Corporate Opportunity Waiver Laws Did Not Produce Disloyal Managers

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Abstract

Corporate Opportunity Waiver (COW) laws permit firms to suspend fiduciary duties related to corporate opportunities. Fich, Harford, and Tran (2023) argue that these laws reduced firm innovation and lowered corporate valuation for research-intensive firms. However, we are unable to replicate these results. We further show that the reported decline in Tobin's q is confounded by the effects of the dot-com bubble burst. Moreover, public firms subject to COWs reduce takeover defenses, contradicting their argument that COW laws weaken corporate governance. Overall, their conclusion that COW laws foster managerial disloyalty and harm shareholder value is not supported by the data.

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1 Introduction

The corporate opportunity doctrine represents a legal principle that prohibits an officer, a director, a shareholder, or a manager of a U.S. corporation from appropriating any business opportunity that the corporation might reasonably exploit itself. Under this doctrine, any such business opportunity is considered a corporate asset, and its unlawful appropriation amounts to corporate theft (Brudney and Clark, 1981). The opportunity doctrine represents a critical component of the fiduciary duty of loyalty for corporate individuals, which has been the cornerstone of U.S. corporate law for nearly two centuries (Rauterberg and Talley, 2017).

In 2000, Delaware state law departed from this long-established principle by allowing firms to waive this specific duty of loyalty for corporate directors, officers, employees, or shareholders. Delaware was followed by eight other states. The new corporate opportunity waiver (COW) laws enable firms to explicitly renounce their interest in any future business opportunities discovered by certain individuals in their professional roles. The legislative change was prompted by legal ambiguities and challenges associated with board overlap in startups: venture capital investors, who sought representation on multiple firm boards, could easily be deemed in violation of the corporate opportunity doctrine.

Fich et al. (2023) (hereafter FHT) argue that the COW laws enabled managers to exploit corporate opportunities for private benefits, fostered managerial disloyalty, undermined corporate governance, and diminished shareholder value. Their analysis further indicates that the COW law adoption resulted in decreased R&D investments, lowered patent count and patent value, increased frequency of inventor departure, and reduced Tobin's q for research-intensive firms. FHT also argue that these declines in innovation activities are associated with a decreased market value of cash and an increased propensity for firms to engage in acquisition activities. These significant adverse effects carry important implications for legal experts, regulators, and investors, indicating that restrictions may be warranted to curb potential misuse of COW laws by large public firms and to safeguard the integrity of corporate governance.

In this paper, we re-examine the findings in FHT and find that their results are not supported when we replicate the analyses using the same data sources. We identify various discrepancies between their program code and the description in the journal article concerning the sample selection, variable construction, econometric models (e.g., model specifications and control variable choice), and outlier management. FHT's original results either become non-robust or even disappear after applying the data procedures described in the publication. In addition, we collect new firm-level data on COW implementation to refine the identification and carry out separate analyses to test other implications of COW laws. Taken together, our analyses do not support FHT's conclusion that COW laws foster disloyal managers and destroy shareholder value.

Our analyses unfold in six steps. First, we re-examine the impact of COW laws on firm innovation (FHT Figure 1). The result, while presented in a figure, is one of the key findings of the study and is highlighted in their abstract. FHT show an abrupt and persistent decrease in three innovation measures following the COW law adoption, namely R&D spending, patent value, and patent count—all measured relative to total assets. However, our review of FHT's software code reveals that this result arises from a mis-specified regression that is not presented in the publication. After we correct the specification, we find no evidence of a decline in corporate innovation after the state-level adoption of COW laws.

Second, we reassess FHT's finding that state-level COW laws reduce Tobin's q for research-intensive firms. We identify multiple discrepancies between the journal publication and the submitted software code. A key discrepancy concerns the choice of size control: market capitalization in the publication versus total assets in the code, which leads to the replication failure. However, even after adopting total assets as the size control, the results remain non-robust.

More importantly, the dynamic treatment effects, as shown in Figure 2, suggest that the lack of robustness is not due to insufficient statistical power, but rather that FHT's original results are confounded. In examining the source of this confounding, we highlight that the adoption of COW laws in Delaware in 2000 coincides with the bursting of the dot-com bubble. Because Delaware covers 95% of treated firm-years, a meaningful inference on the causal effect of COW laws on firm valuations (i.e., Tobin's q) should account for this confounding effect. Controlling for dot-com-boom-related valuation effects eliminates all the negative valuation effects of COW laws. Additionally, we highlight that the robustness test in FHT that excludes Delaware-incorporated firms (FHT Table 11, Panel A) cannot be replicated.

Third, while COW laws enable firms to waive officers' responsibilities under the corporate opportunity doctrine, they still require a firm-level implementation through a change to bylaws or corporate charters. Based on SEC filings, we find that only 13% of all firms in COW states eventually implemented the waiver option in our sample. Even if this represents an endogenous firm choice that correlates with firm characteristics, we expect that any causal effects of COW laws should only concern firms that proceed to implementation. However, our analyses show no evidence that Tobin's q is significantly correlated with the implementation of COW laws for research-intensive firms in regressions that feature firm fixed effects, control for time-varying firm characteristics, and account for post-dot-com effects. This finding directly challenges the disloyal manager hypothesis advanced in FHT.

Fourth, we replicate the inventor-level results (FHT Table 3). FHT find an increased rate of inventor departure and reduced productivity among staying inventors after the state-level adoption of COW laws (FHT Table 3). These results serve as evidence that COW laws make it possible for someone to take an opportunity away from the firm. We are able to quantitatively reproduce the reported results *only when* we follow the specifications in FHT's software code, which differ from the specifications described in the publication in terms of fixed effects as well as standard error clustering methods. Moreover, FHT employ a more comprehensive sample that includes a large number of private firms. This sample expansion contradicts the initial emphasis on public firms[1] and poses two conceptual issues. First, it is unclear where these private firms are actually incorporated and if they are subject to state-level treatment. FHT assume in their analysis that the reported addresses of private firms on patent documents match their states of incorporation. This critical assumption is *not* discussed in the published text and contradicts previous studies that venture capitalists generally prefer a Delaware incorporation regardless of the headquarter location (Guzman and Stern, 2015). Eldar and Grennan (2024) document (in their Table D.5) that 72.6% of venture-backed startups are incorporated in Delaware, although many startups are actually located in California. Second, it is unclear what percentage of private firms implemented COW laws even if they had the option to do so under state law. If we sidestep these identification issues and limit the analysis to public firms for which we can precisely determine the state of incorporation and potential treatment, we fail to confirm any higher inventor departure rates and lower productivity among remaining inventors after COW laws are adopted.

Fifth, we evaluate the impact of COW laws on corporate governance in a separate analysis that examines changes in corporate takeover defenses around COW law adoption. The idea is that if implementing COWs comes with increased managerial disloyalty as argued in FHT, the same manager should seek more entrenchment and increase takeover defenses. However, using the takeover defense index developed by Bebchuk, Cohen, and Ferrell (2009), we find the opposite: firms reduce takeover provisions following COW laws and COW implementation. This evidence further challenges FHT's argument that COWs undermine corporate governance.

Sixth, we highlight the narrow scope of the law that concerns only the appropriation of corporate

[1] For example, FHT's abstract states "We show that public firms covered by waiver laws [...] exhibit abnormally high inventor departures."

opportunities. The waivers do *not* limit a director's liability with respect to any other forms of corporate misbehavior, for example, the use of corporate cash for private benefits. However, FHT show a reduced market value of corporate cash even though the narrow scope of the law does not predict such effects.

In contrast, the corporate opportunity doctrine creates the strong presumption that corporate affiliates (e.g, board members) might violate the law if they sit simultaneously on the boards of two rival firms. In fact, several studies provide evidence showing increased intra-industry board overlap following COW laws (Cabezon and Hoberg, 2025; Eldar and Grennan, 2024; Geng, Hau, Michaely, and Nguyen, 2021; Hu, Jiang, John, Ju, et al., 2023). However, these studies usually face identification challenges due to the lack of data on firms' actual implementation of COWs. We overcome this limitation with newly collected data on the implementation of COWs at the firm level. We show that both the adoption of COW laws and their subsequent implementation at the firm level lead to a significant increase in intra-industry board overlap among research-intensive firms. Such increased board overlap can have secondary adverse consequences for intra-industry competition as shown by Geng, Hau, Michaely, and Nguyen (2024). This provides a promising path for future research.

