The Effect of Takeover Protection in Quiet Life and Bonding Firms

Eliezer M. Fich [ID]
Drexel University LeBow College of Business Finance Department
efich@drexel.edu (corresponding author)

Jarrad Harford [ID]
University of Washington Michael G. Foster School of Business Department of Finance
jarrad@uw.edu

Adam S. Yore [ID]
University of Missouri Trulaske College of Business Department of Finance
yorea@missouri.edu

Abstract

Antitakeover measures are controversial because the evidence of their net effect on shareholders is mixed. We propose that, for many firms, the potential bonding benefits outweigh the agency costs of the quiet life, explaining the mixed results. We study business combination and poison pill laws as exogenous shocks to takeover vulnerability and use shareholder valuation of internal slack as an indicator of the net effect of takeover protection. Firms susceptible to quiet life agency problems exhibit a decrease in the market-assessed value of internal slack. Conversely, cash appreciates at companies where takeover protection bonds commitments with major counterparties.

I. Introduction

Takeover threats can play an important role, both good and bad (as in Stein (1988)), in the overall governance and incentive structure of top managers. As a result, a large literature in both finance and law debates the effect of various antitakeover provisions and related legislation, offering mixed conclusions. Gompers, Ishii, and Metrick (2003), along with Bebchuk, Cohen, and Ferrell (2009), propose indexes of internally and externally adopted antitakeover provisions as measures of entrenchment, or more broadly, agency problems between top managers and shareholders. Others have used staggered state-level adoptions of

antitakeover legislation as natural experiments to examine shocks to takeover vulnerability. Work by Giroud and Mueller (2010), Atanassov (2013), and Gormley and Matsa (2016), respectively, examines changes in productivity, corporate innovation, and distress risk after the adoption of one of the most commonly enacted antitakeover laws, the business combination (BC) law.[1]

The competing views on takeover protection are driven by differing weights put on agency costs versus the value of bonding commitments to employees and customers. Thus, we propose that an external shock to takeover protection should have heterogeneous effects depending on how the agency and bonding effects balance. To examine this possibility and further our understanding of the nuanced effects of takeover protection, we use shareholder valuation of internal cash as an experimental indicator. This setting is motivated by Jensen and Meckling's (1976) view that internal slack is the asset most sensitive to agency conflicts because it may enable managers to invest in value-reducing projects that afford them personal benefits. Consequently, providing managers with enough cash reserves to avoid underinvestment, but not so much as to allow overinvestment, is central to Jensen (1986) and Stulz (1990), which laid the foundation for much of the study on the agency costs of free cash flow. Thus, the value shareholders place on a marginal dollar in the hands of managers is a powerful barometer of what matters in corporate governance.

There are several specific channels through which antitakeover provisions (ATPs) can affect the value of cash. Two cited benefits of ATPs are the increased ability to commit to relationships with stakeholders such as other firms and employees, and decreased pressure on short-term results through a decrease of the takeover threat described in Stein (1988). In both cases, the value of growth opportunities (unlocked through increased relationship-specific investment or through longer-term focus, even absent such relationships) increases, and the utility of internal slack to fund those opportunities, likely to be characterized by significant information asymmetry, increases. Notably, in the case of relationship-specific investment, Johnson, Karpoff, and Yi ((2015), (2022)) find evidence supporting the *bonding hypothesis* predicting that ATPs benefit firms by encouraging their key commercial counterparties to invest in the business relationship.

On the other hand, takeover protection heightens the potential for self-interested actions by managers. These can take the form of either empire-building or *quiet-life* behavior, although Bertrand and Mullainathan (2003) find stronger evidence the latter is a better characterization of managerial behavior. Specifically, those authors find support for the *quiet-life* hypothesis predicting that managers in noncompetitive industries exploit the takeover protection afforded by BC laws by exerting less effort on difficult activities, like cutting inefficient investment or confronting unions. Notably, in this case, shareholders would put a lower value on internal slack because of the flexibility it affords under-controlled managers.

---

[1]BC laws impose a moratorium on certain transactions, particularly mergers and asset sales, between a large shareholder and the firm for a period ranging from 2 to 5 years after the large shareholder's equity stake exceeds a pre-specified threshold. This moratorium prevents corporate raiders from gaining access to the target firm's assets for the purpose of paying down acquisition debt, thereby making hostile takeovers more difficult and often impossible.

Of course, ATPs affect the value of the firm through channels other than the value of internal cash.[2] For example, the increased ability of the firm to commit to its stakeholders comes from the fact that ATPs increase the bargaining power of target managers receiving an unsolicited takeover bid and decreases the likelihood of receiving a bid in the first place (e.g., DeAngelo and Rice (1983), Stulz (1988), and Comment and Schwert (1995)). The net effect of fewer bids, but greater bargaining power conditional on a bid, will be priced into firms' stock as well. We focus on the channels that will act through the marginal value of cash because doing so allows us to focus on the agency/bonding tradeoffs without conflating effects from changes in unconditional expected takeover premiums. Because these multiple channels are affected and sometimes in opposing directions, the academic literature on BC laws reports mixed evidence. To circumvent this issue, we use the marginal value of cash as the outcome variable as it helps isolate two specific channels underlying the impact of BC laws: bonding commercial relationships and quiet-life agency problems.

Our econometric approach relies on the staggered adoption of state-level BC laws as a natural experiment in a difference-in-differences (DiD) setting. This strategy circumvents endogeneity concerns associated with the willful selection of antitakeover provisions by firms. Our DiD tests generate strong and consistent evidence that the marginal value of 1 dollar is about 62 cents for the average firm without a BC law in place, but that value increases by about 24 cents when BC laws pass. This baseline result is robust to different econometric specifications, including the use of high-order fixed effects (Gormley and Matsa (2014)) and the addition of the controls used by Faulkender and Wang (2006) and Halford, McConnell, Sibilkov, and Zaiats (2024)). Moreover, we confirm the baseline evidence with a different natural experiment: the passage of poison pill endorsement laws. Poison pills have existed since 1982, but their legality has been questioned. Starting in the mid-1980s, some states started passing laws explicitly authorizing corporations to issue the rights that are at the heart of the poison pill defense. We find that the marginal value of cash increases by about $0.21 after poison pill endorsement (PP) laws pass.

Our baseline evidence that the marginal value of cash increases after BC and PP laws pass is somewhat surprising. It sharply contrasts with the results in Dittmar and Mahrt-Smith (2007) that entrenchment, measured by antitakeover provisions and institutional blockholder ownership, decreases the value of cash. This is because BC and PP laws, like antitakeover provisions in corporate charters, are widely regarded to weaken the market for corporate control—a mechanism viewed as effective in disciplining managers and reducing agency costs within a firm (e.g., Jensen and Ruback (1983), Shleifer and Vishny (1997), and Bertrand and Mullainathan (2003)). However, our results support the bonding hypothesis' prediction that takeover protections can benefit shareholders when high relationship-specific investments are needed (see Johnson et al. (2015), (2022), Cremers, Litov, and Sepe (2017)).

---

[2]Also, it is possible for a change in firm prospects to increase the value of internal cash, but decrease overall firm value (such as financial distress). Nonetheless, our hypothesized channels through which ATPs would affect the value of cash do not fall into that category of effects.

Indeed, the evidence on BC laws' effect is mixed. For example, Cen, Dasgupta, and Sen (2016) report a positive impact on return-on-assets (ROA), while John, Li, and Pang (2017) report a decrease in ROA. These mixed results on operating performance contrast with our average positive effect, found using a measure that focuses on the effect of agency/bonding tradeoffs rather than an overall effect that is conflated by the loss of the potential takeovers and their premiums. Investors' valuation of internal cash should change quickly to incorporate long-run expected improvements in operating performance that should occur from greater relationship-specific investment, but with a lag. However, such long-run operating performance improvements can be harder to detect and may require longer windows to do so. Given this discussion, we expand our natural experiment-based tests to test the *quiet life hypothesis* in the context of the marginal value of cash after antitakeover laws pass. The tests confirm that the value of an additional dollar increases, on average, after BC laws pass. Yet, the marginal value of cash declines in quiet life firms. In these companies, the value of an extra dollar drops by 26 to 30 cents after BC laws pass and by 15 to 25 cents once PP laws pass. These findings, which rely on defining quiet life firms as those in highly concentrated industries (Giroud and Mueller (2010)) with a low threat of import penetration (Bena and Xu (2017)), also hold in subsample tests and disappear in falsification tests. Not surprisingly, quiet life firms do not increase their cash balances after antitakeover laws pass.

In contrast to the results for quiet life firms, we find that firms with a key industrial client exhibit increases in their market-assessed value of internal slack and the level of cash holdings after BC or PP laws pass. This evidence, which suggests that the passage of these laws allows firms to credibly commit to their major business partners, supports the *bonding hypothesis* of takeover defenses advanced by Johnson et al. (2015). They propose (and empirically show) that takeover defenses provide bonding firms a commitment device with their major business counterparties. Such a device reassures these partners that the firm will honor their claims and encourages them to invest in the business relationship. Once a firm can commit that its business relationships are less likely to be disrupted from a hostile acquirer, cash reserves' value (as a source of financial stability) increases in relevance to the firm's relational partners. Together, with the main results, our findings on bonding firms produce a more nuanced inference on how the market views the impact of takeover protection; it clearly matters and can even be positive for certain groups of firms.

Given this nuanced interpretation of how takeover protection affects firms, we study whether the ATPs act through the channels discussed earlier and whether those channels align with bonding versus agency concerns in a manner consistent with our inferences. Additional tests reveal that after BC laws pass, the marginal value of cash i) increases in the firm's investment opportunities, ii) increases in firms that belong to industries where overinvestment is less likely, and iii) increases in firms raising cash but not in those distributing it. Further, these characteristics align with the firms we have characterized as benefiting from increased bonding versus being susceptible to agency issues in a way that bolsters our confidence in our inferences.

Our empirical analyses rely on a DiD estimation that allows us to evaluate the effect of antitakeover laws by comparing the changes in the value of internal slack over time between firms incorporated in states that enact these laws (the treatment group) and firms incorporated in states that do not (the control group). To ensure the internal validity of the DiD models, the parallel trends condition must be fulfilled. It requires that, in the absence of treatment, the difference between the "treatment" and "control" group is constant over time. In addition, Bertrand, Duflo, and Mullainathan (2004) note that another potential caveat with DiD models is that the estimated standard errors may understate the standard deviation of the treatment effect, leading to the inflation of t-statistics and significance levels. Moreover, Bertrand et al. (2004), also note that, to verify the validity of empirical findings generated with DiD models, researchers should check whether the control group is intrinsically different from the treatment group. Appendix B presents different tests showing that our setting appears to satisfy the parallel trends condition, that our control and treated groups are not intrinsically different, and that our analyses are likely not subject to problems associated with artificially inflated t-statistics or to concerns related to a common divisor.

The use of the staggered passage of state-level BC laws as a natural experiment mitigates endogeneity concerns related to the selection and implementation of antitakeover provisions by firms. Moreover, as noted by Karpoff, Schonlau, and Wehrly (2022), not all provisions, particularly some in the Gompers et al. (2003) index, affect takeover likelihood. At the same time, Heath, Ringgenberg, Samadi, and Werner (2023) show that "reusing" BC laws as a natural experiment increases the chance of false discoveries. Our main results survive the "reusing" concern as their associated t-statistics exceed the critical values indicated by Heath et al. (2023).

This article contributes to the body of work studying how firm characteristics or corporate settings affect the value of cash holdings.[3] Our baseline finding indicates that the marginal value of cash increases after antitakeover laws pass. On the surface, this result conflicts with the evidence in Dittmar and Mahrt-Smith (2007) that the value of an extra dollar deteriorates in firms with poor corporate governance—measured with the Gompers et al. (2003) index and the level of institutional monitoring. To reconcile the findings, we further show that our result is robust to controlling for the Gompers et al. (2003) index and, more importantly, that there is cross-sectional heterogeneity in how the marginal value of cash changes after antitakeover laws pass. Specifically, we find that firms susceptible to quiet life agency problems exhibit a decrease in the value of internal slack. Conversely, cash appreciates in firms where takeover protection helps bond commitments with major counterparties. These novel results, which provide a more complete picture of the impact of takeover protection laws, also complement concurrent work showing that the value of an extra dollar depends on whether firms accumulate or distribute cash

---

[3]Other work in this literature includes Harford (1999), Opler, Pinkowitz, Stulz, and Williamson (1999), Pinkowitz and Williamson (2001), Dittmar, Mahrt-Smith, and Servaes (2003), Mikkelson and Partch (2003), Faulkender and Wang (2006), Dittmar and Mahrt-Smith (2007), Foley, Hartzell, Titman, and Twite (2007), Pinkowitz and Williamson (2007), Denis and Sibilkov (2010), Duchin (2010), Harford et al. (2014), Masulis and Reza (2014), Dessaint and Matray (2017), and Duchin, Gilbert, Harford, and Hrdlicka (2017).

(Halford et al. (2024)). Moreover, our results suggest that all else equal, antitakeover provisions voluntarily adopted by the firm—like those in Dittmar and Mahrt-Smith (2007) study—can affect the value of internal slack differently than similar provisions embedded in legislation adopted by their state of incorporation. This divergence could be due to an omitted variable bias whereby voluntarily adopted provisions proxy for, and signal to shareholders, greater agency problems at the firm. An alternative but consistent explanation is that, unlike most of the components of the Gompers et al. index, BC laws do not automatically undermine shareholder rights.

By showing that the promulgation of BC laws affects the value of firms' internal slack, our work contributes novel evidence to the strand of the literature evaluating antitakeover laws' influence on various corporate characteristics and policies. Papers in this area include Bertrand and Mullainathan (1999), (2003), Garvey and Hanka (1999), Qiu and Yu (2009), Francis, Hasan, John, and Waisman (2010), Giroud and Mueller (2010), Armstrong, Balakrishnan, and Cohen (2012), Atanassov (2013), Cen et al. (2016), Gormley and Matsa (2016), and John et al. (2017). Karpoff and Wittry (2018) assert that the effect of BC laws varies according to the legal environment in which these laws are deployed and the institutional characteristics of the affected firms. We address these concerns by conducting all robustness tests suggested by Karpoff and Wittry (2018).[4]

Our evidence on the heterogeneous effect of BC laws on the marginal value of cash also contributes to the literature on the dissimilar impact of one-size-fits-all legislation (see, e.g., Bourveau, Lou, and Wang (2018) and Appel (2019) for evidence on Universal Demand laws, as well as Eldar and Grennan (2024) and Fich, Harford, and Tran (2023) for evidence on Corporate Opportunity Waiver laws).

The article continues as follows: Section II describes the data and methods. Section III presents our empirical analyses. Section IV contains our conclusions. Appendix A provides definitions for all the variables used in this study and Appendix B addresses some econometric issues.

