag log of assets	Recession dummy	State
	Lag market-to-book ratio	State unemployment	Industry
	Lag leverage		Year
	Lag working capital		Year
	ROA		Year
	Credit rating dummy		Year