2 Data Issues

Verifying the results in FHT was more involved because we only had access to their software code deposited with *The Review of Financial Studies*, but not to the original data sample.[2] We reproduced the sample using the descriptions in the published article and the program code submitted to the journal. In this process, we identified numerous discrepancies between the journal article and the software code, as summarized in Table A.1.

[2] We were unable to obtain the original data sample from FHT because of licensing issues. The authors provided us with additional software code for FHT Figure 1, which was missing from the initial deposition at the RFS.

2.1 Firm Sample

Sample Selection Issues. The published article states a sample period 1996-2017 (p. 1845). However, the data period used in the analysis extends to 2018. To align our replication sample as closely as possible with the sample in FHT, we use a replication sample extending to 2018. The same year, 2018, is also the end year of another firm-level sample reported in FHT Table 5.

The firm-level sample used in FHT Table 5 also shows significant inconsistencies compared to the sample in FHT Table 2. Although both tables present firm-level analyses, the sample in FHT Table 5 is noticeably larger.[3] More importantly, the inclusion of an extra 60 distinct firms in FHT Table 5 comes with a reported mean *Market value of equity (in $ millions)* of only $1,688$ compared to $3,006$ in FHT Table 2. Similarly, the reported mean *ROA* of 0.119 in FHT Table 5 is roughly four times larger than the 0.027 reported in FHT Table 2.

Outlier Management. According to the publication, FHT have winsorized all continuous variables at the 1st and 99th percentiles for the analyses in FHT Figure 1 and FHT Table 2.[4] However, in reality, their software code shows that they trimmed the continuous variables at 2.5th and 97.5th percentiles for FHT Figure 1 and trimmed only two out of eight continuous variables at 1st and 99th percentiles for FHT Table 2.[5] All our subsequent regression analyses use the winsorized sample as stated in FHT's publication, as the different trimming approach appears ad hoc and the rationale underlying it is unclear.

Our firm-level sample includes 76,908 firm-year observations, which is close to the 76,558 reported in FHT Table 2. Although FHT Figure 1 features key results, we cannot directly compare our sample with theirs because they do not report summary statistics.

[3] Our review of FHT's code reveals that FHT Table 2 imposes a non-missing book-to-market requirement, whereas FHT Table 5 does not. However, the book-to-market variable is not used in the analysis presented in either table.

[4] See footnote 8 in FHT and the description in FHT Table 2.

[5] The two trimmed variables are *R&D Spending* and *Patent Dollar Value*.

[Insert Table 1 about here.]

Variable Construction. Our Table 1 presents our replication sample alongside the summary statistics published in FHT Table 2, Panel A. To clarify the discrepancy in FHT's treatment of outliers, we report both a winsorized version (labeled 'Winsor') and a trimmed version (labeled 'Trim') of the replication sample. The trimmed version follows FHT's software code by trimming only *R&D Spending* and *Patent Dollar Value* at the 1st and 99th percentiles, while leaving other variables unadjusted. The winsorized version follows the published study by winsorizing all continuous variables at the 1st and 99th percentiles. As shown in Table 1, comparing summary statistics between FHT's sample and our trimmed replication sample indicates that our sample closely matches FHT's.

Below, we explain the remaining discrepancies in the statistics. Some may reflect oversights in FHT's implementation. The difference in patent dollar value likely stems from updates to the patent research dataset. Updates to Compustat are unlikely to be responsible, as accounting variables generally match well. While not all discrepancies are material, we believe it is useful to document them to clarify why some of our summary statistics differ from those reported by FHT.

(i) The median value of the dummy variable *COW* in Column (10) cannot logically be zero if its mean in Column (3) is 0.531.

(ii) The Q3 value for *R&D spending* (normalized by total book assets) appears implausibly large at 0.997 in Column (12). Given that *R&D spending* is non-negative, a Q3 value of 0.997 implies a mean greater than $0.249 = (0.997/4)$, which contradicts the value of 0.078 reported in Column (3).[6]

(iii) The statistics for *Number of patents* match well. However, the method in FHT for calcu-

[6] The issues identified in (i) and (ii) is also found in the working paper and not a result of flawed copy editing by the journal.

lating the *Number of patents* deviates from their described approach on page 1845, which states: "Moreover, counts are susceptible to a truncation bias because patents are recorded [...] only after they are granted. We alleviate these issues by weighting each patent by the mean number of patents granted in the same year and technology class [...]." However, FHT do *not* apply any such adjustment in their count statistics for patents according to their software code.[7]

(iv) There is a notable difference in the mean *Dollar value of patents* between our replication (0.055) and the sample in FHT (0.03). The difference is likely due to updates to the patent research dataset maintained by the authors of Kogan, Papanikolaou, Seru, and Stoffman (2017).

(v) The mean of *Leverage* differs notably between our replication and the value reported by FHT. We find the difference arises because FHT's calculation of *Leverage* deviates from the definition described in the article. The journal text refers to $(dltt + dlc)/(at - ceq + csho \times prcc\_f)$ (FHT Table A1, p. 1883), whereas the software code shows $(dltt + dlc)/(dltt + dlc + cshpri \times prcc\_f)$. When we apply the formula used in the software code, the difference in means almost disappears.

(vi) The replicated *Market value of equity* and *ROA* closely match the mean values reported by FHT only when we use the trimmed version of the replication sample. Following FHT's software code, we have adjusted *Market value of equity* for inflation to 2001 constant dollars.

(vii) Although FHT report using the market capitalization of equity as a proxy for firm size in

[7] It is unclear why the authors are concerned about the truncation bias, which only occurs when researchers count the number of patent applications filed by a firm in a year that are eventually granted (Lerner and Seru, 2022). The concern stems from the lag between patent application and approval. For example, an application filed in 2019, but granted in 2022, would be missed if the data were collected until 2020. However, FHT count the number of patents granted to a firm in each year and *not* the number of (ultimately successful) patent applications in a given year. Hence, the approval truncation issue should be irrelevant in their context. For this reason, we do not apply the adjustment to *Number of patents*.

their main results reported in Table 2, their software code indicates that total assets are used instead. Accordingly, we also report summary statistics for total assets.

2.2 Inventor Sample

We retrieve data on inventors, granted patents, and assignees from *PatentsView*. An inventor is considered to have changed jobs when two consecutive patents filed by the same inventor display different assignees. The timing for the job change is determined by the mid-point of the respective patent application years. Following the software code provided by FHT, our replication sample comprises 6,115,363 inventor-firm-year observations, which roughly matches the 6,092,123 observations reported in FHT. Table 7, Panel A, presents our replication sample alongside the summary statistics reported in FHT Table 3, Panel A. These summary statistics match closely.

FHT state in their abstract and many other places in the paper that their analysis is focused on public firms. For example, FHT's abstract states "We show that public firms covered by waiver laws invest less in R&D, produce fewer and less valuable patents, and exhibit abnormally high inventor departures." However, our replication reveals that their analysis includes inventors working for *private firms*. These inventors from private firms account for about 44% of inventor-firm-year observations in the sample. But the incorporation state of private companies is difficult to identify, and FHT assume that it coincides with the reported postal address in patent documents. This assumption is not discussed in the journal publication. We discuss these issues in more detail in Section 3.5.

2.3 COW Implementation Data

The adoption of COW laws at the state level changes the corporate environment only if a firm grants such waivers to corporate individuals through amendments to its corporate charter or bylaws, or by signing private contracts. To improve the identification procedure, we follow the methodology outlined in Rauterberg and Talley (2017) and use machine learning techniques to analyze corporate regulatory disclosures related to COW adoption. Instead of using a BERT model as in Rauterberg and Talley (2017), we apply more advanced OpenAI large language models (LLMs) to all sample firms from 1997 to 2018. We also conduct a comprehensive manual review of all cases flagged by the LLM as potential instances of COW adoption. In addition, we compare our set of independently identified firms with the list of firms identified by Rauterberg and Talley (2017).[8] A detailed description of this comparison and our approach is provided in Appendix Table A.3. In total, we identify 1,027 distinct firms that have implemented COWs during our sample period.