II.   Data and Experimental Design

We begin with the universe of firms listed in the Compustat database spanning the fiscal years 1972 to 2010, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration/non-classifiable firms (SIC 9000–9999). As shown in Table 1, BC laws are approved over the 13-year period from 1985 to 1997. Our sample period reflects three equal windows comprised of the 13 years

---

[4]These robustness tests include controls for existing firm-level takeover defenses, for firms that lobbied for BC laws, for other state laws, for notable (and potentially confounding) case law, and for the unique effect of the pre-1982 period in which first-generation antitakeover laws were effective. After accounting for all these issues, we find that the positive association of BC laws and the value of cash, at least after the *Amanda Acquisition Corp. v. Universal Foods Corp.* decision (which affirmed the constitutionality of these laws), persists. In other robustness tests, we consider the effect of firms that reincorporate to other states and the influence of Delaware incorporation. The latter test also addresses an issue noted by Cain, McKeon, and Solomon (2017) of a potential lack of variation in BC law coverage. These tests generate results consistent with the baseline findings.

TABLE 1

Business Combination and Poison Pill Legislation

Table 1 presents the dates that the business combination laws (BC Law) and poison pill laws (PP Law) in our sample were adopted. Data on the adoption of these laws are obtained from Bertrand and Mullainathan (2003) and Karpoff and Wittry (2018). The number of unique firms in our sample that are incorporated in each state is indicated below.

State	Date BC Law Adopted	Date PP Law Adopted	# Firms Incorporated in the State	State	Date BC Law Adopted	Date PP Law Adopted	# Firms Incorporated in the State
Alabama			9	Montana			4
Alaska			1	Nebraska	4/8/1988		6
Arizona	7/22/1987		26	Nevada	6/25/1991	6/21/1989	294
Arkansas			5	New Hampshire			3
California			352	New Jersey	8/5/1986	6/29/1989	165
Colorado		3/31/1989	177	New Mexico			9
Connecticut	6/7/1988	6/26/2003	25	New York	12/16/1985	12/21/1988	386
Delaware	2/2/1988		5,814	North Carolina		6/8/1989	72
District of Columbia			3	North Dakota			3
Florida		6/27/1989	230	Ohio	4/11/1990	11/22/1986	164
Georgia	3/3/1988	4/7/1988	120	Oklahoma	4/9/1991		46
Hawaii		6/17/1988	6	Oregon	4/5/1991	3/5/1989	67
Idaho	3/22/1988	3/22/1988	6	Pennsylvania	3/23/1988	3/23/1988	187
Illinois	8/2/1989	8/2/1989	36	Rhode Island	7/3/1990	7/3/1990	11
Indiana	3/5/1986	3/5/1986	79	South Carolina	4/22/1988	6/9/1998	16
Iowa	5/2/1997	6/1/1989	26	South Dakota	2/20/1990	2/20/1990	4
Kansas	4/10/1989		24	Tennessee	3/11/1988	5/29/1989	60
Kentucky	3/28/1986	7/15/1988	10	Texas	5/28/1997		217
Louisiana			25	Utah		3/13/1989	76
Maine	4/6/1988	4/8/2002	10	Vermont			4
Maryland	4/11/1989	5/13/1999	98	Virginia	3/31/1988	4/2/1990	96
Massachusetts	7/18/1989	7/18/1989	198	Washington	8/11/1987	3/23/1998	91
Michigan	5/24/1989	7/23/2001	99	West Virginia			4
Minnesota	6/25/1987		255	Wisconsin	9/17/1987	9/17/1987	74
Mississippi			5	Wyoming	3/11/1989		10
Missouri	6/23/1986		48	Total Number of Firms			9,755

before these laws are enacted (1972–1984), the 13 years during which these laws are promulgated (1985–1997), and the 13 after these laws pass (1998–2010). This procedure yields an initial base sample of 274,173 firm-year observations from 24,554 unique firms. Table 1 also reports the dates on which different states enact poison pill legislation. Information on the adoption of both BC and poison pill laws is obtained from Bertrand and Mullainathan (2003) and Karpoff and Wittry (2018). Later in the article, we use poison pill laws to validate the results we obtain with BC laws.

To conduct our empirical tests, for each observation we require information on the state of incorporation (INCORP), the location of the company's headquarters (STATE), leverage ((DLC + DLTT)/(DLC + DLTT + CSHO × PRCC_F)), new external finance ((SSTK − PRSTKC) + (DLTIS − DLTR)), and the beginning- and end-of-period values of cash and marketable securities (CHE), market capitalization (PRCC_F × CSHO), earnings (IB + XINT + TXDI + ITCI), interest expense (XINT), and common dividends (DVC). We obtain these Compustat data for a sample of 117,204 firm-year observations from 12,455 unique firms. We retain firms listed in the CRSP monthly stock file with enough data to compute their size and book-to-market matched excess returns for the fiscal year (see Fama and French

(1993), Lyon, Barber, and Tsai (1999)). The final sample comprises 86,418 firm-year observations from 9,755 unique firms with complete data for all control variables.

Table 2 describes our sample. Panel A presents summary statistics for our key variables. All data are converted to real values in 2010 dollars, and we note that many exhibit characteristics comparable (in magnitude) to those reported elsewhere. For example, the average (median) cash ratio is 19% (10%). For the same ratio, Faulkender and Wang (2006) report an average (median) of 17% (9.5%). Our mean (median) market leverage is 27% (21%). Faulkender and Wang (2006) report a mean (median) leverage ratio of 28% (23%) for their sample. The mean (median) return on assets (ROA) is 8% (12%), comparable to the 7.4% (10.4%) in Giroud and Mueller (2010).

In Panel B of Table 2, we report the temporal distribution of our sample. The panel provides information about the percentage of companies affected during the years in which BC laws have been effective for at least 6 months. Panel C provides the industrial distribution of our sample using the Fama and French (1997) 48 industry groups.

A.   Excess Stock Returns

To evaluate whether BC laws affect the marginal value of cash, we proxy for the change in firm value by calculating the excess stock returns accruing to shareholders during the fiscal year. According to Panel A of Table 2, the mean (median) excess return in our sample is 1% (-8%).[5] Excess returns for firm i at time t, $r_{i,t} - R^B_{i,t}$, are calculated as the difference between the buy-and-hold returns for the sample company and that of a size and book-to-market matched benchmark over 12 months ending at the fiscal year-end date. These returns are computed as:

$$(1) \qquad r_{i,t} - R^B_{i,t} = \left[\prod_{t=-11\ldots+0} (1+\text{RET}_{i,t}) - 1 \right]$$
$$- \left[\prod_{t=-11\ldots+0} (1+\text{BENCHMARK\_RET}_{i,t}) - 1 \right],$$

where $\text{RET}_{i,t}$ is the monthly holding period return of sample firm i at time t and $\text{BENCHMARK\_RET}_{i,t}$ is the return of the benchmark portfolio. Following Fama and French (1993), the sample firms are assigned to 1 of 25 value-weighted size and book-to-market benchmark portfolios as of June of each year t. The 25 portfolios are formed from the five size quintiles using the market capitalization of NYSE firms as of June 30 of each year and five book-to-market quintiles are formed from NYSE firms as of December of the prior year. If a sample firm is delisted, the delisting value is reinvested into the benchmark portfolio.

---

[5]Comparably, for the same variable Faulkender and Wang (2006) report a mean (median) of -0.5% (-8.5%).

TABLE 2

Sample Description

Panel A of Table 2 reports the summary statistics for the 86,418 firm-year-observations from the universe of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. All variables are defined in Appendix A. Panel B reports the temporal distribution of our sample observations, the number of firms incorporated in a law with an active BC Law, and the percentage of firms in the sample year affected by a BC Law. Panel C reports the industry composition of our sample firms which are incorporated in a state with an active BC Law.

Panel A. Summary Statistics

	No. of Obs.	Mean	Std. Dev.	Q1	Median	Q3
ROA	86,418	0.08	0.26	0.06	0.12	0.18
Excess Stock Return	86,418	0.01	0.59	−0.34	−0.08	0.21
Cash	86,418	0.19	0.26	0.03	0.10	0.23
Earnings	86,418	0.07	0.27	0.01	0.08	0.17
Net Assets	86,418	1,400.29	4,312.64	37.31	154.00	698.33
Market Value of Equity	86,418	1,266.96	4,103.97	30.61	122.91	612.93
R&D	86,418	0.03	0.07	0.00	0.00	0.04
Interest	86,418	0.07	0.12	0.01	0.02	0.07
Dividends	86,418	0.01	0.02	0.00	0.00	0.02
Leverage	86,418	0.27	0.25	0.05	0.21	0.44
New Finance	86,418	0.04	0.27	−0.04	0.00	0.07
HHI	86,418	0.17	0.14	0.08	0.14	0.21
Large Industrial Customer	86,418	0.03	0.14	0.00	0.00	0.00

Panel B. Temporal Distribution

Year	Full Sample	% of Firms Affected by BC Law in Year	Non-DE Incorporated Firms in Sample	% of Non-DE Incorporated Firms Affected by BC Law in Year
1972	779	0.00	327	0.00
1973	932	0.00	389	0.00
1974	1,198	0.00	518	0.00
1975	1,388	0.00	619	0.00
1976	1,534	0.00	694	0.00
1977	1,566	0.00	720	0.00
1978	1,590	0.00	745	0.00
1979	1,613	0.00	770	0.00
1980	1,662	0.00	802	0.00
1981	1,707	0.00	850	0.00
1982	1,785	0.00	898	0.00
1983	1,963	0.00	1,010	0.00
1984	2,107	0.00	1,085	0.00
1985	2,336	0.00	1,204	0.00
1986	2,450	6.41	1,266	12.40
1987	2,536	12.74	1,308	24.69
1988	2,673	56.38	1,361	44.38
1989	2,754	76.83	1,339	52.35
1990	2,798	82.81	1,346	64.26
1991	2,808	86.36	1,332	71.25
1992	2,824	87.54	1,320	73.33
1993	2,823	87.81	1,290	73.33
1994	2,951	88.41	1,298	73.65
1995	3,059	89.11	1,310	74.58
1996	3,068	88.95	1,306	74.04
1997	3,031	89.64	1,298	75.81
1998	3,019	90.69	1,282	78.08
1999	2,848	91.12	1,188	78.70
2000	2,621	91.19	1,095	78.90
2001	2,431	91.77	983	79.65
2002	2,435	92.36	912	79.61
2003	2,329	92.87	880	81.14
2004	2,238	92.72	832	80.41
2005	2,182	92.85	799	80.48
2006	2,127	92.99	770	80.65
2007	2,100	92.86	729	79.42
2008	2,104	93.35	700	80.00
2009	2,066	93.13	693	79.51
2010	1,983	93.55	670	80.90
Total	86,418		37,938	

TABLE 2 (continued)

Sample Description

Panel C. Industry Distribution for BC Law Firms (Firms Not Covered by BC Laws Omitted)

	FF48 Industry	No. of Obs.	% of BC Law Firm-Years
1	Agriculture	166	0.31
2	Food Products	1,248	2.37
3	Candy and Soda	130	0.25
4	Beer and Liquor	203	0.39
5	Tobacco Products	91	0.17
6	Recreation	584	1.11
7	Entertainment	1,113	2.11
8	Printing and Publishing	635	1.20
9	Consumer Goods	1,214	2.30
10	Apparel	957	1.82
11	Healthcare	1,309	2.48
12	Medical Equipment	2,029	3.85
13	Pharmaceutical Products	2,674	5.07
14	Chemicals	1,273	2.41
15	Rubber and Plastic Products	707	1.34
16	Textiles	459	0.87
17	Construction Materials	1,376	2.61
18	Construction	839	1.59
19	Steel Works Etc.	1,023	1.94
20	Fabricated Products	341	0.65
21	Machinery	2,375	4.51
22	Electrical Equipment	1,009	1.91
23	Automobiles and Trucks	916	1.74
24	Aircraft	333	0.63
25	Shipbuilding, Railroad Equipment	110	0.21
26	Defense	162	0.31
27	Precious Metals	161	0.31
28	Non-Metallic and Industrial Metal Mining	179	0.34
29	Coal	97	0.18
30	Petroleum and Natural Gas	2,602	4.94
32	Communication	1,655	3.14
33	Personal Services	685	1.30
34	Business Services	5,982	11.35
35	Computers	2,503	4.75
36	Electronic Equipment	3,628	6.88
37	Measuring and Control Equipment	1,493	2.83
38	Business Supplies	1,020	1.93
39	Shipping Containers	210	0.40
40	Transportation	1,678	3.18
41	Wholesale	2,654	5.03
42	Retail	3,224	6.12%
43	Restaurants, Hotels, Motels	1,428	2.71
48	Almost Nothing	240	0.46
Total		52,715	100.00

B.   Experimental Design

To determine the marginal value of cash, we follow the research design implemented in prior literature and regress excess stock returns on the change in cash.[6] We implement three regression specifications in our primary tests. The first, which augments the experimental design in Faulkender and Wang (2006), regresses excess stock returns for firm i at time t, $r_{i,t} - R^B_{i,t}$, upon the change in cash in the presence of a BC law while controlling for the changes in a host of other firm-specific factors known to affect shareholder wealth. Our first specification, given by equation (2), is as follows:

---

[6]See, for example, Faulkender and Wang (2006), Dittmar and Mahrt-Smith (2007), Denis and Sibilkov (2010), Harford et al. (2014), Masulis and Reza (2014), Dessaint and Matray (2017), and Duchin et al. (2017).

(2)
$$r_{i,t} - R^B_{i,t} = \gamma_0 + \gamma_1 \mathrm{BC\_LAW}_{s,t} + \gamma_2 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times \mathrm{BC\_LAW}_{s,t} + \gamma_3 \frac{\Delta C_{i,t}}{M_{i,t-1}}$$
$$+ \gamma_4 \frac{\Delta E_{i,t}}{M_{i,t-1}} + \gamma_5 \frac{\Delta NA_{i,t}}{M_{i,t-1}} + \gamma_6 \frac{\Delta RD_{i,t}}{M_{i,t-1}} + \gamma_7 \frac{\Delta I_{i,t}}{M_{i,t-1}} + \gamma_8 \frac{\Delta D_{i,t}}{M_{i,t-1}}$$
$$+ \gamma_9 \frac{C_{i,t-1}}{M_{i,t-1}} + \gamma_{10} L_{i,t} + \gamma_{11} \frac{NF_{i,t}}{M_{i,t-1}},$$

where ΔX reflects the change in the variable X.[7] $BC\ Law_{s,t}$ is a (0,1) indicator variable denoting that a business combination law is effective in the state of incorporation s at time t for at least half of the fiscal year. $C_{i,t}$ and $C_{i,t-1}$ are cash and marketable securities at the end and beginning of the period (respectively), $E_{i,t}$ is earnings before interest and extraordinary items, $NA_{i,t}$ is total assets net of cash, $RD_{i,t}$ is research and development expenditures, $I_{i,t}$ is interest expense, $D_{i,t}$ is total dividends, $L_{i,t}$ is market leverage, and $NF_{i,t}$ is the net amount of external financing. We normalize all firm-level control variables by the beginning of period market capitalization ($M_{i,t-1}$). Our primary coefficient of interest, γ_2, measures the dollar change in equity value resulting from a dollar change in the firm's cash holdings after BC laws pass.