Our firm-level COW implementation data captures instances where COWs are implemented for any entities or individuals, such as managers, directors, or officers. In the absence of a standard disclosure procedure for COW implementations, it is challenging to determine *when* a firm first implements a COW. As a result, we use a time-invariant dummy variable *IMP* to distinguish firms that have implemented COWs at least once from those that have not. Table 1, Panel B, compares firms in COW states that implement COWs (*IMP*=1) with those that do not (*IMP*=0).

3 Innovation and Firm Value Effects

3.1 Innovation Around COW Legislation

FHT report that three different measures of corporate innovation uniformly and persistently decrease after the COW law adoption, as illustrated in their Figure 1. They state on pages 1845/6 that they "use the method of Gormley and Matsa (2014) and construct cohorts of treated and control firms for the 3 years before, and the 3 years after each COW adoption event."[9] This suggests

[8] We thank Professor Anh Tran for making this data available to us.

[9] We believe FHT meant to cite Gormley and Matsa (2011) rather than Gormley and Matsa (2014); the latter does not discuss issues on difference-in-difference regressions.

the use of a stacked DID framework, which we seek to replicate with the following specification:

$$Innovation_{i,c,t} = \sum_{T=-3}^{3} \alpha_T Treat_{i,c} \times \mathbf{1}(T)_{c,t} + \theta_{c,i} + \gamma_{c,s,t} + \delta_{c,j,t} + \epsilon_{i,c,t}, \tag{1}$$

where i, c, s, and j index firms, cohorts, headquarter states, and industries (based on three-digit SIC codes), respectively. Each cohort c comprises firms incorporated in *one* COW state ($Treat_{i,c} = 1$) and all other contemporaneous firms incorporated in states that never adopt COW laws ($Treat_{i,c} = 0$). For example, the Delaware cohort includes firms incorporated in Delaware as treated observations and firms from states that never adopt a COW law as controls. Firms incorporated in other states that ever adopt a COW law at any point are excluded from the control group in all cohorts. In total, the stacked DID regression sample comprises nine cohorts, which include repeated firm-year observations because control firms can be repeatedly used in various cohorts.

The time dummy $\mathbf{1}(T)_{c,t}$ equals one if year t is T years away relative to the COW legislation year for cohort c, and zero otherwise. We denote by $\theta_{c,i}$ firm-cohort fixed effects. $\gamma_{c,s,t}$ and $\delta_{c,j,t}$ represent headquarter state-year-cohort and industry-year-cohort fixed effects. Our choice of fixed effects is the same as that described by FHT. The specification in Eq. (1) includes no control variables except for a $Zero\ Innovation$ dummy, which marks outcome variables with a zero value. Including this dummy ensures that the estimates are not skewed by the frequent occurrence of zero values for any of the three innovation measures.

As a robustness check, we also estimate a staggered version of the same model. Here, we add two additional treatment effects α_{-4} and α_{+4} for all available observations before $T = -3$ and after $T = +3$, respectively. This allows us to estimate the dynamic treatment effect of COW laws across the entire sample period 1996-2018, rather than limiting it to an event window of only 7 years.

[Insert Figure 1 about here.]

Figure 1 plots the estimates $\hat{\alpha}_T$ for stacked and staggered DID models in rows (1) and (2), respectively. The evolution of the three innovation variables around the state-level COW law adoption is very similar across both models. However, they show no resemblance to FHT Figure 1. In particular, we observe no evidence of a permanent decrease in innovation activity for any of the three innovation measures. Visual inspection also suggests that the parallel trends assumption is violated for the graph featuring the evolution of the *Patent Value*. Table 2 tabulates the coefficient estimates plotted in Figure 1.

[Insert Table 2 about here.]

We acknowledge that a firm's state of incorporation may change over time, and Compustat only reports the most recent record. To address this limitation, we perform a robustness analysis using historical information on the state of incorporation sourced from Spamann and Wilkinson (2019). The results, illustrated in Appendix Figure A.1, are very similar to those in Figure 1 and again show no evidence for reduced innovation.

We also note that any quick response of innovation output to the COW laws, as shown in FHT Figure 1, may warrant further scrutiny. First, COW laws require implementation at the firm level. Second, FHT measure patent output based on the number of patents *granted* in a firm-year. However, given the average two-year lag between patent application and grant (Hall, Jaffe, and Trajtenberg, 2001), along with the additional lag between R&D spending and the patent application, any reduction in R&D spending should be reflected in patent counts approximately two years later. This timing issue raises additional concerns about FHT Figure 1, which depicts a simultaneous decline in both R&D and patent count.

Moreover, indexing patent output by award year in FHT deviates from standard practice in the patent literature, which typically uses the patent application year. The latter is much closer to the time of the invention. As Lerner and Seru (2022) states on page 2,672, "The patent literature has generally focused on analyzing patent filings by the application year, rather than the award year."

To understand why our replication does not reproduce the results in FHT Figure 1, we analyze the underlying software code used to produce the figure. This code was not deposited on the RFS website, but was provided to us by FHT in response to our request. The software code for FHT Figure 1 features a colinear regression specification given by

$$Innovation_{i,c,t} = \sum_{T=-3}^{3} \alpha_T Treat_{i,c} \times \mathbf{1}(T)_{c,t} + Treat_{i,c} \times \theta_{i,c} + Treat_{i,c} \times \gamma_{s,c,t} + Treat_{i,c} \times \delta_{j,c,t} + \epsilon_{i,c,t}. \tag{2}$$

where the treatment dummy $Treat_{i,c}$ is separately interacted with a headquarter state-cohort-year fixed effect $\gamma_{s,c,t}$ and with industry-cohort-year fixed effect $\delta_{j,c,t}$. This specification absorbs the variation in $Treat_{i,c} \times \mathbf{1}(T)_{c,t}$, and should generate collinear regressors.

FHT still obtain coefficient estimates using Equation (2) because the cohort time dummy $\mathbf{1}(T)_{c,t}$ is defined using calendar years (based on fiscal year-end), whereas the fixed effects are indexed by fiscal years. This mismatch implies that identification relies solely on a small subset of firm-years in which the fiscal year differs from the calendar year. As a result, the specification may produce spuriously identified estimates and could explain why FHT Figure 1 differs significantly from our replication.

3.2 COW Legislation and Tobin's Q

The second key finding in FHT concerns a reduced shareholder value (measured by Tobin's q) after the state-level adoption of COW laws. They report the following regression:

$$\begin{aligned} ln(Tobin's\ q)_{i,t} =& \alpha_i + \beta_1(COW_{i,t} \times Innovation_{i,t}) + \beta_2 COW_{i,t} + \beta_3 Innovation_{i,t} + \beta_4 Controls_{i,t} \\ & + \gamma_{s,t} + \delta_{j,t} + \epsilon_{i,t}. \end{aligned} \tag{3}$$

The dependent variable is the natural logarithm of Tobin's q for firm i in year t. $COW_{i,t}$ denotes a dummy variable equal to one if a firm's state of incorporation enacts the COW legislation before year t and zero otherwise. The symbols α_i, $\gamma_{s,t}$, and $\delta_{j,t}$ represent firm fixed effects, headquarter-state-year fixed effects, and industry-year fixed effects, respectively. Firm-level control variables include firm size measured by (the natural log of) market capitalization of equity, market leverage, and return on assets. Missing innovation variables are set to zero, and a dummy variable $Zero\ Innovation$ is included to identify these missing values.

The key variable of interest is the regression coefficient $\hat{\beta}_1$ for the interaction term $COW_{i,t} \times Innovation_{i,t}$. FHT report a statistically significant and negative coefficient $\hat{\beta}_1$ across all three innovation measures and interpret this as evidence that COW legislation adversely affects shareholder value for firms engaged in more innovation activities.

[Insert Table 3 about here.]

We find an inconsistency in the choice of firm size control between the publication and the code. While the publication states that the size control is the log of market capitalization, it is actually the log of total assets (Compustat mnemonic: AT) in their software code. This choice of firm size control is critical. Our replication suggests that using the stated market capitalization results in replication failure. For example, in Table 3, Panel A, we do not observe significantly negative coefficients for $COW_{i,t} \times Innovation_{i,t}$ in most of the specifications. Columns (2) and (6) even feature positive and statistically significant coefficients for the interaction term. The uniformly negative and statistically significant valuation effect for high R&D firms in FHT Table 2 does not emerge in our replication.