Our second specification follows from Gormley and Matsa ((2014), (2016)), which leverages the staggered adoptions of business combination laws across the U.S. along with higher order fixed effects to achieve identification. Equation (3) describes our second empirical specification.

(3) $$r_{i,t} - R^B_{i,t} = \gamma_0 + \gamma_1 \mathrm{BC\_LAW}_{s,t} + \gamma_2 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times \mathrm{BC\_LAW}_{s,t} + \gamma_3 \frac{\Delta C_{i,t}}{M_{i,t-1}} + f_i + \omega_{l,t} + \lambda_{j,t}.$$

Equation (3) controls for unobserved firm heterogeneity, time-varying differences across states, and time-varying differences across industries by including firm (f_i), HQ state-by-year ($\omega_{l,t}$), and 4-digit SIC industry-by-year ($\lambda_{j,t}$) fixed effects for a firm i, headquartered in state l, operating in industry j, at time t. The firms in our sample rarely change their state of incorporation, which could lead to serial correlation in the residuals. Therefore, as in Gormley and Matsa (2016), we cluster our robust Rogers (1993) standard errors at the state of incorporation level s.

Angrist and Pischke (2009) and Gormley and Matsa (2014) argue that including additional controls in the presence of fixed effects may lead to biased parameter estimates if they are contemporaneously affected by the identifying construct (in our case, the passage of BC Laws). Hence, equation (3) includes only the standalone BC Law indicator, the change in cash, the interaction of these variables, and the aforementioned fixed effects.

In equation (4), we expand equation (3) with the time-varying firm controls deemed important by Faulkender and Wang (2006). We therefore use equation (4) as our third specification to assess the robustness of our results.

---

[7]In some specifications, following Giroud and Mueller (2010), we augment equation (2) to additionally include firm (f_i) fixed effects.

(4)
$$r_{i,t} - R^B_{i,t} = \gamma_0 + \gamma_1 \text{BC\_LAW}_{s,t} + \gamma_2 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times \text{BC\_LAW}_{s,t}$$
$$+ \gamma_3 \frac{\Delta C_{i,t}}{M_{i,t-1}} + \gamma_4 \frac{\Delta E_{i,t}}{M_{i,t-1}} + \gamma_5 \frac{\Delta \text{NA}_{i,t}}{M_{i,t-1}} + \gamma_6 \frac{\Delta \text{RD}_{i,t}}{M_{i,t-1}} + \gamma_7 \frac{\Delta I_{i,t}}{M_{i,t-1}}$$
$$+ \gamma_8 \frac{\Delta D_{i,t}}{M_{i,t-1}} + \gamma_9 \frac{C_{i,t-1}}{M_{i,t-1}} + \gamma_{10} L_{i,t} + \gamma_{11} \frac{\text{NF}_{i,t}}{M_{i,t-1}} + f_i + \omega_{l,t} + \lambda_{j,t}.$$

Provided that our benchmark returns control for common risk factors affecting stock returns and our firm, state, and industry controls account for other idiosyncratic factors, our research design is essentially a fiscal year long event study (as in Dittmar and Mahrt-Smith (2007)).

III.   Empirical Analyses

Our empirical strategy draws on the methods used by Faulkender and Wang (2006) in a study in which they estimate the marginal value of cash using a sample of firms during the 1972–2001 period. In regressions that use excess stock returns as the dependent variable, they show that on average, an extra dollar of cash is valued by shareholders at $0.75 and that the marginal value of cash for a firm with zero cash and no leverage is $1.47.[8]

A.   Baseline Result

In the first two models of Table 3, we replicate the specification and sample period in Faulkender and Wang (2006) and obtain results that are very similar to theirs. According to model 1, we find that an extra dollar of cash is valued by shareholders at about $0.76. The estimates in model 2 indicate that the marginal value of cash for a company without cash and leverage is approximately $1.35. Moreover, as in Faulkender and Wang (2006), we also find that the marginal value of cash is decreasing with liquidity and leverage. These results suggest that there is nothing unusual about our sample.

In models 3–10 of Table 3, we examine whether the passing of BC laws affects the value of cash by using the interacted model that splits our sample firms based on whether a BC law is promulgated in a state. For this purpose, in all these models we regress excess stock returns on the change in cash (Δ Cash) interacted with the BC law indicator. This interaction term reflects the change in the marginal value of cash following the passage of a BC law.

Model 3 of Table 3 estimates equation (3) which includes the higher-order fixed effects (Gormley and Matsa (2014)). Models 4 and 5 extend the baseline specification with the controls in Faulkender and Wang (2006) as specified by equation (4).[9] Regressions 6–10 estimate different iterations of equation (2) based on the marginal value of cash tests reported by Faulkender and Wang (2006), Dittmar and Mahrt-Smith (2007), Denis and Sibilkov (2010), and Harford, Klasa, and Maxwell (2014), with model 7 being the closest to the baseline test in

---

[8]These analyses are reported in Table II in Faulkender and Wang ((2006), p. 1973)).

[9]Specifically, models 3 to 5 include firm, HQ state by year, and 4-digit SIC industry by year fixed effects.

TABLE 3

Marginal Value of Cash

Table 3 reports regressions of the effect of BC Laws on the marginal value of cash for the 86,418 firm-year-observations of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. The dependent variable in each model is the Excess Stock Return. Models 1 and 2, which replicate Faulkender and Wang (2006), are run on the 1972–2001 sample observations from their period of study. All other regressions use the full 1972–2010 sample period. Models 3 through 5 include firm, HQ state by year, and 4-digit SIC industry by year fixed effects. Models 7 and 8 run the Faulkender and Wang (2006) models in our sample period, while models 9 and 10 include firm fixed effects as in Giroud and Mueller (2010). All variables are defined in Appendix A. p-values based on robust clustered standard errors (at either the firm or state of incorporation level) are reported in parentheses below the parameter estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Excess Stock Return									
	1972–2001		1972–2010							
	1	2	3	4	5	6	7	8	9	10
BC Law × Δ Cash			0.239*** (0.001)	0.269*** (0.000)	0.216*** (0.000)	0.243*** (0.000)	0.326*** (0.000)	0.273*** (0.000)	0.287*** (0.000)	0.238*** (0.000)
BC Law			0.011 (0.440)	0.009 (0.527)	0.009 (0.479)	0.012*** (0.001)	0.005 (0.147)	0.006* (0.070)	−0.002 (0.766)	−0.000 (0.978)
Δ Cash	0.759*** (0.000)	1.354*** (0.000)	0.618*** (0.000)	0.736*** (0.000)	1.293*** (0.000)	0.712*** (0.000)	0.663*** (0.000)	1.297*** (0.000)	0.749*** (0.000)	1.336*** (0.000)
Δ Earnings	0.419*** (0.000)	0.421*** (0.000)		0.386*** (0.000)	0.386*** (0.000)		0.450*** (0.000)	0.449*** (0.000)	0.413*** (0.000)	0.413*** (0.000)
Δ Net Assets	0.199*** (0.000)	0.205*** (0.000)		0.160*** (0.000)	0.165*** (0.000)		0.191*** (0.000)	0.198*** (0.000)	0.155*** (0.000)	0.161*** (0.000)
Δ R&D	1.159*** (0.000)	1.090*** (0.000)		0.718*** (0.000)	0.685*** (0.000)		1.013*** (0.000)	0.935*** (0.000)	0.757*** (0.000)	0.724*** (0.000)
Δ Interest	−1.298*** (0.000)	−1.230*** (0.000)		−0.679*** (0.000)	−0.633*** (0.000)		−1.299*** (0.000)	−1.234*** (0.000)	−0.781*** (0.000)	−0.728*** (0.000)
Δ Dividends	3.047*** (0.000)	2.989*** (0.000)		1.910*** (0.000)	1.872*** (0.000)		2.948*** (0.000)	2.908*** (0.000)	1.623*** (0.000)	1.589*** (0.000)
Cash (t−1)	0.332*** (0.000)	0.309*** (0.000)		0.732*** (0.000)	0.716*** (0.000)		0.365*** (0.000)	0.345*** (0.000)	0.726*** (0.000)	0.705*** (0.000)
Leverage	−0.534*** (0.000)	−0.521*** (0.000)		−1.022*** (0.000)	−1.004*** (0.000)		−0.492*** (0.000)	−0.480*** (0.000)	−0.980*** (0.000)	−0.962*** (0.000)
New Finance	0.041*** (0.002)	0.020 (0.124)		0.055** (0.024)	0.036* (0.097)		0.017 (0.182)	−0.004 (0.742)	0.048*** (0.000)	0.028** (0.036)

(continued on next page)

TABLE 3 (continued)

Marginal Value of Cash

| | 1972–2001 | | Excess Stock Return | | | | | | | |
| | | | 1972–2010 | | | | | | | |
	1	2	3	4	5	6	7	8	9	10
Leverage × Δ Cash		−1.023***			−0.959***			−1.094***		−0.988***
		(0.000)			(0.000)			(0.000)		(0.000)
Cash ($t-1$) × Δ Cash		−0.438***			−0.378***			−0.422***		−0.413***
		(0.000)			(0.000)			(0.000)		(0.000)
Firm FE	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes
HQ State-Year FE	No	No	Yes	Yes	Yes	No	No	No	No	No
Industry-Year FE	No	No	Yes	Yes	Yes	No	No	No	No	No
Clustering	Firm Level		State of Incorporation Level			Firm Level				
Adj. R^2	0.202	0.214	0.123	0.291	0.299	0.050	0.202	0.214	0.238	0.248
No. of obs.	66,854	66,854	86,418	86,418	86,418	86,418	86,418	86,418	86,418	86,418

Faulkender and Wang (2006). Models 9 and 10 augment the Faulkender and Wang (2006) model (i.e., equation (2)) to include firm fixed effects as in Giroud and Mueller (2010). These fixed effects control for existing takeover defenses at the firm level.

The coefficient for the interaction term in Table 3 is positive and statistically significant in all models in which that variable is used. According to the estimates in model 3, investors value an additional dollar of cash at about $0.62 for the average firm without a BC law in place, but that value increases by about $0.24 when BC laws become effective. Models 4, 6, 8, and 10 imply similar economic effects. Models 5, 8, and 10 show that, on average, the marginal value of cash for a firm with no cash and no leverage is about $1.29, but that value increases by 22 cents to 27 cents after BC laws pass. Thus, our initial results support the hypothesis that shareholders view BC laws as consequential, as evidenced by their revaluation of internal slack. Perhaps surprisingly, that revaluation is positive.[10] In the following sections, we study this baseline finding further, given that it contrasts with the evidence in Dittmar and Mahrt-Smith (2007). They find that the marginal value of cash declines in firms with poor corporate governance—measured with the Gompers et al. (2003) index of shareholder rights and the level of institutional monitoring.

We leave robustness checks for Section III.E, where we implement additional methodological checks suggested by Gormley and Matsa ((2014), (2016)), apply all of the BC-law test fixes proposed by Karpoff and Wittry (2018), check whether Delaware incorporation (or changes in incorporation) drive our results, address the cash-regime issue raised by Halford et al. (2024), tackle the potential concern related to reusing BC laws as a natural experiment noted by Heath et al. (2023), and perform subsample and falsification tests.

B.   Quiet Life Effects

Our baseline results indicate that, on average, the marginal value of cash improves after BC laws pass. On the one hand, this result may appear counterintuitive given the thesis that, by deterring takeovers, BC laws promote managerial entrenchment, exacerbate agency problems, and induce managerial actions that are not in shareholders' best interests. This possibility conforms to the conclusion by Bertrand and Mullainathan (2003) that managers enjoy "the quiet life" after states adopt antitakeover laws. On the other hand, shielding managers from takeovers may discourage myopic investment policies and shift their focus toward long-term growth.

The above discussion suggests that, while BC laws have an average positive effect on the value of cash, the effect will differ across firms. We therefore study this conjecture in the context of quiet life firms. We follow Qui and Yu (2009) and Giroud and Mueller (2010) and use the Herfindahl–Hirschman index (HHI) to identify firms operating in noncompetitive industries. With this information, we

---

[10]It is possible that firms will rationally increase their cash balances if the marginal value of cash increases once BC laws are enacted. Gormley and Matsa (2016) find empirical evidence consistent with this conjecture. They report that average total cash holdings increase by about 13% after BC laws are adopted. We study the effect of BC laws on cash holdings in Section III.D.

TABLE 4

Takeover Protection and Cash Value for Quiet Life and Bonding Firms

Table 4 reports regressions of the effect of BC Laws on the marginal value of cash for firms for quiet life or bonding firms for the full sample of 86,418 firm-year-observations of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. The dependent variable in each model is the Excess Stock Return. Quiet Life refers to an indicator of High Herfindahl Index (HHHI) with Low Import Penetration. Bonding refers to an indicator for Low Herfindahl Index (LHHI) with Large Industrial Customer (> 66% of sales from a single customer). Additional Controls indicate that the model also contains Δ Earnings, Δ Net Assets, Δ R&D, Δ Interest, Δ Dividends, Cash ($t-1$), Leverage, and New Finance, but the estimates are omitted to conserve space. Each model includes firm, HQ state by year, and 4-digit SIC industry by year fixed effects. All variables are defined in Appendix A. p-values based on robust clustered standard errors at the state of incorporation level are reported below the parameter estimates. The joint effect of the double and triple interaction terms is reported in the last line of estimates with the p-value for statistical significance (in parentheses) as indicated. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Excess Stock Return | | | | |
	1	2	3	4	5
BC Law × Δ Cash × Quiet Life	−0.542*** (0.006)	−0.532*** (0.002)			−0.517*** (0.002)
BC Law × Δ Cash × Bonding			0.416** (0.027)	0.557*** (0.001)	0.543*** (0.002)
BC Law × Δ Cash	0.239*** (0.003)	0.272*** (0.000)	0.211*** (0.003)	0.242*** (0.000)	0.257*** (0.000)
BC Law × Quiet Life	0.069* (0.051)	0.029 (0.329)			0.030 (0.325)
BC Law × Bonding			−0.016 (0.590)	−0.000 (1.000)	0.000 (0.993)
Δ Cash × Quiet Life	−0.047 (0.837)	0.015 (0.934)			0.013 (0.940)
Δ Cash × Bonding			−0.020 (0.885)	−0.149 (0.245)	−0.148 (0.242)
Quiet Life	−0.054 (0.185)	−0.013 (0.714)			−0.013 (0.708)
Bonding			−0.015 (0.695)	−0.026 (0.511)	−0.026 (0.500)
BC Law	0.004 (0.795)	0.001 (0.935)	0.008 (0.601)	0.003 (0.870)	0.001 (0.948)
Δ Cash	0.636*** (0.000)	0.747*** (0.000)	0.635*** (0.000)	0.749*** (0.000)	0.749*** (0.000)
Additional Controls	No	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
HQ State-Year FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.116	0.287	0.116	0.287	0.287
No. of obs.	86,418	86,418	86,418	86,418	86,418
Significance of combined effects:					
[BC Law × Δ Cash] + [BC Law × Δ Cash × Quiet Life]	−0.303** (0.026)	−0.260** (0.030)			−0.260** (0.029)
[BC Law × Δ Cash] + [BC Law × Δ Cash × Bonding]			0.627*** (0.005)	0.799*** (0.000)	0.800*** (0.000)

define a *High HHI* indicator and set it to 1 for firms in the top quartile of the HHI distribution for all industries. In the spirit of Bena and Xu (2017), we create an indicator variable set to 1 for firms in the bottom quartile of import penetration. Our Quiet Life proxy interacts with the high HHI and the low import penetration indicators. To assess the differential impact across competitive and quiet life firms, we use a triple interaction term of the change in cash, the BC law indicator, and the Quiet Life proxy. This triple interaction is the key independent variable in Table 4 and reflects the difference in the value of cash following the passage of a BC law for

quiet life firms. These tests are variants of equations (3) and (4), which use excess stock returns as the dependent variable.