In Panel B, where the (log) total assets are used as firm size measure, the regression coefficient $\hat{\beta}_1$ is robustly negative at the one percent significance level *only if* innovation is measured by the $Dollar\ Value\ of\ Patents$, but not for the two other measures of research intensity. We also note that only the level effect for COW is robustly negative at the one or five percent level in all specifications in Panels A and B.

To show that the estimates in Panel B do not arise from underpowered statistical analyses, Figure 2 plots the dynamic treatment effect using the following regression specification:

$$\begin{aligned} ln(Tobin's\ q)_{i,t} = \alpha_i + COW(-4^-)_{s,t} \times Innovation_{i,t} + \sum_{T=-3}^{3} COW(T)_{s,t} \times Innovation_{i,t} \\ + COW(4^+)_{s,t} \times Innovation_{i,t} + COW(-4^-)_{s,t} + \sum_{T=-3}^{3} COW(T)_{s,t} + COW(4^+)_{s,t} \\ + Innovation_{i,t} + Controls_{i,t} + \gamma_{s,t} + \delta_{j,t} + \epsilon_{i,t}. \end{aligned} \tag{4}$$

Here, we follow the notations used in FHT Table B.3 (reported in their Internet Appendix) as much as possible, but use a slightly longer window to maintain the consistency with Figure 1. Coefficients are omitted for the sake of space. The dummy variable $COW(T)_{s,t}$ is one if year t is T years relative to the COW adoption year for firms incorporated in COW state s, and zero otherwise. The dummy $COW(-4^-)$ identifies the observations for firms incorporated in COW states on and before $T = -4$, whereas $COW(4^+)$ identifies the observations on and after $T = 4$. The control variables and fixed effects are the same as those reported in the table.

Figure 2 exhibits no clear structural decline in Tobin's q after COW laws, confirming that the regression estimates in Panel B of Table 3 are not due to the lack of statistical power. The assumption of parallel pre-trend also seems to be violated when innovation is measured by R&D as well as patent count. Again, to account for the fact that Compustat only keeps the most recent incorporation states, we use historical incorporation states from Spamann and Wilkinson (2019) and repeat the analyses. The results reported in Appendix Table A.4 and Figure A.2 remain qualitatively similar.

3.3 Confounding Effects of Dot-com Bubble Burst

In addition to regression specification issues, a more fundamental problem afflicting FHT's analyses of Tobin's q is the confounding effects of the dot-com bubble burst around 2000. Around 95% of all treated firm-years (marked by $COW = 1$) are incorporated in Delaware, where COW laws were adopted in 2000. This coincides exactly with the peak of the dot-com bubble on March 10, 2000, when the Nasdaq index reached 5,048, before declining by 78% over the next 31 months to a low of 1,114 on October 9, 2002. Such a dramatic decline significantly impacts Tobin's q measures, potentially confounding any valuation effects attributed to COW laws for Delaware-incorporated firms. Additionally, the dot-com-related valuation corrections during 2000–2002 disproportionately affected high R&D firms, which commonly incorporate in Delaware. Taken together, these facts imply that inferences drawn from the specification in Eq. (3) may be substantially confounded by dot-com-related valuation corrections.

[Insert Table 4 about here.]

We respond to this issue by adding two new control variables for persistent valuation effect coming from the dot-com equity value correction. First, firms incorporated in Delaware could be more exposed to the dot-com bubble. Guzman and Stern (2015) report that venture capitalists in particular prefer companies to incorporate in Delaware due to the advantages conferred by the state's corporate law. Hence, we control for a valuation effect by interacting a Delaware incorporation dummy ($Delaware = 0/1$) with a second dummy that marks all years after 2000 ($PostDotcom = 0/1$). Second, the bursting of the dot-com bubble could have a greater influence on innovative firms. We therefore augment the previous specification with an interaction term $Innovation \times PostDotcom$.

Table 4 shows the augmented regressions. None of the six specifications shows a significant valuation reduction effect for the interaction term $COW \times Innovation$. At the same time, this interaction term even exhibits positive coefficients, though insignificant, for Columns (2), (5), and (6). We also highlight that the coefficients for the COW dummy itself are no longer statistically significant, unlike in Table 3. We conclude that there is no robust evidence of a negative valuation effect from the state-level adoption of COW laws on firms with high innovation activities.

[Insert Table 5 about here.]

We acknowledge that FHT present robustness evidence in FHT Table 11, Panel A, which excludes Delaware-incorporated firms from the sample and still obtains a highly significant valuation effect for R&D-intensive firms after state COW law adoption. We replicate FHT Table 11, Panel A and present the results in our Table 5. None of the coefficient estimates for the interaction term $COW \times Innovation$ is statistically significant at the conventional 10% level. In fact, the magnitude for five out of six coefficient estimates is smaller than one standard error, which is very far from the statistical significance reported in FHT. The hypothesis of negative valuation effects from COW laws does *not* pass the robustness test of excluding Delaware firms or controlling for valuation effects related to the dot-com bubble burst.

3.4 Firm-Level COW Implementation

State-level COW laws provide firms with the option to waive specific fiduciary duties for corporate directors, and their impact depends on whether firms actually implement them. Without such firm-level implementation, the governance regime remains unchanged. FHT acknowledge that they conduct an intention-to-treat analysis because they do not distinguish whether the firms incorporated in the treated states actually implement the COW laws. As a result, they recognize that their estimated effects should be viewed as a lower bound and the actual effects pertaining to those firms with COW implementation should be much larger.[10] In the following analysis, we identify the firms that actually implement COWs, and investigate whether COW laws have a more pronounced negative impact on firm value for research-intensive firms that implement COWs.

As discussed in Section 2.3, we use large language models to identify firms that actually implement COWs. We define a time-invariant dummy IMP to identify those firms that have implemented COWs at least once during the sample period. This leads to the following regression with a triple interaction term:

$$
\begin{aligned}
ln(Tobin's\ q)_{i,t} =& \alpha_i + \beta_1(IMP_i \times COW_{i,t} \times Innovation_{i,t}) \\
& + \beta_2(IMP_i \times COW_{i,t}) + \beta_3(IMP_i \times Innovation_{i,t}) + \beta_4(COW_{i,t} \times Innovation_{i,t}) \\
& + \beta_5 COW_{i,t} + \beta_6 Innovation_{i,t} + \beta_7 Controls_{i,t} + \gamma_{s,t} + \delta_{j,t} + \epsilon_{i,t}.
\end{aligned}
\tag{5}
$$

The coefficient β_1 captures the valuation effect for innovative firms that implement COWs, whereas β_4 represents the placebo effect (of non-treatment) if the dummy IMP_i correctly and completely identifies all cases of COW implementation. If COW implementation generates negative valuation

[10] On page 1839, FHT state "As a result, the ITT effects we report should be viewed as a lower bound to the treatment-on-the-treated (TOT) effects (i.e., the effects of actually including the waiver in a corporate charter)."

effects among innovative firms, we expect a negative coefficient estimate on β_1.

[Insert Table 6 about here.]

Table 6 presents the augmented regression specification with the triple interaction term. We include firm controls from Table 4 as well as interact IMP with post-dot-com controls. None of the coefficient estimates for $IMP \times COW \times Innovation$ are statistically significant at a 10% level: four out of the six specifications even feature positive point estimates for β_1. Overall, we find no evidence that firm value measured by Tobin's q decreases as a consequence of state-level COW laws and their firm-level implementation.

3.5 Inventor Mobility and Productivity

FHT also argue in Section 2.2 that inventors leave firms more frequently after the state-level COW adoption and that the remaining inventors are less productive. They argue that the COW laws help inventors take their ideas elsewhere and that firms find it more difficult to protect their intellectual property. The departure of innovators leads to reduced innovation output for the affected firms and a reduction in the innovation productivity of stayers. This increased inventor mobility is presented as the key channel explaining the reduced firm innovation (FHT Figure 1) and the adverse valuation effect (FHT Table 2).

[Insert Table 7 about here.]