Estimates for the double interaction term [BC Law × Δ Cash] are positive and significant whereas those for the triple interaction term [BC Law × Δ Cash × Quiet Life] are negative and significant in models 1 and 2 of Table 4. The joint effect of these interactions indicates that, following the ratification of BC laws, the value of an extra dollar of cash increases for the average firm but decreases for quiet life firms. According to the estimates, an extra dollar of cash is about 26 to 30 cents *lower* for quiet life firms after BC laws pass.[11] This evidence concurs with the agency-based hypothesis that managerial preference for a quiet life negatively impacts firm value.[12]

Importantly, the results in Table 4, which are consistent with those by Dittmar and Mahrt-Smith (2007) showing that cash is less valuable when agency problems are more severe, conform to the "quiet life" agency model in which managers use internal slack to reduce effort and risk to their income stream (see Hicks (1935), Bertrand and Mullainathan (2003)) or physical health (Borgschulte, Guenzel, Liu, and Malmendier (2021)). The findings support the joint hypothesis that shareholders view the enactment of BC laws as negatively impacting the control of agency problems and that some managers use the decrease in external control threat to shift toward quiet life preferences.

C.   Further Analysis: The Bonding Hypothesis

Johnson et al. (2015) argue that by bonding its implicit commitments with major business counterparties, some firms can obtain advantageous contracting terms that increase their value. They conjecture that establishing personal connections with these counterparties leads to the undertaking of profitable collaborations that might not otherwise occur without such relationships. Provisions that lower takeover probabilities increase incentives to invest in the relationship by mitigating the possibility that acquiring managers will rescind the implicit commitments made by their predecessors to large customers and other major stakeholders (Shleifer and Summers (1988)). Johnson et al. (2015) refer to this as the bonding hypothesis of takeover defenses and empirically evaluate it in a sample of IPO firms. Their results, which support this hypothesis, show that IPO firms' valuation and operating performance are positively related to the use of takeover defenses when they have a key business tie such as a major customer.

Based on the evidence from Johnson et al. (2015), we identify firms with a major industrial customer in the Compustat customer segment database to create a *Bonding* indicator variable. With this information, in Table 4 we run the marginal value of cash regressions to test the bonding hypothesis by using [BC Law × Δ Cash × Bonding] as the key independent variable. In models 3 and 4 the bonding proxy is an indicator that

---

[11]The marginal value of cash estimates for the Quiet Life firms reflect the joint effect of the double and triple interaction terms reported in Table 4. In model 1, the computation is $-0.303 = 0.239 + -0.542$, while in model 2 the computation is $-0.260 = 0.272 + -0.532$.

[12]Using the continuous HHI variable (instead of the high HHI indicator) produces results that yield inferences like those from the tabulated test. Raising the HHI by 1 standard deviation lowers the marginal value of cash by $0.18.

is set to 1 if, during the calendar year, the firm reports a major industrial customer, and set to 0 otherwise.[13] The estimates in these models show that, after BC laws pass, cash appreciates (by 63 to 80 cents) for firms with a major industrial customer.[14] This evidence supports the bonding hypothesis.

In model 5 of Table 4, we include both the quiet life and bonding indicators along with their respective double and triple interaction terms in the same regression specification. Concurrently estimating the quiet life and bonding effects does not change the inferences.

D.   Cash Balances

Our baseline results show that the average value of internal slack appreciates once BC legislation passes. Given this finding, we conjecture that managers in firms experiencing an increase in their marginal value of cash should optimally accumulate more cash. We test this possibility in Table 5.

The results in columns 1 and 2 of Table 5 show that average total cash holdings increase by 4.3 log points (or about 4.4%) once BC laws pass. This evidence corroborates the findings by Gormley and Matsa (2016) of an average increase in cash balances after BC laws pass. In model 3, we include the Quiet Life indicator term and in model 4 we add the Bonding indicator. According to our estimates in the remaining columns of Table 5, bonding firms drive the increase in total cash holdings after BC laws pass. Consistent with our conjecture, bonding firms increase their average cash holdings by about 13.3%. We observe that despite the increase in cash holdings, we still observe an increase in the value of cash. Given this, it is possible that managers choose not (or are unable) to increase cash to the point where its marginal value reverts to its pre-BC law level.[15] Notably, the joint effect for quiet life companies is not significantly different from zero, suggesting that these firms do not meaningfully alter their cash balances after BC laws pass.

E.   Robustness Tests

We note that the increases in the market-assessed value of internal slack reported in Table 3 obtain after controlling for unobserved time-invariant firm characteristics, time-varying differences across states, and time-varying industry effects. Next, we probe the preceding findings by including additional control variables that could confound the effect of BC legislation.

1.   First Generation State Anti-Takeover Laws

Karpoff and Wittry (2018) show that the effect (size, direction, and significance) of BC laws depends on other state antitakeover laws and on the legal regime as reflected in important court decisions. To address this issue, in columns 1 and 2 of Panel A in Table 6, we expand the tests presented in Table 3 with the additional

---

[13]To prevent overlap with quiet life firms and ensure a focus on business competition, the bonding proxy focuses on firms in the Low HHI subgroup (i.e., the bonding firm is in a competitive industry).

[14]The joint effects for the Bonding Firms reported in Table 4, models 3 and 4 are likewise estimated as $0.627 = 0.211 + 0.416$ and $0.799 = 0.242 + 0.577$, respectively.

[15]Dittmar and Duchin (2010) find slow speed of adjustment toward target cash balances and find evidence of significant adjustment costs.

TABLE 5

Takeover Protection and the Change in Cash Balances

Table 5 reports regressions of the effect of BC Laws on the change in cash balances for the full sample of 86,418 firm-year-observations of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. The dependent variable in each model is the natural log of cash. Quiet Life refers to an indicator of High Herfindahl Index (HHHI) with Low Import Penetration. Bonding refers to an indicator for Low Herfindahl Index (LHHI) with Large Industrial Customer (>66% of sales from a single customer). Additional Controls indicates that the model also contains Δ Earnings, Δ Net Assets, Δ R&D, Δ Interest, Δ Dividends, Cash ($t-1$), Leverage, and New Finance, but the estimates are omitted to conserve space. Each model includes firm or firm, HQ state by year, and industry by year fixed effects. All variables are defined in Appendix A. p-values based on robust clustered standard errors at the state of incorporation level are reported in parentheses below the parameter estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ln(Cash)				
	1	2	3	4	5
BC Law	0.040**	0.043**	0.044**	0.042**	0.043**
	(0.027)	(0.027)	(0.023)	(0.030)	(0.026)
BC Law × Quiet Life			−0.030		−0.029
			(0.572)		(0.582)
Quiet Life			−0.024		−0.024
			(0.576)		(0.572)
BC Law × Bonding				0.083*	0.082*
				(0.062)	(0.065)
Bonding				0.002	0.003
				(0.962)	(0.953)
Additional Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
HQ State-Year FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.818	0.843	0.843	0.843	0.843
No. of obs.	86,418	86,418	86,418	86,418	86,418
Combined effect significance:					
BC Law + [BC Law × Quiet Life]			0.014		0.014
			(0.797)		(0.799)
BC Law + [BC Law × Bonding]				0.125***	0.125***
				(0.008)	(0.008)

independent variables recommended by Karpoff and Wittry (2018). These variables control for states where first-generation takeover laws were passed, for several important court decisions (e.g., *Moran v. Household International, CTS Corp. v. Dynamics Corp.*) and for the existence of other related laws (e.g., Fair Price, Control Share Acquisition). Appendix A provides a detailed description of all of these (and other) variables.[16] The key explanatory variable in all tests is the BC law indicator interacted with the change in cash.

Giroud and Mueller (2010) note that most state antitakeover laws were adopted during emergency sessions under the political pressure of a single firm facing a takeover threat. They argue that such pattern mitigates the concern that BC laws were passed due to lobbying by a broad coalition of firms. Notwithstanding this, Karpoff and Wittry (2018) identify firms that lobbied for business combination legislation and argue that explicitly controlling for them is necessary to evaluate the effect of these laws.

The possibility that BC laws pass because of lobbying raises questions related to reverse causality. In our tests, the inclusion of state- and industry-year fixed

---

[16]Karpoff and Wittry (2018) provide a comprehensive explanation for each of the various relevant case law decisions.

effects lessens the reverse causality concern that the enactment of BC laws results from lobbying at the local and industry levels, respectively. Nevertheless, to further mitigate this issue, the tests in Panel A of Table 6 include the lobbying control variable suggested by Karpoff and Wittry (2018).

The results in columns 1 and 2 in Panel A of Table 6 continue to show that shareholders value an extra dollar of cash more highly after BC laws are promulgated. This evidence indicates that our baseline results are robust to the potential

TABLE 6

Robustness Tests

Table 6 reports robustness tests for the regressions of the effect of BC Laws on the marginal value of cash for U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data. In Panel A, we control for the other contemporaneous state anti-takeover laws passed identified by Karpoff and Wittry (2018). The dependent variable in each model is the Excess Stock Return. Models 1 and 2 use our full 1972–2010 sample period, while models 3 and 4 are run on the 1983–2010 sample years which follow the 1982 *Edgar v. MITE Corp.* SCOTUS decision which overturned the so-called first-generation state anti-takeover laws. In Panel B, we bifurcate the BC Law indicator by whether the sample firm is incorporated in Delaware or incorporated in a state other than Delaware (models 1 and 2). In models 3 and 4, we exclude those firms which reincorporate at some point in their histories during our sample period. Additional Controls indicate that the model also contains Δ Earnings, Δ Net Assets, Δ R&D, Δ Interest, Δ Dividends, Cash ($t − 1$), Leverage, and New Finance, but the estimates are omitted to conserve space. Models 1 and 3 include firm fixed effects, while Models 2 and 4 include firm, HQ state by year, and 4-digit SIC industry by year fixed effects. All variables are defined in Appendix A. *p*-values based on robust clustered standard errors (at either the firm or state of incorporation level) are reported in parentheses below the parameter estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Karpoff and Wittry (2018) Controls

	Excess Stock Return			
	1972–2010		1983–2010	
	1	2	3	4
BC Law × Δ Cash	0.286***	0.269***	0.192***	0.180*
	(0.000)	(0.000)	(0.000)	(0.064)
BC Law	0.001	0.006	0.005	−0.006
	(0.863)	(0.652)	(0.545)	(0.744)
BC Law Motivating Firm	0.036	0.001	0.004	−0.023
	(0.219)	(0.988)	(0.932)	(0.532)
CSA Law	−0.014	0.011	−0.007	0.033
	(0.437)	(0.476)	(0.752)	(0.189)
CTS Corp v. Dynamics Corp	0.020	−0.013	0.029	−0.038
	(0.254)	(0.444)	(0.155)	(0.142)
Poison Pill Law	0.001	−0.008	0.005	0.001
	(0.904)	(0.652)	(0.702)	(0.978)
Moran v. Household Intl	−0.042***	−0.018	−0.002	−0.038***
	(0.000)	(0.132)	(0.887)	(0.001)
DD Law	−0.017	0.004	−0.001	0.008
	(0.200)	(0.815)	(0.921)	(0.680)
FP Law	−0.005	0.005	−0.014	−0.013
	(0.651)	(0.730)	(0.364)	(0.359)
First Gen ATP Law	−0.048***	−0.064***		
	(0.000)	(0.000)		
Δ Cash	0.752***	0.736***	0.875***	0.854***
	(0.000)	(0.000)	(0.000)	(0.000)
Additional Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
HQ State-Year FE	No	Yes	No	Yes
Industry-Year FE	No	Yes	No	Yes
Clustering	Firm Level	State of Incorporation Level	Firm Level	State of Incorporation Level
Adj. R^2	0.239	0.291	0.241	0.285
No. of obs.	86,418	86,418	70,664	70,664

TABLE 6 (continued)

Robustness Tests

Panel B. Cash Value by Delaware and Excluding Reincorporation Events

	Excess Stock Return			
	Delaware Firms		No Reincorporation Events	
	1	2	3	4
Delaware BC Law × Δ Cash	0.262***	0.272***		
	(0.001)	(0.000)		
Non-Delaware BC Law × Δ Cash	0.286***	0.324***		
	(0.001)	(0.000)		
Delaware BC Law	0.000	−0.012		
	(0.994)	(0.102)		
Non-Delaware BC Law	0.018	0.011		
	(0.260)	(0.142)		
BC Law × Δ Cash			0.275***	0.245***
			(0.000)	(0.000)
BC Law			0.000	0.018
			(0.969)	(0.130)
Δ Cash	0.736***	0.749***	0.754***	0.744***
	(0.000)	(0.000)	(0.000)	(0.000)
Additional Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
HQ State-Year FE	No	Yes	No	Yes
Industry-Year FE	No	Yes	No	Yes
Clustering	Firm Level	State of Incorporation Level	Firm Level	State of Incorporation Level
Adj. R^2	0.291	0.238	0.239	0.291
No. of obs.	86,418	86,418	76,725	76,725

caveats identified by Karpoff and Wittry (2018) associated with the staggered adoption of state BC laws.

An additional issue of concern (also noted by Karpoff and Wittry (2018)) is that first-generation antitakeover laws were in effect until June 1982 when the U.S. Supreme Court decided in *Edgar v. MITE Corp.* that these laws were unconstitutional. Karpoff and Wittry (2018) argue that, even with the inclusion of year-fixed effects, testing the impact of BC laws in samples that include the pre-1982 period may generate biased results because the estimates are interpreted relative to the existence of the takeover deterrence provided by the first-generation laws. Columns 3 and 4 of Panel A of Table 6 address this concern by limiting our sample to the post-1982 (until 2010) period, which removes the confounding effect from the first-generation laws. The results also show that the marginal value of cash improves (by about 18 to 19 cents) after BC laws are enacted.[17]

[17]The effect of BC laws on the marginal value of cash might be muted in firms with numerous antitakeover provisions. We address this issue in untabulated tests (like those in model 4 of Table 3) by adding the Gompers et al. (2003) index as a control variable as well as its interaction with ΔCash. While the inclusion of this control considerably lowers the number of observations, we still find that the marginal value of cash significantly increases after BC laws pass. Notably, the coefficient for the interaction term between the G-index and the ΔCash is negative, indicating an inverse association between the number of antitakeover provisions and the marginal value of cash (Dittmar and Mahrt-Smith

2. Delaware Incorporation

Since a large fraction of the firms we analyze are incorporated in Delaware, we test whether our baseline result obtains in both Delaware and non-Delaware companies. We account for the effect of reincorporation to other states and also consider whether the strength of the BC legislation alters its effect on the marginal value of cash. Armstrong et al. (2012) note that a valid concern arises when examining the effect of BC laws because the majority of U.S. firms are incorporated in Delaware. In their sample, approximately 60% of the companies are incorporated in Delaware. Comparably, the incidence of Delaware firms is also 60% (=5,814/9,755) in our sample (Table 1).

As in Armstrong et al. (2012), we split the BC Law indicator into Delaware BC Law and Non-Delaware BC Law dummies, which are respectively set to 1 if the firm year is from Delaware or another state with BC laws during a year in which these laws were in effect, and set to 0 otherwise. We use the split indicators in the marginal value of cash regressions reported as models 1 and 2 in Panel B of Table 6. These tests examine whether the effect of BC laws solely accrues to Delaware firms or can be found in firms incorporated in other states. The results show that, regardless of the state where companies are incorporated, the marginal value of cash increases after BC legislation passes. Notably, the appreciation in the value of an extra dollar of cash in both groups is similar. Parameter estimates for the Delaware × BC Law and Non-Delaware × BC Law interactions are not statistically different from one another. In models 1 and 2 of Panel B, the p-values for differences in the coefficients of these variables are 0.8931 and 0.4390, respectively.

Models 1 and 2 in Panel B of Table 6 also address the concern of Cain et al. (2017), who question the use of BC laws as an effective proxy for corporate governance. They point to the lack of variation for this proxy in their data, observing that while no firms are covered by BC laws pre-1983, most are covered by 1990. While this pattern is essentially similar in our overall sample as well, we note that there is considerably more variation in BC coverage for non-Delaware incorporated firms (see the last column of Panel B of Table 2). Therefore, the evidence for non-Delaware firms in models 1 and 2 of Panel B of Table 6 strengthens the view that these laws are consequential and well-suited to proxy for corporate governance.

A related concern is reincorporation since CRSP and Compustat assign firms to their current state of incorporation. To address this issue, we examine historical firm incorporation data and find that about 8.4% (or 823 out of 9,755) of the unique firms we study reincorporate at least once during our sample period (Gormley and Matsa (2016) find 6%).[18] Removing firm years affected by a reincorporation does not change our results. Notably, in untabulated regressions, we analyze firms that move from a state without a BC law to one in which such legislation is already in effect. The estimates indicate that, on average, the marginal value of cash increases by approximately 17 cents for those firms. These checks mitigate concerns that firm reincorporation events meaningfully affect our findings.

(2007) find an analogous result). However, the interaction term narrowly misses statistical significance (p-value = 0.133).

   [18]We begin with the reincorporation data used by Heron and Lewellen (1998) and complement it with our own searches of historical records kept by Compustat.

3. Subsample Analyses and Falsification Tests

To assess whether our data are unusual, we use the subperiod of our sample that overlaps with the time period examined by Bertrand and Mullainathan (2003). Therefore, we limit the analyses of models 1 and 2 in Panel A of Table 7 to observations that occur during 1976–1995. By using this sample period, we exclude the passing of BC laws by Iowa and Texas in 1997. Giroud and Mueller (2010)

TABLE 7

Subsample and Falsification Tests

Table 7 reports subsample tests and falsification tests of the effect of BC Laws on the marginal value of cash. We estimate models 1 and 2 in Panel A using the 1976–1995 sample as in Bertrand and Mullainathan (2003) and Giroud and Mueller (2010). Those tests closely follow the specification of Giroud and Mueller (2010). Regressions 3 and 4 in Panel A and models 1 and 2 in Panel B use our full sample. The dependent variable in models 1 and 3 in Panel A and model 1 in Panel B is ROA and the dependent variable in Panel A models 2 and 4 and Panel B model 2 is the Excess Stock Return. Ind-Year Avg DV and State-Year Avg DV are the industry-year and state-year averages of the dependent variable in the model. Firm Size is the natural log of assets and Firm Age is the natural log of number of years the firm is listed on CRSP or Compustat. BC Year (−1) indicates the firm is incorporated in a state that will pass a BC Law 1 year hence. BC Year (0) indicates the firm is incorporated in a state that will pass a BC Law in the fiscal year. BC Year (1) and BC Year (2+) indicates the firm is incorporated in a state that passed a BC Law in the prior fiscal year or 2 or more years prior, respectively. Models 1 and 2 of Panel B include the additional control variables contained in Panel A, but the estimates are omitted to conserve space. All models include firm and year-fixed effects. All other variables are defined in Appendix A. We report p-values based on Rogers (1993) robust clustered standard errors (at the state of incorporation level) in parentheses below the parameter estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. G&M (2010) Replication and Subsample Tests

	ROA	Excess Stock Return	ROA	Excess Stock Return
	1	2	3	4
BC Law	0.007*** (0.011)	0.021** (0.038)	0.006* (0.077)	0.005 (0.547)
BC Law × HHI	−0.029*** (0.001)	−0.066** (0.031)	−0.026*** (0.000)	0.016 (0.476)
BC Law × Δ Cash × HHI		−0.923*** (0.000)		−0.809*** (0.004)
BC Law × Δ Cash		0.213** (0.018)		0.291*** (0.000)
Δ Cash × HHI		−0.076 (0.797)		−0.067 (0.751)
Ind-Year Avg DV	0.000*** (0.004)	0.352*** (0.000)	0.001*** (0.000)	0.259*** (0.000)
State-Year Avg DV	−0.001 (0.645)	0.384*** (0.000)	0.000 (0.840)	0.361*** (0.000)
Firm Size	0.045*** (0.000)	−0.062*** (0.000)	0.058*** (0.000)	−0.040*** (0.000)
Firm Size Squared	−0.003*** (0.000)	−0.003*** (0.005)	−0.004*** (0.000)	−0.002*** (0.003)
Firm Age	−0.028*** (0.000)	0.010 (0.478)	−0.027*** (0.000)	−0.025*** (0.006)
HHI	0.035*** (0.005)	0.041 (0.434)	0.040*** (0.000)	0.017 (0.550)
Δ Cash		0.637*** (0.000)		0.582*** (0.000)
Constant	0.080*** (0.000)	0.287*** (0.000)	0.002 (0.900)	0.203*** (0.000)
Firm FE Year FE	Yes Yes	Yes Yes	Yes Yes	Yes Yes
Sample	1976–1995		1972–2010	
Adj. R^2 No. of obs.	0.056 51,571	0.075 51,306	0.054 85,905	0.072 85,891

TABLE 7 (continued)

Subsample and Falsification Tests

Panel B. Falsification Tests

	ROA	Excess Stock Return
	1	2
BC Year (−1) × Δ Cash × HHI		−0.064
		(0.942)
BC Year (0) × Δ Cash × HHI		−1.529*
		(0.060)
BC Year (1) × Δ Cash × HHI		−1.345**
		(0.034)
BC Year (2+) × Δ Cash × HHI		−0.702***
		(0.010)
BC Year (−1) × Δ Cash		0.147
		(0.540)
BC Year (0) × Δ Cash		0.304*
		(0.100)
BC Year (1) × Δ Cash		0.305
		(0.105)
BC Year (2+) × Δ Cash		0.297***
		(0.000)
BC Year (-1) × HHI	−0.010	−0.001
	(0.384)	(0.989)
BC Year (0) × HHI	−0.022**	0.162***
	(0.035)	(0.002)
BC Year (1) × HHI	−0.017*	0.069
	(0.098)	(0.324)
BC Year (2+) × HHI	−0.029***	−0.005
	(0.000)	(0.868)
BC Year (-1)	0.006	−0.015
	(0.160)	(0.254)
BC Year (0)	0.006**	−0.026
	(0.027)	(0.111)
BC Year (1)	0.006	−0.008
	(0.159)	(0.667)
BC Year (2+)	0.007	0.003
	(0.146)	(0.773)
HHI	0.041***	−0.059
	(0.000)	(0.767)
Δ Cash × HHI		−0.059
		(0.767)
Δ Cash		0.572***
		(0.000)
Firm FE	Yes	Yes
Year FE	Yes	Yes
Sample	1972–2010	
Adjusted R-squared	0.054	0.072
Observations	85,905	85,891

consider the same time period to evaluate whether the passing of BC laws affects operating performance, and we construct this sample following their research design. They find that treated companies experience a significant decline in operating performance after the laws' passage that gets stronger as industry competition decreases.

Model 1 in Panel A of Table 7 evaluates whether the passing of BC laws affects operating performance. We use ROA as our dependent variable to proxy for operating performance as in Giroud and Mueller (2010). All our right-hand

variables (industry and state profitability, firm size, firm age, and HHI) are similar to theirs, including the main explanatory variable, which interacts the BC law indicator with the Herfindahl index (HHI). Like the evidence in Giroud and Mueller (2010) (Table 2, Panel A, model 2, p. 318), our estimates in model 1 of Panel A shows an inverse association between operating performance and industry concentration after BC laws pass. Giroud and Mueller (2010) report that a single standard-deviation increase in the HHI is associated with a reduction in ROA of 0.5 percentage points (-0.033×0.156). In our data, a similar reduction is associated with a drop in ROA of 0.4 percentage points (-0.029×0.14).

In model 2, we replace the ROA dependent variable with excess stock returns. In that test, the independent variable of interest is a triple interaction consisting of the BC law indicator, the change in cash, and the Herfindahl index. As with our previous analyses, in model 2 of Panel A, we find that the marginal value of cash increases after the laws' enactment (by about 21 cents) and that industry concentration depresses such increase. A 1-standard-deviation increase in the HHI lowers the gains in the value of cash by 13 cents ($-0.923 \times 0.14 = -0.13$).

The tests in columns 3 and 4 (Panel A of Table 7), which we estimate using our full 1972–2010 sample, produce inferences like those in columns 1 and 2. Once BC laws are enacted, increasing HHI by 1 standard deviation is associated with a decline in ROA of 0.36 percentage points (column 3) and with an 11 cent drop in the marginal value of cash (column 4).

Even though our research design accounts for several dimensions of unobserved heterogeneity, we are conscientious of the possibility of reverse causality. To mitigate this concern, Roberts and Whited (2013) suggest running "falsification" tests with the implementation of placebo indicator variables around the dates of interest.

Panel B of Table 7 presents falsification tests of the effect of BC laws. Specifically, we create dummy variables to falsify the year in which the laws are enacted. Thus, BC Year (-1) is coded as 1 for the year before the law passes, and BC Year ($+1$) is coded as 1 for the year after the laws pass. The actual year in which the law is promulgated is BC Year (0). The falsification tests are important because they mitigate both anticipation and reverse causality concerns. In the spirit of Giroud and Mueller (2010), model 1 of Panel B uses ROA as the dependent variable and that regression produces results similar to theirs. Specifically, we find an inverse association between industry concentration and operating performance starting at the time of the passage of BC laws but no association beforehand.

Model 2 of Panel B repeats the falsification procedure in the context of the marginal value of cash. The results show that the association between the value of cash and industry concentration starts when BC laws pass and continue every year thereafter. Importantly, model 2 also shows that such association does not exist before the laws' passage. This evidence is consistent with a causal interpretation of our results.

4.   Reusing Natural Experiments

Work by Heath et al. (2023) suggests that the more researchers use the same natural experiment the higher the chance of false discoveries. Based on comprehensive empirical analyses, they find that many results in the existing literature are

false positives. According to Heath et al. (2023), a new result that relies on the BC laws natural experiment is less vulnerable to the false discoveries concern if it exhibits a *t*-statistic above 3.0.

We note that the increase in the marginal value of cash estimates reported in Table 3 has associated *t*-statistics ranging from 3.48 (column 3) to 8.83 (column 7). These *t*-statistics exceed the critical value of 3.0 noted by Heath et al. (2023). Despite this, and to more closely follow the process implemented by Heath et al. (2023), we replicate our baseline analyses reported in Table 3 in a separate test that uses the same control variables, fixed effects, and sample period used by Heath et al. (2023).[19] Equation (5) reports the regression estimates and their corresponding *t*-statistics in parentheses.

$$(5) \quad r_{i,t} - R_{i,t}^B = \underset{\substack{0.4949 \\ (6.94)}}{\gamma_0} + \underset{\substack{0.0099 \\ (0.68)}}{\gamma_1 BC\,Laws_{s,t}} + \underset{\substack{0.2575 \\ (3.55)}}{\gamma_2 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times BC\,Laws_{s,t}} + \underset{\substack{0.6209 \\ (9.78)}}{\gamma_3 \frac{\Delta C_{i,t}}{M_{i,t-1}}} + \gamma_4 X_{i,t} + f_i + \omega_{l,t} + \lambda_{j,t}.$$

Consistent with our baseline tests, the results in equation (5) show that the marginal value of cash rises by 26 cents after BC laws pass. Importantly, the *t*-statistic for this variable, 3.55, is above the critical value noted by Heath et al. (2023) suggesting that our novel findings are not false.

5.   Other Robustness Tests

Gormley and Matsa ((2014), (2016)), advise against adjusting stock returns when using them as the dependent variable in regression analyses and instead recommend implementing their fixed-effects methods. We follow their advice by using raw returns in untabulated regressions with industry-year fixed effects and find that once BC laws are enacted, the value on an extra dollar increases by 20 cents to 22 cents. These findings agree with our baseline results.

F.   Alternative Antitakeover Protection: Poison Pills

It is possible that BC laws are inherently different from other antitakeover devices such as poison pills. BC laws represent an act of the state government (legislature and governor) that signals the value the state places on companies incorporated there. A state business combination law effectively certifies the target board's just-say-no approach to a takeover bid. Poison pill provisions, on the other hand, are implemented by management and the board and may be more assailable by outside pressure to negotiate with the bidder.[20] A related factor is that the implications of antitakeover protection for the valuation of cash can change over

---

[19]Heath et al. (2023) analyzed 88,648 firm-year observations for 10,213 firms from 1976 to 1995, excluding financial firms, utilities, and observations with missing/negative sales or total assets. The control variables ($X_{i,t}$) they use are like those in Karpoff and Wittry (2018) including the natural log of book value of assets (size), size squared, firm age, and firm age squared. Heath et al. (2023) also include firm (f_i), state of location-year ($\omega_{l,t}$), and industry-year ($\lambda_{j,t}$) fixed effects and standard errors are clustered at the firm level. Our regression reported as equation (5)) is similarly specified, it analyzes 86,418 firm-year observations for 9,755 firms (i.e., our full sample).

[20]Please see Table II and the Appendix in Karpoff and Wittry (2018) for a summary of the institutional details of poison pill laws and the timing of their staggered adoption.