We reassess the inventor mobility evidence reported in FHT Table 3. Using the software code deposited with RFS, we construct a replication dataset that closely matches theirs in terms of summary statistics across all variables. Panel A of Table 7 presents the comparison of summary statistics. In Panel B, we successfully replicate the regression results quantitatively, except for Column (8). Here, we find the same magnitude for the coefficient estimate in Column (8), but

FHT fail to show that the coefficient is statistically significant at the 1% level. We note that this particular result is inconsistent with the finding in FHT that the remaining inventors are less productive following COW legislation.

Several discrepancies between the software code and the publication emerge in the replication process. First, FHT report to include $Tech\ sector \times Year$ fixed effects in all inventor-level analysis, with $Tech\ sector$ defined at the level of the Cooperative Patent Classification (CPC) section. However, as we highlight in Panel B of Table 7, FHT inconsistently define $Tech\ sector$ fixed effects, using CPC subclasses in some specifications and CPC sections in others. The rationale for this variation is unclear and appears to be ad hoc. Importantly, the CPC subclass represents a more granular technology classification than the CPC section, with the regression sample comprising over 600 CPC subclasses grouped into 9 CPC sections.

Second, while FHT state that standard errors reported in the inventor regressions are clustered at the firm level, our replication finds that this is not the case. No clustering correction appears to be applied to the innovation productivity analyses in FHT Table 3, Panel C. If we correctly apply firm-level clustering, none of the reported negative inventor productivity results remains statistically significant as shown in our Table 7, Panel C, Columns (5)-(8).

Third, the inventor data used by FHT Table 3 features inventors in both private and public firms. The inclusion of private firms contrasts with all other firm-level analyses restricted to public firms. In addition, the state of incorporation (rather than location) is only known for public firms. Here, FHT assume that the reported address of private firms in patent documents matches their state of incorporation. But this assumption is not discussed in the published text and contradicts previous studies that venture capitalists generally prefer a Delaware incorporation regardless of headquarter location (Guzman and Stern, 2015). Eldar and Grennan (2024) document (in their Table D.5) that 72.6% of VC-backed startups, which are mostly technology firms, are incorporated in Delaware, although Delaware does not locate many technology startups.

In Panel D of Table 7, we exclude inventors from private firms due to their uncertain treatment status. Among public firms, we find no evidence of an enhanced departure rate for inventors or a reduction in the innovation productivity of stayers.

In conclusion, inventor-level analyses in FHT exhibit several key problems in their design and reported results. Their findings of higher inventor mobility and productivity disappear entirely once we focus on the public firm sample for which the incorporation and treatment status can be reliably discerned. FHT argue that these inventor results serve as the channel for the reduced R&D investment and for decreased valuation effect. In this sense, our inability to replicate their inventor-level findings is consistent with the replication failure reported earlier.

4 Governance Effects of COW Legislation

4.1 The Narrow Scope of COW Laws

The corporate opportunity waivers are not only a new contracting option contingent on firm-level implementation, but they also have a very narrow scope. In particular, such a waiver does not dispense with the general fiduciary duties of a director outside any transfer of a presumed business opportunity. For example, if a director tolerates irresponsible or unethical business practices, a corporate opportunity waiver would not discharge them of their fiduciary responsibilities. However, FHT misunderstand this narrow scope of COW legislation and suggest that waivers give rise to a more *general agency problem*. This motivates them to explore the impact of COW laws on marginal value of cash holding. As FHT state on Page 1852: "[...] the value of an extra dollar of cash is lower in firms with poor corporate governance. If our setting, we would expect a similar finding if corporate opportunity waivers increase the expression of agency problems in firms incorporated in states that approve such waivers." However, there is no reason to believe that the waivers and their implementation actually create any such general agency costs. With respect to the embezzlement or unproductive use of firm resources, the fiduciary duty of a director is the same with or without COW. This means that FHT's analyses on the corporate governance consequences of COW laws are conceptually misguided.

In addition, FHT suggest in their initial analyses (e.g., FHT Figure 1, FHT Tables 2-3) that COW laws primarily concern firms with high R&D intensity and patent filings. Yet, it is unclear why their subsequent analyses of the marginal value of cash holdings (FHT Table 4) and mergers and acquisitions (FHT Tables 5 and 6) no longer differentiate between firms with and without innovation activities by including the interaction term $COW \times Innovation$. Instead, they focus on the unconditional effect of COW laws captured by COW. Given that 47% of firm-years incorporated in COW states report zero R&D investments, this shift contradicts the statement in the abstract that "Remaining innovation activities [due to COW laws] contribute less to firm value, a fact confirmed by the market reaction when firms reveal their curtailed internal growth opportunities by announcing acquisitions".

Apart from these conceptual issues, the econometric evidence on the reduced value of cash holdings (FHT Table 4) and reduced acquisition value (FHT Table 6) suffers from the same shortcomings as the evidence in FHT Tables 2—namely, the confounding effects of the dot-com bubble burst, which coincided with the introduction of the COW law in Delaware, accounting for 95% of treated firm-years (marked by $COW = 1$). The dramatic equity price decline for the Nasdaq is likely to come with new evaluations of the real investment opportunities of many technology firms, thereby affecting the dollar value of cash as well as acquisition opportunities even absent any corporate governance deterioration or managerial disloyalty at play.

4.2 Takeover Defenses

FHT argue that the implementation of COW laws exacerbates agency problems, leading to weakened corporate governance and reduced firm value. If director loyalty to shareholders indeed declines, we would expect directors to entrench themselves by strengthening corporate takeover defenses to protect their private benefits. We test this hypothesis using the E-index created by Bebchuk et al. (2009) based on six governance provisions aimed at deterring takeovers. These provisions include staggered boards, limits to shareholder amendments of the bylaws, poison pills, golden parachutes, supermajority requirements for mergers, and supermajority requirements for charter amendments. A higher value for the E-index corresponds to more takeover defense and weaker shareholder rights. While the E-index is available only for the 1996–2006 period, we note that by the end of 2006, five out of nine states had already enacted COW laws, accounting for over 92% of all distinct treated firms.

Table 8 compares the takeover defenses of firms with a COW implementation to those without. In these regressions, we do not interact COW with innovation measures to align with FHT's regression specifications on the corporate governance implications of COW laws. We find that firms implementing COWs are associated with fewer takeover defenses. Both Columns (1) and (2) report a significant reduction in E-index for firms that actually implement COWs ($IMP \times COW = 1$). The coefficient estimate for $IMP \times COW$ in Column (2) suggests that, on average, implementing COWs is associated with a reduction in takeover defenses of 0.405, equivalent to nearly 18% of E-index's mean. Even if firms' implementation of COWs is endogenous to the pre-existing firm governance, this evidence is hard to reconcile with any increased director disloyalty to shareholders.

[Insert Table 8 about here.]

4.3 COW Laws and Board Overlap

The conceptual confusion in FHT about the scope of the COW laws does not imply that these laws were without real effects for corporate conduct. The specific intent of the COW laws was to deal with potential legal liabilities arising from corporate affiliates who simultaneously work for multiple firms within the same industry (Rauterberg and Talley, 2017). For example, prior to COW laws, board directors sitting on multiple boards can be accused of being insiders to business opportunities of one company while using the acquired information to advise and influence business decisions of another firm. This legal challenge constrains startup financing because venture capital investors usually seek board representation and find it difficult to invest in more than one startup within each industry. In line with this argument, Eldar and Grennan (2024) document an increase in common venture ownership among startups within the same industry following COW laws. They attribute this common ownership increase to intra-industry board overlap permitted by these laws.

Studies focusing on public firms also document an expansion in intra-industry board overlap following COW laws but for different reasons. Geng et al. (2024) and Cabezon and Hoberg (2025) separately find that increased board overlap due to COW laws leads to more research coordination and technology diffusion among interlocked high-tech firms. Gopalan, Li, and Zaldokas (2024) find that price collusion through board overlap is more pronounced among firms covered by COW laws.

However, all these aforementioned studies do not condition on the implementation of COW laws at the firm level and therefore do not allow a quantification of the conditional overlap effect. Our

COW implementation data allows for a more *refined analysis* given by:

$$
\begin{aligned}
\textit{Intra-industry board overlap}_{i,t} = {} & \alpha_i + \beta_1(IMP_i \times COW_{s,t} \times HighR\&D_i) + \beta_2(IMP_i \times COW_{s,t}) \\
& + \beta_3(COW_{s,t} \times HighR\&D_i) + \beta_4 COW_{s,t} \\
& + \beta_5 Controls_{i,t} + \gamma_{s,t} + \delta_{j,t} + \epsilon_{i,t}
\end{aligned}
\tag{6}
$$

The dependent variables are: (i) *# Intra-industry Board Overlap*, defined as the number of external board seats a firm's directors hold within the same three-digit SIC industry and (ii) *% Intra-industry Board Overlap*, which divides *# Intra-industry Board Overlap* by the number of firm's board seats. The director information comes from BoardEx. We exclude firm-years with fewer than three identified directors due to inadequate board information. The dummy variable $HighR\&D_i$ is one for firms among the top quintile of research expenditure relative to total assets and zero otherwise. Following Geng et al. (2024), we define $HighR\&D$ as a time-invariant variable based on the firm's first observation in the sample.[11] We interact COW with time-invariant variable $HighR\&D_i$, rather than with the time-varying $Innovation$ used before, especially given that FHT argue that COW and $Innovation$ are correlated. Other variables in these regressions are the same as in Eq. (4). The variables $HighR\&D_i$ and IMP_i and their interaction are absorbed by firm fixed effects.

[Insert Table 9 about here.]

Table 9, Panel B, reports the regression results. The triple interaction term $IMP \times COW \times HighR\&D$ is positive across all specifications and statistically significant at 1% level. The point estimates in Column (2) imply that COW-law implementation ($IMP \times COW = 1$) increases intra-industry board overlap by 0.081 percentage points ($= -0.012 + 0.093$) for research-intensive firms

[11] The quintiles are constructed using observations with non-zero R&D.

($HighR\&D = 1$), amounting to more than 100% of the mean value and 44% of the standard deviation for *%Intra-industry board overlap*. Our evidence confirms that COW laws achieve their stated purpose of reducing legal liability for overlapping board members, as they effectively enable more frequent board interlocks.

5 Conclusion

This paper challenges the conclusion in FHT that the COW laws, implemented in nine U.S. states between 2000 and 2016, destroyed shareholder value, because they gave rise to disloyal managers who appropriated business opportunities that should have accrued to the firm.

We replicate their main empirical results, namely a sharp drop in innovation activities in the years following the COW legislation and a parallel reduction in Tobin's q. Our analyses cannot confirm either result. Moreover, the proposed regression specifications suffer from confounding effects from the burst of the dot-com boom in 2000 when most of the treatment occurred for firms incorporated in Delaware. We also use new data on firm-level COW implementation to re-examine FHT's conclusion with improved statistical power and find no evidence to support it in the data. Our replication also examines FHT's findings on increased inventor departure rates and reduced productivity among remaining inventors following state-level COW adoption. However, aside from issues related to regression design and result reporting, we are unable to confirm these findings when restricting the analysis to inventors at public firms.

A further conceptual issue is that the COW laws have a very narrow scope, confined to the illegitimate transfer of business opportunities, which renders arguments regarding a general deterioration of corporate governance untenable. Using a firm-level takeover defense index to measure governance quality, we find that firms reduce their takeover defenses following their implementation of COW laws. This finding is inconsistent with FHT's finding that COW laws lead to deteriorated corporate governance. The main governance impact of the COW laws was to enable intra-industry board overlap in line with the legislative intent. Here, we find very strong evidence that such board overlap among rivals increased dramatically and that this increase was largely concentrated in research-intensive firms.

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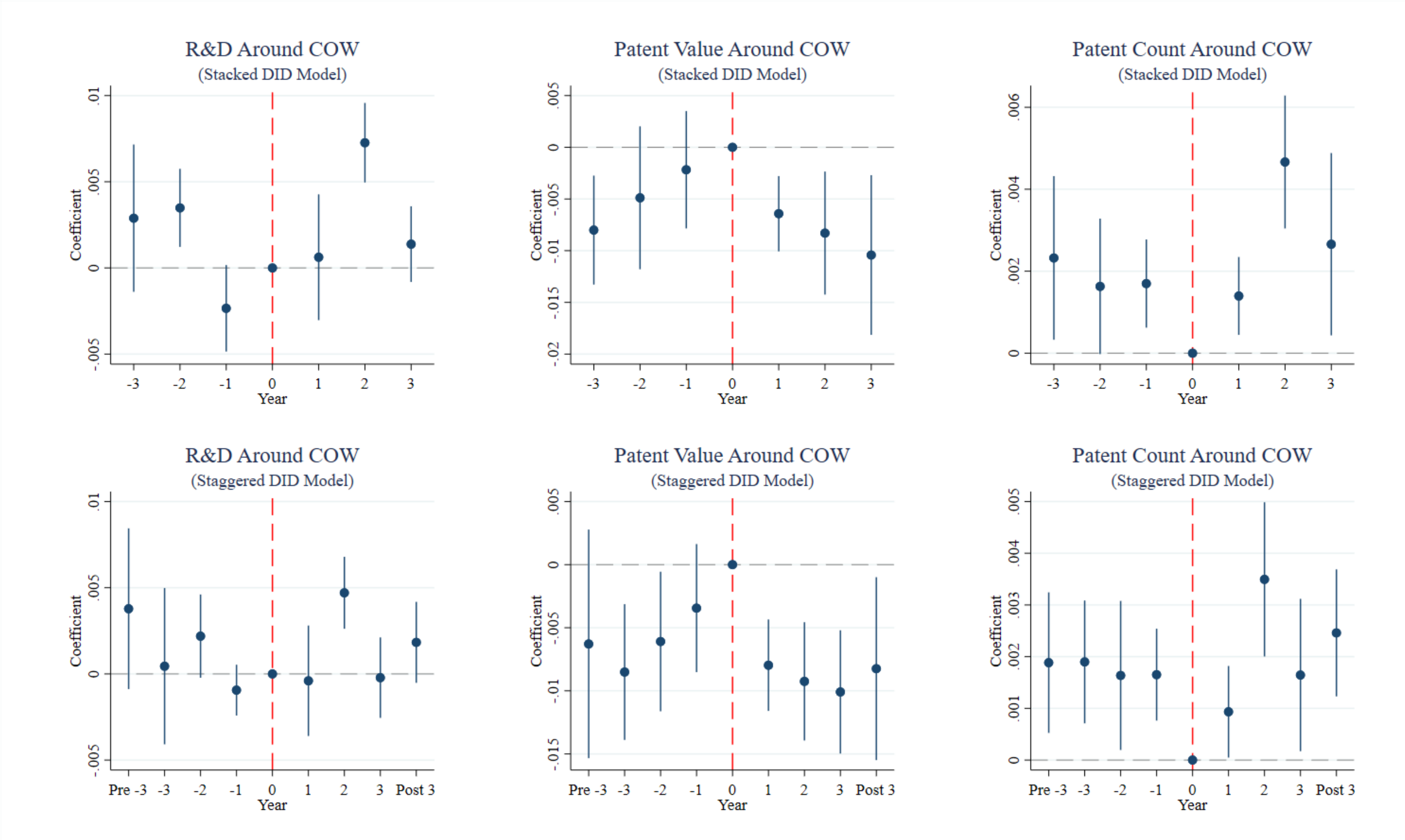

Figure 1: Innovation around COW adoption

We replicate FHT Figure 1 using stacked and staggered DID models in rows 1 and 2, respectively. Reported are point estimates for the effect of state-level COW laws on firm innovation measured by *R&D spending*, *Patent value*, and *Patent count*, around the state-level adoption of COW legislation. The full model is reported in Table 2. The vertical bars in all panels represent 90% confidence intervals and the standard errors are clustered at the level of the state of incorporation.