TABLE 8

Takeover Protection and Cash Value for Quiet Life and Bonding Firms

Table 8 reports regressions of the effect of PP Laws on the marginal value of cash for firms for quiet life or bonding firms for the full sample of 86,418 firm-year-observations of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. The dependent variable in each model is the Excess Stock Return. Quiet Life refers to an indicator of High Herfindahl Index (HHHI) with Low Import Penetration. Bonding refers to an indicator for Low Herfindahl Index (LHHI) with Large Industrial Customer (>66% of sales from a single customer). Additional Controls indicate that the model also contains Δ Earnings, Δ Net Assets, Δ R&D, Δ Interest, Δ Dividends, Cash ($t-1$), Leverage, and New Finance, but the estimates are omitted to conserve space. Each model includes firm, HQ state by year, and 4-digit SIC industry by year fixed effects. All variables are defined in Appendix A. p-values based on robust clustered standard errors at the state of incorporation level are reported below the parameter estimates. The joint effect of the double and triple interaction terms is reported in the last line of estimates with the p-value for statistical significance as indicated. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Excess Stock Return					
	1	2	3	4	5	6
PP Law × Δ Cash × Quiet Life		−0.474*** (0.002)	−0.393*** (0.007)			−0.380*** (0.009)
PP Law × Δ Cash × Bonding				0.302* (0.060)	0.490*** (0.004)	(0.005)
PP Law × Δ Cash	0.212*** (0.000)	0.225*** (0.000)	0.239*** (0.000)	0.200*** (0.000)	0.214*** (0.000)	0.225*** (0.000)
PP Law × Quiet Life		0.037 (0.350)	−0.061 (0.113)			−0.062 (0.111)
PP Law × Bonding				−0.014 (0.732)	−0.027 (0.569)	−0.028 (0.558)
Δ Cash × Quiet Life		−0.060 (0.770)	−0.037 (0.816)			−0.038 (0.812)
Δ Cash × Bonding				0.061 (0.635)	−0.121 (0.427)	−0.124 (0.411)
Quiet Life		−0.029 (0.499)	0.063 (0.146)			0.063 (0.145)
Bonding				−0.014 (0.775)	−0.001 (0.987)	−0.000 (0.995)
PP Law	−0.001 (0.938)	−0.002 (0.821)	−0.001 (0.962)	−0.000 (0.964)	−0.003 (0.843)	−0.000 (0.995)
Δ Cash	0.628*** (0.000)	0.630*** (0.000)	0.751*** (0.000)	0.627*** (0.000)	0.751*** (0.000)	0.752*** (0.000)
[PP Law × Δ Cash] + [PP Law × Δ Cash × Quiet Life]		−0.249* (0.093)	−0.154 (0.268)			−0.155 (0.266)
[PP Law × Δ Cash] + [PP Law × Δ Cash × Bonding]				0.502*** (0.003)	0.713*** (0.000)	0.715*** (0.000)
Additional Controls	No	No	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.116	0.116	0.287	0.116	0.287	0.287
No. of obs.	86,418	86,418	86,418	86,418	86,418	86,418

time as internal governance strengthens. A different possibility is that both poison pills and BC laws might deter takeovers, but their effect (on the marginal value of cash) changes according to certain firm characteristics.

In the context of this study, the above conjectures suggest that the effect of BC laws on the value of an extra dollar of cash does not have to be the same as the effect poison pills may have on cash. To study this possibility, in Table 8 we estimate the marginal value of cash regressions using different interaction terms to evaluate the

quiet life hypothesis in firms with poison pill coverage. For this purpose, we define a poison pill indicator (*PP Law*) that is equal to 1 whenever a firm is incorporated in a state that passes a law or adjudicates a case establishing the right of incorporated firms to deploy a poison pill. The indicator is equal to 0 otherwise.

Consistent with our baseline evidence, model 1 shows that, on average, the marginal value of 1 dollar is about $0.63 for the average firm without a PP law in place, but that value increases by about $0.21 when PP laws pass. Next, we identify quiet life firms with a process analogous to that in Table 4. Specifically, to track quiet life firms, models 2 and 3 in Table 8 use our proxy tracking high HHI firms subject to low import penetration. The estimates reveal that when pill laws are present, the marginal value of cash is about 15 to 25 cents *lower* for quiet life firms.

Together with our previous results, the results indicate that, regardless of the way antitakeover protection is measured, such protection increases the marginal value of cash for the average company. However, our tests continue to show that the value of an extra dollar is lower in quiet-life companies. Consequently, our evidence is consistent with "quiet life" models of free cash flow, in which managers use cash on hand to simplify their jobs by exerting less effort (Hicks (1935), Bertrand and Mullainathan (2003)).

The evidence in models 2 and 3 of Table 8 indicates that internal slack depreciates in quiet-life firms once poison pill laws are ratified. In contrast, after such ratifications, the results in models 4 and 5 of Table 8 show that the value of an extra dollar rises in firms with key business ties to other companies. As with the bonding results in Table 4, those in models 4 and 5 of Table 8 (which use the same bonding proxy) reaffirm the view that takeover defenses can be beneficial for firms that need to credibly commit to a business relation with a major counterparty. To probe our PP law findings, we use both the quiet life and bonding-related variables in model 6 of Table 8. Our results are robust to the simultaneous estimation of bonding and quiet life effects.

G.   Channels

Our baseline findings that the marginal value of cash increases after states adopt BC laws are driven by bonding firms. As we discuss in the introduction, there are several specific channels through which increased bonding ability would affect the marginal value of cash. In this section, we discuss the results of testing for evidence that these channels are at work.

1.   Costly External Financing and Investment Opportunities

To test the bonding hypothesis, we flag firms that have a key industrial client, positing that the adoption of BC laws helps such firms commit to the relationship, increasing the relationship specific investment that the client is willing to make. That investment, in turn, will increase the value of similar investments and growth opportunities for the bonding firm. Thus, the marginal value of cash will work through increasing bonding firms' ability to invest in their growth opportunities, which is a function of having the opportunities in the first place, and having access to funding to finance the investment (hence the increase in the value of internal cash). A feedback effect occurs whereby increased relationship-specific investment

results in higher costs of financial distress due to the potential lost rents from the relationship. This effect would further increase the value of internal cash.

Models 1 and 2 of Table 1 in the Supplementary Material report regression analyses (based on model 9 of Table 3) where we distinguish high-investment opportunities firms as those in the top 30th percentile in terms of Tobin's Q (like in Faulkender and Wang (2006)). We find similar results using industry-level R&D-based measures of growth opportunities as in Korteweg (2007). These tests show that after BC laws pass, the marginal value of cash for firms with high investment opportunities increases by 52 cents but by only 8 cents for firms with low investment opportunities (difference = 44 cents; p-value = 0.000). Notably, when comparing bonding and quiet-life firms, a chi-square test indicates that a significantly larger proportion of our bonding firms (~54%) exhibit high investment opportunities. Notably, only 15% of bonding firms are classified as having low investment opportunities.

2. Overinvestment

Business combination laws may foster managerial entrenchment in some firms, which in turn, could lower their marginal value of cash due to agency problems. While we focus on quiet-life manifestation of agency problems, the overinvestment channel could also be present in the data. It is notoriously difficult to identify overinvestment at the firm level, so to explore this possibility, we consider firms that belong to overinvestment-prone industries. Such industries are defined as those in the top 30th percentile (among all 2-digit SIC codes) in terms of the number of bad acquisitions completed during 2 consecutive years. We regard acquisitions as "bad" if they are met with negative acquirer stock market reactions exceeding -2.5% upon the M&A announcement. The rationale for this empirical design is that the investors' reaction captures potential overinvestment. The control group (i.e., firms that belong to industries less likely to overinvest) are those with a similar number of acquisitions during the same time period that do not earn negative and significant M&A announcement returns. Columns 3 and 4 of Table 1 in the Supplementary Material report multivariate tests based on model 9 of Table 3. These tests reveal that, after the passing of BC laws, the marginal value of cash does not increase in firms belonging to overinvestment-prone industries. By contrast, the marginal value of cash rises by 14 to 16 cents in firms that operate in industries less likely to overinvest.[21]

3. Payout Policy and Cash Regimes

If cash is used for payout decisions, then, as Faulkender and Wang (2006) point out, the value of cash depends on the firm's payout policy. Halford et al. (2024) conclude that the cash distribution regime and the cash raising regime should be identified (ex post) when analyzing the value of an extra dollar of cash. We follow this recommendation. Specifically, as in Halford et al. (2024), we define

---

[21]These results should be interpreted with caution because they are based on an experimental design that is subject to a number of concerns. For example, it is possible that deals in an industry could be regarded as "bad" for a number of years but that such a trend reverses. Moreover, as with any M&A deal in which the acquirer exhibits a negative M&A announcement return, it is possible that the same acquirer would be worse off if it does not complete deal.

firms to be cash Distributing if they distribute cash to shareholders but do not service or issue debt. Likewise, we classify firms as cash Raising if they conduct a stock sale that exceeds 3 percent of their market capitalization, they do not pay a dividend, and they do not meaningfully pay down or issue new debt.

Columns 1 and 2 of Table 2 in the Supplementary Material report marginal value of cash regressions using control variables like those in Faulkender and Wang (2006), except for the net finance variable, which is split into two cash regime components. The double interaction term [BC Law × Δ Cash] is positive and significant *only* for the cohort of cash-raising firms. These firms exhibit a marginal value of cash increase of 48 cents after BC laws pass. These results indicate that cash regimes matter, and that the BC law effect is operating through the internal financing channel. Notably, a significantly larger proportion (~65%) of quiet-life firms are classified as cash distributing, consistent both with them not deriving extra value from internal slack, but also from being in more mature industries with fewer growth opportunities.

H.   Are All Antitakeover Provisions Equal?

An important contribution of our work is that we document an opposite result from that in Dittmar and Mahrt-Smith (2007). However, to emphasize and clarify this contribution, it is important to understand what drives the differences between the two sets of results. We address this question by examining cross-sectional heterogeneity in how the marginal value of cash changes after BC laws pass. While this is useful, we recognize that more is needed to reconcile the two sets of results.

One possibility is that the majority of firms in the sample of Dittmar and Mahrt-Smith (2007) are those where the quiet life hypothesis is most severe, while the majority of firms in our sample are those where bonding with customers is most important. Another possibility centers on Dittmar and Mahrt-Smith's (2007) focus on the Gompers et al. (2003) index to measure governance quality. Specifically, that index consists of several attributes related to takeovers that are *voluntarily* adopted by the firm (e.g., not conditioning severance compensation on control changes, the presence of blank check preferred stock or a classified board; and requiring super-majority voting to approve business combinations). Such voluntarily adopted attributes could be correlated with other, unobserved, characteristics related to agency problems within the firm. By contrast, BC laws provide plausibly exogenous variation in antitakeover protection coming from state laws.

Based on the above discussion, we estimate our DiD specification to reproduce the key result in Dittmar and Mahrt-Smith (2007) using their sample period and sample selection criteria. The goal here is to assess whether the difference between our results and theirs occurs because the majority of firms in their sample feature quiet-life agency issues. Thus, we examine a subsample of 11,247 firm-year observations from 1990 to 2003 that excludes public administration, utilities, and financial firms. The replication yields the following estimates (*p*-values in parentheses).[22]

---

[22]To more closely emulate Dittmar and Mahrt-Smith's (2007) specification, we remove both the BC Law and BC Law × Δ Cash terms from the regression. When we do that, the coefficient on γ_4 (−0.055, *p*-value = 0.000) is qualitatively similar to the −0.061 coefficient on γ_4 we estimate.

$$r_{i,t} - R_{i,t}^B = \gamma_0 + \gamma_1 \, BC \, Law_{s,t} + \gamma_2 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times BC \, Law_{s,t} + \gamma_3 \, \text{Gindex} + \gamma_4 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times \text{Gindex} + Controls$$

<div align="center">

0.074*	0.656**	−0.008	0.061***
(0.083)	(0.023)	(0.245)	(0.000)

</div>

The regression results show that our baseline finding is robust to the inclusion of the Gompers et al. (2003) index as a control variable. Moreover, our specification successfully replicates the main result by Dittmar and Mahrt-Smith (2007). Combined, these findings do not support the conjecture that the majority of firms in the sample of Dittmar and Mahrt-Smith (2007) are those where the quiet life hypothesis is most severe. Therefore, we interpret these results as evidence that antitakeover provisions voluntarily adopted by firms do not have the same value implications as antitakeover laws enacted by their state of incorporation. This is likely due to the fact that voluntarily adopted provisions signal high agency problems at the firm, and, unlike BC laws, often weaken shareholder rights.

IV.   Conclusions

There is considerable debate about whether business combination laws represent a significant change to the takeover environment for covered firms, and more generally, about the effect of takeover protections on firms. In the context of the staggered adoption of business combination laws as a natural experiment, we use shareholders' valuation of internal slack as a mechanism to assess whether and how these antitakeover regulations matter. We find a surprising, robust, and novel baseline result: the value of internal cash improves following the adoption of a business combination law. Specifically, while an extra dollar of cash is worth about 62 cents for the average firm that value increases by about 24 cents after business combination laws are enacted.

Our baseline result is opposite to the finding by Dittmar and Mahrt-Smith (2007) that the value of an extra dollar worsens in firms with poor corporate governance—determined with the Gompers et al. (2003) index of antitakeover provisions and the level of institutional monitoring. Therefore, we perform additional analyses to reconcile the findings. These tests show that the increase in the marginal value of cash that occurs after the adoption of business combination laws is present in firms with a major industrial customer. In contrast, we detect a decrease in the marginal value of cash in firms subject to quiet-life type agency problems. Importantly, these effects are not anomalous; we find similar patterns following the adoption of poison pill certification laws. We further find evidence that the cash value effect works through predicted channels to which bonding firms are more exposed. Overall, our results support the positive effect that takeover protection has on the ability of managers to deploy it effectively whenever bonding commercial relations is important, while recognizing the substantial potential for misuse in firms that may fall prey to quiet-life agency problems.

Appendix A. Variable Definitions

BC Law: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a business combination (BC) law, and 0 otherwise.

BC Motivating Firm: (0,1) indicator: 1 denotes the firm is identified by Karpoff and Wittry (2018) as having lobbied for the passage of a BC law, and 0 otherwise.

CSA Law: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a control share acquisition (CSA) law, and 0 otherwise.

CTS Corp v. Dynamics Corp: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a CSA law and the observation is after the 1987 *CTS Corp v. Dynamics Corp of America* court decision, and 0 otherwise.

PP Law: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a poison pill (PP) law or adjudicated a court decision ratifying poison pills in the state, and 0 otherwise.

Moran v. Household Intl: (0,1) indicator: 1 denotes the firm is incorporated in Delaware and the observation is after the 1985 *Moran v. Household International, Inc.* court decision, and 0 otherwise.

DD Law: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a directors' duties (DD) law, and 0 otherwise.

FP Law: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a fair price (FP) law, and 0 otherwise.

First Gen ATP Law: (0,1) indicator: 1 denotes the firm is incorporated in a state which has passed a first generation anti-takeover law, but the observation is prior to the 1982 *Edgar v. MITE* court decision, and 0 otherwise.

CHE: Cash and Short-Term Investments.

AT: Assets – Total.

CSHO: Common Shares Outstanding.

PRCC_F: Price Close – Annual – Fiscal Year.

OIBDP: Operating Income Before Depreciation.

IB: Income Before Extraordinary Items.

XRD: Research and Development Expense.

XINT: Interest and Related Expense – Total.

TXDI: Income Taxes – Deferred.

ITCI: Investment Tax Credit (Income Account).

DVC: Dividends Common/Ordinary.

DLC: Debt in Current Liabilities – Total.

DLTT: Long-Term Debt – Total.

SSTK: Sale of Common and Preferred Stock.

PRSTKC: Purchase of Common and Preferred Stock.

DLTIS: Long-Term Debt – Issuance.

DLTR: Long-Term Debt – Reduction.

ROA: Operating return on assets [OIBDP/AT].

Excess Stock Return: Stock return during the fiscal year minus the Fama–French 5 × 5 size and book-to-market matched benchmark portfolio return.

Cash: Cash and marketable securities normalized by beginning market value of equity [CHE/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

Earnings: Earnings before interest and extraordinary items normalized by beginning market value of equity [(IB + XINT + TXDI + ITCI)/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

Net Assets: Total assets minus cash and marketable securities normalized by beginning market value of equity [(AT − CHE)/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

Market Value of Equity: Beginning of period market value of equity [CSHO$(t-1)$ × PRCC_F$(t-1)$].

R&D: Research & Development normalized by beginning market value of equity [XRD/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

Interest: Interest expense normalized by beginning market value of equity [XINT/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

Dividends: Common dividends normalized by beginning market value of equity [DVC/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

Leverage: Debt in current liabilities and total long-term debt normalized by debt and market value of equity [(DLC + DLTT)/((DLC + DLTT) + (CSHO × PRCC_F))].

New Finance: Net stock issued plus net debt issues normalized by beginning market value of equity [((SSTK − PRSTKC) + (DLTIS − DLTR))/(CSHO$(t-1)$ × PRCC_F$(t-1)$)].

HHI: Revenue Herfindahl–Hirschman index at the 3-digit SIC code level.

HHHI: (0,1) indicator: 1 denotes firms residing in industries with high industry concentrations (as measured by the HHI) above the 75th percentile of all firms, 0 otherwise.

LHHI: (0,1) indicator: 1 denotes firms residing in industries with high industry concentrations (as measured by the HHI) below the 25th percentile of all firms, 0 otherwise.

Large Industrial Customer: (0,1) indicator: 1 denotes the firm has a single customer that represents at least 66% of the firm's sales, 0 otherwise.

Appendix B. Econometric Issues

B.1.   Parallel Trends

With DiD models, we compare changes in the marginal value of cash among firms in states that adopt a BC law (the treatment group) with changes in the same variable among firms located elsewhere (the control group). The key identification assumption necessary for internal validity of the DiD models is that the two groups satisfy parallel trends. In our setting, the parallel trends condition will be met if the difference in the marginal value of cash across the treatment and control groups in the years before a BC law is adopted is not statistically significant.

For firms in states that adopt a BC law, we define a *Parallel Trends* variable that is set to 1 during each of the 5 years *prior* to the adoption. The Parallel Trends variable is otherwise set to 0. In Table B1, we estimate four different marginal value of cash regressions in which the independent variable of interest, *Parallel Trends* × Δ*Cash*, is designed to test for parallel trends. In all models, coefficient estimates for this interaction variable fail to attain statistical significance. This evidence suggests that the parallel trends condition is satisfied.
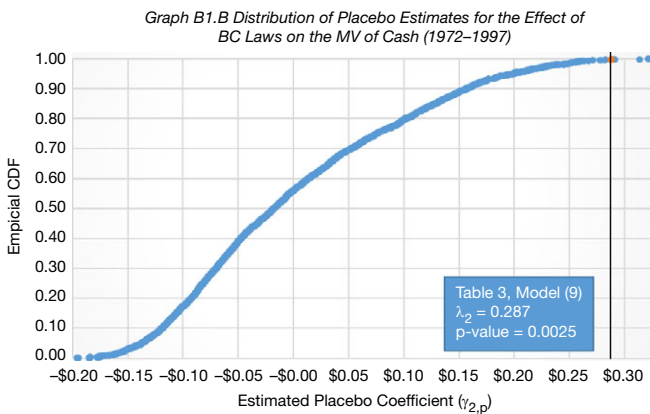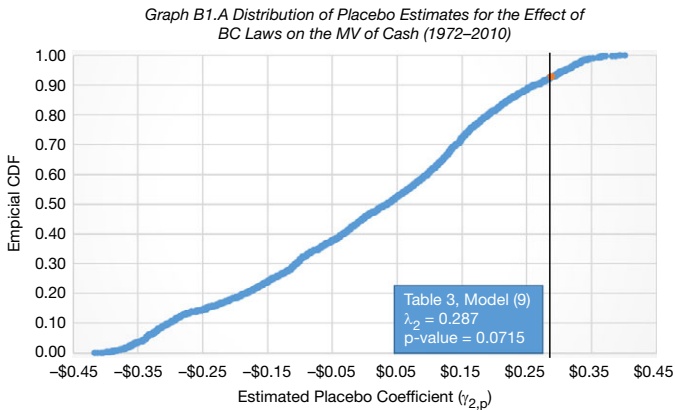
TABLE B1

Parallel Trends

Table B1 reports regressions of the effect of BC Laws on the marginal value of cash for the 86,418 firm-year-observations of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. The dependent variable in each model is the Excess Stock Return. Models 1 and 2, which replicate Faulkender and Wang (2006), are run on the 1972–2001 sample observations from their period of study. All other regressions use the full 1972–2010 sample period. Models 3 to 5 include firm, HQ state by year, and 4-digit SIC industry by year fixed effects. Models 7 and 8 run the Faulkender and Wang (2006) models in our sample period, while models 9 and 10 include firm fixed effects as in Giroud and Mueller (2010). All variables are defined in Appendix A. *p*-values based on robust clustered standard errors (at either the firm or state of incorporation level) are reported below the parameter estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Excess Stock Returns			
	1	2	3	4
Parallel Trends × Δ Cash	−0.096	−0.097	−0.097	−0.080
	(0.208)	(0.181)	(0.181)	(0.124)
Parallel Trends	0.032	0.019	0.019	0.003
	(0.109)	(0.251)	(0.251)	(0.708)
BC Law × Δ Cash	0.194**	0.227***	0.227***	0.264***
	(0.010)	(0.002)	(0.002)	(0.000)
BC Law	0.035	0.019	0.019	−0.001
	(0.152)	(0.503)	(0.503)	(0.934)
Δ Cash	0.665***	0.777***	0.777***	0.772***
	(0.000)	(0.000)	(0.000)	(0.000)
Δ Earnings		0.392***	0.392***	0.413***
		(0.000)	(0.000)	(0.000)
Δ Net Assets		0.158***	0.158***	0.155***
		(0.000)	(0.000)	(0.000)
Δ R&D		0.683***	0.683***	0.756***
		(0.000)	(0.000)	(0.000)
Δ Interest		−0.700***	−0.700***	−0.781***
		(0.000)	(0.000)	(0.000)
Δ Dividends		1.892***	1.892***	1.623***
		(0.000)	(0.000)	(0.000)
Cash ($t-1$)		0.736***	0.736***	0.726***
		(0.000)	(0.000)	(0.000)
Leverage		−1.023***	−1.023***	−0.980***
		(0.000)	(0.000)	(0.000)
New Finance		0.057**	0.057**	0.048***
		(0.023)	(0.023)	(0.000)
Firm FE	No	No	No	Yes
HQ State-Year FE	Yes	Yes	No	No
Industry-Year FE	Yes	Yes	No	No
Clustering	State of Incorporation Level		Firm Level	
Adj. R^2	0.116	0.287	0.287	0.238
No. of obs.	86,418	86,418	86,418	86,418

FIGURE B1

Block Permutation Test

Figure B1 presents the outcome of the block permutation procedure advocated by Chetty, Looney, and Kroft (2009) as applied to our research design. In each iteration, the BC Law treatment variable is randomly re-assigned by state and year without replacement as a placebo through the 1972–1997 and 1972–2010 sample periods. Our augmented Faulkender and Wang (2006) marginal value of cash regression with firm fixed effects (equation (2)/specification 9 in Table 3) is then estimated on the falsified data. Graphs B1.A and B1.B report the resultant empirical cumulative distribution function (CDF) generated from estimating the marginal value of cash in 2,000 random iterations of this procedure and capturing the placebo coefficient estimate ($\gamma_{2,p}$) on the interaction of the falsified BC Law dummy and the change in cash. The horizontal line indicates the actual coefficient estimate (γ_2) of $0.288 (as reported in Table 3, Model 9), which reflects the impact that BC Laws have on the marginal value of cash (as estimated in Table 3) and implied p-value when placed in the context of the CDF.

Graph B1.A Distribution of Placebo Estimates for the Effect of BC Laws on the MV of Cash (1972–2010)

Table 3, Model (9)
$\lambda_2 = 0.287$
p-value = 0.0715

Graph B1.B Distribution of Placebo Estimates for the Effect of BC Laws on the MV of Cash (1972–1997)

Table 3, Model (9)
$\lambda_2 = 0.287$
p-value = 0.0025

B.2.   Inflated t-Statistics

Another potential issue with DiD estimation is that serial correlation could lead to standard errors that may understate the standard deviation of the treatment effect thereby producing inflated *t*-statistics (Bertrand et al. (2004)). To address this issue, we perform nonparametric permutation tests for the marginal value of cash following the methods used by Chetty, Looney, and Kroft (2009). These tests alleviate concerns about serial correlation (Bertrand et al. (2004)). Indeed, as noted by Chetty et al. (2009), since the permutation tests do not make parametric assumptions about the error structure, they are not vulnerable to the over-rejection bias of the *t*-test in the presence of serial correlation.

Each permutation relies on a "placebo ordered pair" that consists of a [state]-[firm year]. In each iteration, the BC Law treatment variable is randomly re-assigned by state and year without replacement as a placebo through the sample period and our augmented Faulkender and Wang (2006) marginal value of cash regression with firm fixed effects (equation (2))/specification 9 in Table 3) is estimated on the falsified data. We repeat this process 2,000 times using as many different random number generator seeds. For each iteration, we record the point estimates to produce the plots in Figure B1 of the cumulative distribution function (CDF) capturing the placebo coefficient estimate ($\gamma_{2,p}$) on the interaction of the falsified BC Law dummy and the change in cash. The CDFs look smooth because of the large number of points used in the plots and not due to parametric smoothing (which we do not apply). We create placebo-ordered pairs to falsify the existence of BC laws. To be thorough, Graph A analyzes the 1972–2010 sample period we use in our main analyses, while Graph B is based on the 1972–1997 sample period (comprising the years preceding the enactment of BC laws in any state as well as the years in which they pass).

For reference, in each CDF plot the horizontal line indicates the actual coefficient estimate (γ_2) of \$0.29 for the impact that BC laws have on the marginal value of cash as reported in Table 3, Model 9 as well as the implied p-value when placed in the context of the CDF. According to Graph A in Figure B1, 143 out of 2,000 (or 7.15%) of the placebo coefficients are larger than those in model 9 of Table 3. In Graph B, only 5 of the 2,000 (or 0.0025%) of the placebo coefficients are larger than the estimated effect.

Overall, aside from mitigating concerns about artificially inflated t-statistics, it is reassuring that the results from our permutation non-parametric tests confirm those from our baseline DiD analyses.

B.3.   Differences Between the Treated and Control Groups

A concern that also arises in experimental designs involving treated and untreated subjects is that, due to unobservable characteristics, the treated population is intrinsically different from the untreated population. Under this possibility and in our context, any observed change in the marginal value of cash arises because firms are fundamentally different and not because their state of incorporation adopts a BC law. To investigate this issue, we perform the robustness test suggested by Bertrand and Mullainathan (2003). They call for the estimation of the DiD model using only eventually treated firms. The rationale is that, because of the staggered adoption of the BC laws over time, eventually treated firms are both control and treatment firms (i.e., they stay in the control group until their state of incorporation adopts a law).

Table B2 reports two regressions of the effect of BC Laws on the marginal value of cash for a sample of eventually treated firms. The estimations use the 10,789 firm-year-observations that reside in a state that will adopt a BC Law during the next 5 years using the baseline Faulkender and Wang (2006) specification. The dependent variable in each model is the Excess Stock Return. The BC Law Adopted indicator is set to 1 for firms incorporated in states passing a BC law in that fiscal year. For firms located in states that will eventually pass a BC law (in the next 4 fiscal years), but have not yet done so, the BC Law indicator is set to 0.

The results in Table B2 indicate that the marginal value of cash increases by about 18 cents once BC Laws are enacted. This result, which is consistent with those from our main tests and obtains using eventually treated firms as the control group, mitigate concerns about intrinsic differences between our treatment and control groups.

TABLE B2

Eventually Treated Firms

Table B2 reports regressions of the effect of BC Laws on the marginal value of cash for the restricted sample of eventually treated firms. The estimations are run on the 10,789 firm-year-observations that reside in a state which will adopt a BC or PP Law in the next 5 years using the baseline Faulkender and Wang (2006) specification. The dependent variable in each model is the Excess Stock Return. The Anti-takeover Law Adopted indicator denotes firms that are incorporated in states passing a BC or PP Law in that fiscal year. Model 2 includes year-fixed effects. All variables are defined in Appendix A. p-values based on robust clustered standard errors (at the state of incorporation level) are reported below the parameter estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Excess Stock Return	
	1	2
BC Law Adopted × Δ Cash	0.179*	0.185*
	(0.077)	(0.070)
BC Law Adopted	−0.004	−0.005
	(0.683)	(0.648)
Δ Cash	0.483***	0.478***
	(0.000)	(0.000)
Δ Earnings	0.363***	0.361***
	(0.000)	(0.000)
Δ Net Assets	0.172***	0.172***
	(0.000)	(0.000)
Δ R&D	1.249***	1.217***
	(0.000)	(0.000)
Δ Interest	−0.934***	−0.901***
	(0.000)	(0.000)
Δ Dividends	4.381***	4.388***
	(0.000)	(0.000)
Cash ($t - 1$)	0.191***	0.189***
	(0.000)	(0.000)
Leverage	−0.461***	−0.466***
	(0.000)	(0.000)
New Finance	0.030	0.031
	(0.372)	(0.341)
Year FE	No	Yes
Clustering	State of Incorporation Level	
Adj. R^2	0.205	0.207
No. of obs.	10,789	10,789

Moreover, the evidence in Table B2 also supports the view that the change in the value of cash results from the adoption of BC laws.