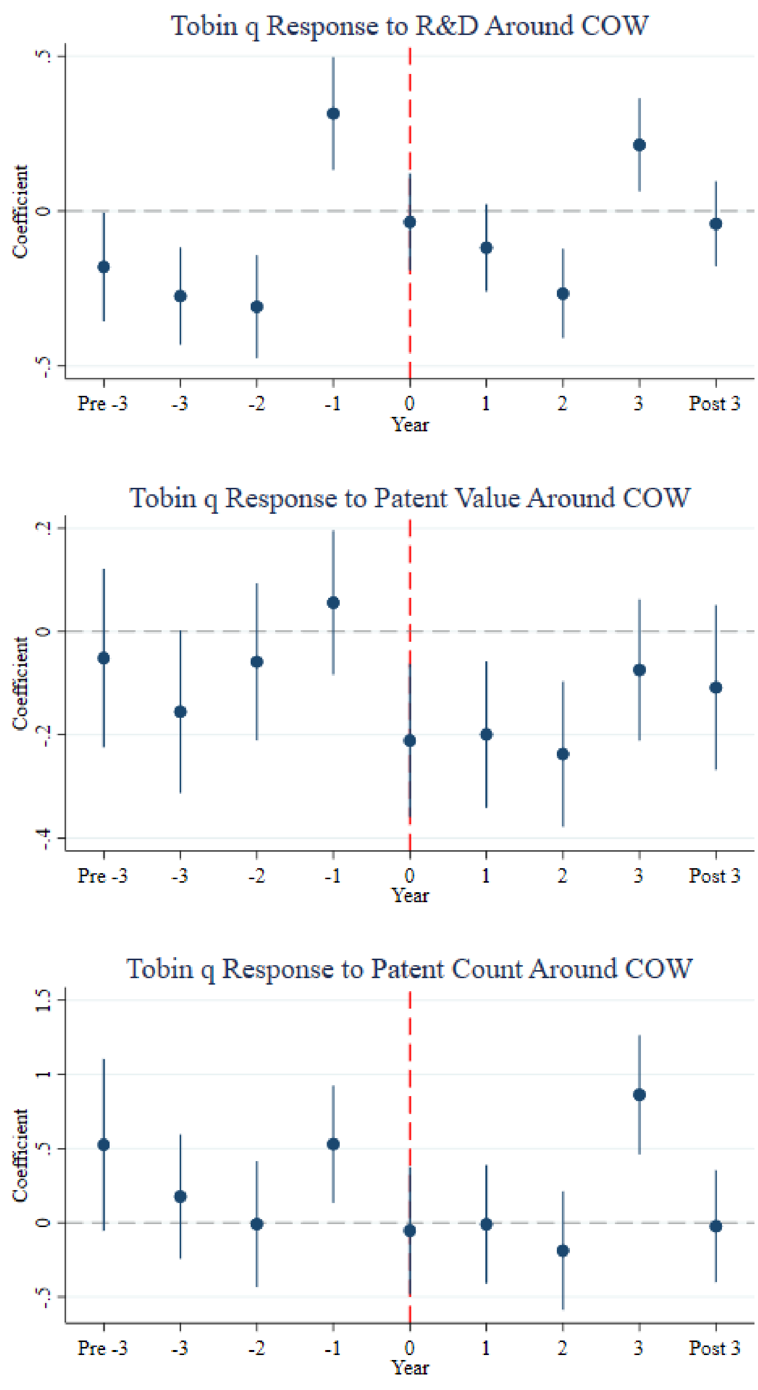

Figure 2: The dynamic effect of COW legislation on Tobin's q

We plot how Tobin's q of firms with high innovation levels responds to the state-level adoption of Corporate Opportunity Waivers (COWs) using a staggered DID model. Innovation is measured separately by three proxies: *R&D spending*, *Patent value*, and *Patent count*. Vertical bars represent 90% confidence intervals. The standard errors are clustered at the level of the state of incorporation.

Table 1: **Descriptive Statistics**

In Panel A, we compare the summary statistics reported in FHT Table 2, Panel A with those from our replication sample. Our replication sample includes 76,908 firm-year observations for the 1996–2018 period, slightly more than the 76,558 reported in the published article. Columns labeled 'Rep' present statistics from our replication, while columns labeled 'FHT' reproduce those reported in FHT. Although FHT state that all continuous variables are winsorized at the 1st and 99th percentiles, their software code reveals that only two variables were actually trimmed. To clarify this discrepancy, we report the mean and standard deviation for both the winsorized version of our sample (labeled 'Winsor') and the trimmed version (labeled 'Trim'). For the 1st quartile (Q1), median, and 3rd quartile (Q3), we report only the winsorized values, as the trimmed counterparts are very close. The three innovation measures *R&D spending*, *Dollar value of (new) patents*, and *Number of (new) patents* are annual values and all scaled by the total assets. The *Market value of equity*, expressed in millions of dollars, is in 2001 constant dollars. FHT erroneously report using market capitalization of equity as a size control, but actually use total assets. For this reason, we also report summary statistics for *Total assets*, which is stated in 2001 constant dollars. Panel B compares the means of various firm statistics by state of incorporation (i.e. non-COW versus COW states), respectively, and distinguishes between firms implementing COW laws (*IMP=1*) and those that do not (*IMP=0*). The winsorized version of the replication sample is reported in Panel B.

Panel A: Replicated versus published statistics

Variable	Mean			SD			Q1		Median		Q3	
	Rep. Winsor.	Rep. Trim	FHT	Rep. Winsor.	Rep. Trim	FHT	Rep. Winsor.	FHT	Rep. Winsor.	FHT	Rep. Winsor.	FHT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
COW	0.538	0.538	0.531	0.499	0.499	0.499	0	0	1	0	1	1
Market valuation												
ln(Tobin's q)	0.557	0.559	0.561	0.601	0.621	0.623	0.123	0.123	0.442	0.444	0.887	0.892
Innovation												
R&D spending	0.064	0.058	0.078	0.124	0.106	0.129	0	0.001	0.004	0.023	0.077	0.997
Dollar value of patents	0.055	0.045	0.030	0.157	0.123	0.076	0	0	0	0	0.020	0.009
Number of patents	0.012	0.015	0.014	0.038	0.084	0.073	0	0	0	0	0.003	0.003
Firm characteristics												
Market value of equity	2,288	3,015	3,006	7,301	16,030	16,375	60	55	268	250	1,125	1,073
Total assets	1,869	2,505	N.A.	5,450	14,394	N.A.	57	N.A.	231	N.A.	1,025	N.A.
Leverage (Mkt)	0.145	0.146	0.191	0.165	0.167	0.221	0.003	0.004	0.089	0.110	0.232	0.305
ROA	0.034	0.028	0.027	0.247	0.323	0.329	0.009	0.005	0.101	0.100	0.160	0.159

Panel B: Firms with and without implementation of COW laws

Firm registration	Non-COW State	COW State			Difference (3)-(4)
			if *IMP* = *1*	if *IMP* = *0*	
	(1)	(2)	(3)	(4)	(5)
Firm-year obs.	18,661	58,247	7,867	50,380	N.A.
Mean value					
ln(Tobin's q)	0.481	0.581	0.540	0.587	-0.048^{***}
R&D spending	0.041	0.072	0.046	0.076	-0.030^{***}
Dollar value of patents	0.039	0.060	0.045	0.062	-0.016^{***}
Number of patents	0.010	0.013	0.007	0.014	-0.007^{***}
Market value of equity	1,985	2,386	3,505	2,211	$1{,}294^{***}$
Total assets	1,616	1,951	3,260	1,746	$1{,}514^{***}$
Leverage(Mkt)	0.145	0.146	0.193	0.138	0.055^{***}
ROA	0.080	0.019	0.058	0.013	0.045^{***}