B.4. Common Divisors

Looking at the empirical evidence on dividend payout yield return predictability, Powell, Shi, Smith, and Whaley (2009) raise the concern that scaling the dependent and the independent variables by a "common" divisor might give rise to spurious correlations. Our setting is vulnerable to this concern because P_{t-1} is divisor in the left-hand side of our regressions, $r_{i,t} - R_{i,t}^B = \frac{P_t + D_t - P_{t-1}}{P_{t-1}} - R_{i,t}^B$, and also the normalizing variable in the right-hand side because market capitalization ($M_{i,t-1}$), is a direct function of stock price (i.e., $M_{i,t-1} = P_{t-1} \times$ # Shares).

To address the common divisors concern, Bollen, Smith, and Whaley ((2004), p. 115) suggest including the normalizing variable as an additional regressor in the model and suppressing the intercept term. In our setting, this approach yields the following modified specification.

TABLE B3

Common Divisor Tests

Table B3 reports regressions of the effect of BC Laws on the marginal value of cash for the 86,418 firm-year-observations of U.S. firms, excluding utilities (SIC 4900–4999), financials (SIC 6000–6999), and public administration firms (SIC 9000–9999), in the merged CRSP-Compustat database with complete data from 1972 to 2010. The dependent variable in each model is the EXCESS_STOCK_RETURN. All models include controls for ΔEarnings, ΔNet Assets, ΔR&D, ΔInterest, ΔDividends, Cash $(t-1)$, Leverage, and New Finance as well as firm, HQ state by year, and 4-digit SIC industry by year fixed effects. These tests address the common divisors problem noted by Bollen et al. (2004) and Powell et al. (2009). The constant term has been suppressed and each model includes 1/Market Capitalization $(t-1)$, which is the inverse of the normalizing variable for each of the independent variables. All variables are defined in Appendix A. p-values based on robust standard errors clustered at the state of incorporation level are reported below the parameter estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Excess Stock Return | |
	1	2
BC Law × Δ Cash	0.319***	0.290***
	(0.000)	(0.000)
BC Law	0.035***	0.000
	(0.000)	(0.971)
Δ Cash	0.671***	0.659***
	(0.000)	(0.000)
1/Market Capitalization $(t-1)$	0.370***	1.369***
	(0.000)	(0.000)
Other Controls	Yes	Yes
Firm FE	No	Yes
HQ State × Year FE	No	Yes
Industry × Year FE	No	Yes
Clustering	Firm	State of Incorporation
Adj. R^2	0.207	0.314
No. of obs.	86,418	86,418

$$
\text{(B-1)} \quad r_{i,t} - R_{i,t}^B = \gamma_0 \frac{1}{M_{i,t-1}} + \gamma_1 \text{BC\_LAW}_{s,t} + \gamma_2 \frac{\Delta C_{i,t}}{M_{i,t-1}} \times \text{BC\_LAW}_{s,t} + \gamma_3 \frac{\Delta C_{i,t}}{M_{i,t-1}}
$$

$$
+ \gamma_4 \frac{\Delta E_{i,t}}{M_{i,t-1}} + \gamma_5 \frac{\Delta \text{NA}_{i,t}}{M_{i,t-1}} + \gamma_6 \frac{\Delta \text{RD}_{i,t}}{M_{i,t-1}} + \gamma_7 \frac{\Delta I_{i,t}}{M_{i,t-1}} + \gamma_8 \frac{\Delta D_{i,t}}{M_{i,t-1}}
$$

$$
+ \gamma_9 \frac{C_{i,t-1}}{M_{i,t-1}} + \gamma_{10} L_{i,t} + \gamma_{11} \frac{\text{NF}_{i,t}}{M_{i,t-1}}.
$$

We use equation (B-1) to estimate regressions that are otherwise similar to those in Table 2, models 4 and 7, respectively. In both tests, which we report in Table B3, the new $\frac{1}{M_{i,t-1}}$ control is statistically significant. Notably, coefficients on the interaction term, γ_2, remain statistically significant yielding point estimates of 29 and 32 cents that are close to the estimates of 27 cents and 33 cents reported in Table 2, models 4 and 7, respectively. This evidence mitigates concerns of a common divisor leading to spurious correlations in our analyses.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023001473.

References

Angrist, D. J., and J. S. Pischke. *Mostly Harmless Econometrics*. Princeton, NJ: Princeton University Press (2009).

Appel, I. "Governance by Litigation." Available at SSRN 2532278 (2019).

Armstrong, C. S.; K. Balakrishnan; and D. Cohen. "Corporate Governance and the Information Environment: Evidence from State Antitakeover Laws." *Journal of Accounting and Economics*, 53 (2012), 185–204.

Atanassov, J. "Do Hostile Takeovers Stifle Innovation? Evidence from Antitakeover Legislation and Corporate Patenting." *Journal of Finance*, 68 (2013), 1097–1131.

Bebchuk, L.; A. Cohen; and A. Ferrell. "What Matters in Corporate Governance?" *Review of Financial Studies*, 22 (2009), 783–827.

Bena, J., and X. Ting. "Competition and Ownership Structure of Closely Held Firms." *Review of Financial Studies*, 30 (2017), 1583–1626.

Bertrand, M.; E. Duflo; and S. Mullainathan. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, 119 (2004), 249–275.

Bertrand, M., and S. Mullainathan. "Is There Discretion in Wage Setting? A Test Using Takeover Legislation." *Rand Journal of Economics*, 30 (1999), 535–554.

Bertrand, M., and S. Mullainathan. "Enjoying the Quiet Life? Corporate Governance and Managerial Preferences." *Journal of Political Economy*, 111 (2003), 1043–1075.

Bollen, N. P.; T. Smith; and R. E. Whaley. "Modeling the Bid/Ask Spread: Measuring the Inventory-Holding Premium." *Journal of Financial Economics*, 72 (2004), 97–141.

Borgschulte, M.; M. Guenzel; C. Liu; and U. Malmendier. "CEO Stress, Aging, and Death." NBER Working Paper No. 28550 (2021).

Bourveau, T.; Y. Lou; and R. Wang. "Shareholder Litigation and Corporate Disclosure: Evidence from Derivative Lawsuits." *Journal of Accounting Research*, 56 (2018), 797–842.

Cain, M.; S. McKeon; and S. D. Solomon. "Do Takeover Laws Matter? Evidence from Five Decades of Hostile Takeovers." *Journal of Financial Economics*, 124 (2017), 464–485.

Cen, L.; S. Dasgupta, and R. Sen. "Discipline or Disruption? Stakeholder Relationships and the Effect of Takeover Threat." *Management Science*, 62 (2016), 2820–2841.

Chetty, R.; A. Looney, and K. Kroft. "Salience and Taxation: Theory and Evidence." *American Economic Review*, 99 (2009), 1145–1177.

Comment, R., and G. W. Schwert. "Poison or Placebo? Evidence on the Deterrence and Wealth Effects of Modern Antitakeover Measures." *Journal of Financial Economics*, 39 (1995), 3–43.

Cremers, M.; L. P. Litov; and S. M. Sepe. "Staggered Boards and Long-Term Firm value, Revisited." *Journal of Financial Economics*, 126 (2017), 422–444.

DeAngelo, H., and E. M. Rice. "Antitakeover Charter Amendments and Stockholder Wealth." *Journal of Financial Economics*, 11 (1983), 329–359.

Denis, D. J., and V. Sibilkov. "Financial Constraints, Investment, and the Value of Cash Holdings." *Review of Financial Studies*, 23 (2010), 247–269.

Dessaint, O., and A. Matray. "Do Managers Overreact to Salient Risks? Evidence from Hurricane Strikes." *Journal of Financial Economics*, 126 (2017), 97–121.

Dittmar, A., and R. Duchin. "The Dynamics of Cash." Working Paper No. 1138, Ross School of Business, available at SSRN 1569529 (2010).

Dittmar, A., and J. Mahrt-Smith. "Corporate Governance and the Value of Cash Holdings." *Journal of Financial Economics*, 83 (2007), 599–634.

Dittmar, A.; J. Mahrt-Smith; and H. Servaes. "International Corporate Governance and Corporate Cash Holdings." *Journal of Financial and Quantitative Analysis*, 38 (2003), 111–133.

Duchin, R. "Cash Holdings and Corporate Diversification." *Journal of Finance*, 65 (2010), 955–992.

Duchin, R.; T. Gilbert; J. Harford; and C. Hrdlicka. "Precautionary Savings with Risky Assets: When Cash Is Not Cash." *Journal of Finance*, 72 (2017), 793–852.

Eldar, O., and J. Grennan. "Common Venture Capital Investors and Startup Growth." *Review of Financial Studies*, 37 (2024), 549–590.

Fama, E. F., and K. R. French. "Common Risk Factors in the Returns on Stocks and Bonds." *Journal of Financial Economics*, 33 (1993), 3–56.

Fama, E., and K. R. French. "Industry Costs of Equity." *Journal of Finance*, 43 (1997), 153–193.

Faulkender, M., and R. Wang. "Corporate Financial Policy and the Value of Cash." *Journal of Finance*, 61 (2006), 1957–1990.

Fich, E. M.; J. Harford; and A. L. Tran. "Disloyal Managers and Shareholders' Wealth." *Review of Financial Studies*, 36 (2023), 1837–1888.

Foley, C. F.; J. C. Hartzell; S. Titman; and G. Twite. "Why Do Firms Hold So Much Cash? A Tax-Based Explanation." *Journal of Financial Economics*, 86 (2007), 579–607.

Francis, B. B.; I. Hasan; K. John; and M. Waisman. "The Effect of State Antitakeover Laws on the Firm's Bondholders." *Journal of Financial Economics*, 96 (2010), 127–154.

Garvey, G. T., and G. Hanka. "Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage." *Journal of Finance*, 54 (1999), 519–546.

Giroud, X., and H. M. Mueller. "Does Corporate Governance Matter in Competitive Industries?" *Journal of Financial Economics*, 95 (2010), 312–331.

Gompers, P.; J. Ishii; and A. Metrick. "Corporate Governance and Equity Prices." *Quarterly Journal of Economics*, 118 (2003), 107–155.

Gormley, T. A., and D. A. Matsa. "Common Errors: How To (and Not To) Control for Unobserved Heterogeneity." *Review of Financial Studies*, 27 (2014), 617–661.

Gormley, T. A., and D. A. Matsa. "Playing it Safe? Managerial Preferences, Risk, and Agency Conflicts." *Journal of Financial Economics*, 122 (2016), 431–455.

Halford, J. T.; J. J. McConnell; V. Sibilkov; and N. Zaiats. "Existing Methods Provide Unreliable Estimates of the Marginal Value of Cash." *Critical Finance Review*, forthcoming (2024).

Harford, J. "Corporate Cash Reserves and Acquisitions." *Journal of Finance*, 54 (1999), 1969–1997.

Harford, J.; S. Klasa, and W. F. Maxwell. "Refinancing Risk and Cash Holdings." *Journal of Finance*, 69 (2014), 975–1012.

Heath, D.; M. C. Ringgenberg; M. Samadi; and I. M. Werner. "Reusing Natural Experiments." *Journal of Finance*, 78 (2023), 2329–2364.

Heron, R. A., and W. G. Lewellen. "An Empirical Analysis of the Reincorporation Decision." *Journal of Financial and Quantitative Analysis*, 33 (1998) 549–568.

Hicks, J. R. "Annual Survey of Economic Theory: The Theory of Monopoly." *Econometrica*, 3 (1935), 1–20.

Jensen, M. C. "Agency Cost of Free Cash Flow, Corporate Finance, and Takeovers." *American Economic Review*, 76 (1986), 323–329.

Jensen, M. C., and W. H. Meckling. "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure." *Journal of Financial Economics*, 3 (1976), 305–360.

Jensen, M. C., and R. S. Ruback. "The Market for Corporate Control: The Scientific Evidence." *Journal of Financial Economics*, 11 (1983), 5–50.

John, K.; Y. Li; and J. Pang. "Does Corporate Governance Matter More for High Financial Slack Firms?" *Management Science*, 63 (2017), 1872–1891.

Johnson, W. C.; J. M. Karpoff; and S. Yi. "The Bonding Hypothesis of Takeover Defenses: Evidence from IPO Firms." *Journal of Financial Economics*, 117 (2015), 307–332.

Johnson, W. C.; J. M. Karpoff; and S. Yi. "The Life Cycle Effects of Corporate Takeover Defenses." *Review of Financial Studies*, 35 (2022), 2879–2927.

Karpoff, J. M.; R. Schonlau; and E. Wehrly. "Which Antitakeover Provisions Deter Takeovers?" *Journal of Corporate Finance*, 75 (2022), 102218.

Karpoff, J. M., and M. D. Wittry. "Institutional and Legal Context in Natural Experiments: The Case of State Antitakeover Laws." *Journal of Finance*, 73 (2018), 657–714.

Korteweg, A.G. "The Costs of Financial Distress Across Industries." Available at SSRN 945425 (2007).

Lyon, J. D.; B. M. Barber; and C. L. Ling Tsai. "Improved Methods for Tests of Long-Run Abnormal Stock Returns." *Journal of Finance*, 54 (1999), 165–201.

Masulis, R., and S. Reza. "Agency Problems of Corporate Philanthropy." *Review of Financial Studies*, 28 (2014), 592–636.

Mikkelson, W., and M. Partch. "Do Persistent Large Cash Reserves Hinder Performance?" *Journal of Financial and Quantitative Analysis*, 38 (2003), 275–294.

Opler, T.; L. Pinkowitz; R. Stulz; and R. Williamson. "The Determinants and Implications of Corporate Cash Holdings." *Journal of Financial Economics*, 52 (1999), 3–46.

Pinkowitz, L., and R. Williamson. "Bank Power and Cash Holdings: Evidence from Japan." *Review of Financial Studies*, 14 (2001), 1059–1082.

Pinkowitz, L., and R. Williamson. "What is the Market Value of a Dollar of Corporate Cash?" *Journal of Applied Corporate Finance*, 19 (2007), 74–81.

Powell, J.; J. Shi; T. Smith; and R. Whaley. "Common Divisors, Payout Persistence, and Return Predictability." *International Review of Finance*, 9 (2009), 335–357.

Qiu, J., and F. Yu. "The Market for Corporate Control and the Cost of Debt." *Journal of Financial Economics*, 93 (2009), 505–524.

Roberts, M., and T. Whited. "Endogeneity in Empirical Corporate Finance." *Handbook of the Economics of Finance*, 2 (2013), 493–572.

Rogers, W. "Regression Standard Errors in Clustered Samples." *Stata Technical Bulletin*, 13 (1993), 19–23.

Shleifer, A., and L. H. Summers. "Breach of Trust in Hostile Takeovers. In *Corporate Takeovers: Causes and Consequences*." Chicago: University of Chicago Press (1988), 33–68.

Shleifer, A., and R. Vishny. "A Survey of Corporate Governance." *Journal of Finance*, 52 (1997), 737–783.

Stein, J. "Takeover Threats and Managerial Myopia." *Journal of Political Economy*, 96 (1988), 61–80.

Stulz, R. "Managerial Control of Voting Rights: Financing Policies and the Market for Corporate Control." *Journal of Financial Economics*, 20 (1988), 25–54.

Stulz, R. "Managerial Discretion and Optimal Financing Policies." *Journal of Financial Economics*, 26 (1990), 3–27.