Table 2: **Firm Innovation Around COW Adoption**

We replicate FHT Figure 1 which shows the effect of state-level COW adoption ($Treat = 0/1$) on three innovation measures around the adoption year. The dummy $\mathbf{1}(T)$ marks the time shift by T years relative to the adoption year. We use a stacked DID model in Columns (1), (3), and (5), respectively, and the staggered DID model in Columns (2), (4), and (6), respectively. The three innovation measures are (1) *R&D spending* defined as a firm's R&D expenditure divided by total (book) assets; (2) *Dollar value of patents* defined as the nominal dollar value of new patents (Kogan et al., 2017) divided by total (book) assets, and (3) *Number of patents* defined as the yearly number of new patents granted scaled by total (book) assets. Control variables include a dummy variable indicating if the innovation variable is equal to zero. We winsorize all continuous variables at the 1st and 99th percentiles. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dep. variables:	*R&D spending*		*Dollar value of patent*		*Number of patents*	
	(1) Stacked DID	(2) Staggered DID	(3) Stacked DID	(4) Staggered DID	(5) Stacked DID	(6) Staggered DID
Treat $\mathbf{1}(< -3)$		0.004		−0.006		0.002**
		(0.003)		(0.005)		(0.001)
Treat $\mathbf{1}(-3)$	0.003	0.000	−0.008**	−0.009**	0.002*	0.002***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)
Treat $\mathbf{1}(-2)$	0.003**	0.002	−0.005	−0.006*	0.002	0.002*
	(0.001)	(0.001)	(0.004)	(0.003)	(0.001)	(0.001)
Treat $\mathbf{1}(-1)$	−0.002	−0.001	−0.002	−0.003	0.002**	0.002***
	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)	(0.001)
Treat $\mathbf{1}(+1)$	0.001	−0.000	−0.006***	−0.008***	0.001**	0.001*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Treat $\mathbf{1}(+2)$	0.007***	0.005***	−0.008**	−0.009***	0.005***	0.003***
	(0.001)	(0.001)	(0.004)	(0.003)	(0.001)	(0.001)
Treat $\mathbf{1}(+3)$	0.001	−0.000	−0.010**	−0.010***	0.003*	0.002*
	(0.001)	(0.001)	(0.005)	(0.003)	(0.001)	(0.001)
Treat $\mathbf{1}(> +3)$		0.002		−0.008*		0.002***
		(0.001)		(0.004)		(0.001)
Zero Innovation (0/1)	−0.030***	−0.030***	−0.066***	−0.081***	−0.037***	−0.036***
	(0.005)	(0.002)	(0.014)	(0.008)	(0.003)	(0.001)
Firm×Cohort FEs	Yes	No	Yes	No	Yes	No
HQ State×Year×Cohort FEs	Yes	No	Yes	No	Yes	No
Industry×Year×Cohort FEs	Yes	No	Yes	No	Yes	No
Firm FEs	No	Yes	No	Yes	No	Yes
HQ State×Year FEs	No	Yes	No	Yes	No	Yes
Industry×Year FEs	No	Yes	No	Yes	No	Yes
Observations	68,452	76,908	68,452	76,908	68,452	76,908
Adj. R^2	0.792	0.771	0.746	0.651	0.612	0.583

Table 3: **Replicating Valuation Effects by Innovation Intensity**

We replicate the panel regression in FHT Table 2, Panel B. The dependent variable is *Tobin's q* defined as the market value of assets divided by the book value of assets. The dummy variable *COW* is one if a firm's state of incorporation has passed the legislation of Corporate Opportunity Waivers by the fiscal year-end date, and zero otherwise. Three innovation proxies are (1) *R&D spending* defined as the R&D expenditure divided by total book assets; (2) *Dollar value of patents* defined as the nominal dollar value of new patents (Kogan et al., 2017) divided by total book assets, and (3) *Number of patents* defined as the yearly number of new patents scaled by total book assets. Control variables include size, market leverage, return on assets, and a dummy variable indicating if the innovation variable is equal to zero. Panel A uses the natural logarithm of market capitalization to measure firm size, whereas Panel B uses the natural logarithm of total book assets, as suggested by the software code in FHT. Both size measures are in 2001 constant dollars. We winsorize all continuous variables at the 1st and 99th percentiles. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Using $ln(MarketCap)$ as firm size control

Dependent variable:	*ln(Tobin's q)*					
Innovation measure:	*R&D spending*		*Dollar value of patents*		*Number of patents*	
	(1)	(2)	(3)	(4)	(5)	(6)
Innovation	0.563***	1.149***	0.848***	0.493***	0.379**	1.120***
	(0.048)	(0.098)	(0.037)	(0.036)	(0.142)	(0.060)
COW	−0.043***	−0.047***	−0.033**	−0.030***	−0.041***	−0.042***
	(0.014)	(0.013)	(0.014)	(0.011)	(0.014)	(0.012)
COW × Innovation	−0.007	0.148**	−0.158***	−0.144***	−0.130	0.344***
	(0.047)	(0.068)	(0.029)	(0.028)	(0.107)	(0.057)
Firm Controls:						
$ln(MarketCap)$		0.310***		0.298***		0.311***
		(0.004)		(0.004)		(0.004)
$Leverage(Mkt)$		−0.655***		−0.703***		−0.697***
		(0.025)		(0.024)		(0.024)
ROA		0.017		−0.248***		−0.237***
		(0.032)		(0.035)		(0.034)
Zero Innovation (0/1)	−0.016	0.038***	0.112***	0.101***	0.062***	0.115***
	(0.012)	(0.010)	(0.007)	(0.009)	(0.004)	(0.008)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,908	76,908	76,908	76,908	76,908	76,908
Adj. R^2	0.602	0.767	0.613	0.761	0.600	0.760

Table 3, continued

Panel B: Using $ln(Assets)$ as firm size control

Dependent variable:	*ln(Tobin's q)*					
Innovation measure:	*R&D spending*		*Dollar value of patents*		*Number of patents*	
	(1)	(2)	(3)	(4)	(5)	(6)
Innovation	0.563***	0.920***	0.848***	0.756***	0.379**	0.297***
	(0.048)	(0.077)	(0.037)	(0.033)	(0.142)	(0.099)
COW	−0.043***	−0.037***	−0.033**	−0.027**	−0.041***	−0.033**
	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.013)
$COW \times Innovation$	−0.007	0.013	−0.158***	−0.134***	−0.130	−0.185*
	(0.047)	(0.045)	(0.029)	(0.026)	(0.107)	(0.099)
Firm Controls:						
$ln(Assets)$		−0.060***		−0.080***		−0.075***
		(0.005)		(0.004)		(0.005)
$Leverage(Mkt)$		−1.334***		−1.291***		−1.355***
		(0.031)		(0.028)		(0.030)
ROA		0.500***		0.323***		0.304***
		(0.041)		(0.037)		(0.040)
Zero Innovation (0/1)	−0.016	0.004	0.112***	0.088***	0.062***	0.041***
	(0.012)	(0.011)	(0.007)	(0.006)	(0.004)	(0.003)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,908	76,908	76,908	76,908	76,908	76,908
Adj. R^2	0.602	0.660	0.613	0.665	0.600	0.654

Table 4: **Valuation Effects After Controlling for Dot-Com Effects**

We repeat the panel regression in Table 3, Panel B, but include two additional control variables, namely the interaction terms *Delaware* × *PostDotcom* and *Innovation* × *PostDotcom*. The dummy *PostDotcom* marks with one (and zero otherwise) all years after the dot-com bubble bust after the end of 1999, and the dummy *Delaware* marks all companies incorporated under Delaware state law with one (and zero otherwise). We winsorize all continuous variables at the 1st and 99th percentiles. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dependent variable:	*ln(Tobin's q)*					
Innovation measure:	*R&D spending*		*Dollar value of patents*		*Number of patents*	
	(1)	(2)	(3)	(4)	(5)	(6)
Innovation	0.555***	0.919***	0.883***	0.782***	0.561***	0.454***
	(0.051)	(0.069)	(0.035)	(0.033)	(0.076)	(0.075)
COW	−0.011	−0.006	−0.005	0.000	−0.015	−0.009
	(0.024)	(0.032)	(0.022)	(0.032)	(0.021)	(0.030)
COW × *Innovation*	−0.016	0.020	−0.061	−0.061	0.360	0.242
	(0.117)	(0.107)	(0.070)	(0.064)	(0.242)	(0.216)
Post-dot-com effects:						
Delaware × *PostDotcom*	−0.040	−0.039	−0.040	−0.037	−0.037	−0.034
	(0.030)	(0.036)	(0.029)	(0.035)	(0.031)	(0.036)
Innovation × *PostDotcom*	0.019	−0.004	−0.127**	−0.095*	−0.659***	−0.573***
	(0.123)	(0.112)	(0.060)	(0.055)	(0.236)	(0.209)
Firm controls:						
ln(Assets)		−0.060***		−0.080***		−0.075***
		(0.005)		(0.005)		(0.005)
Leverage(Mkt)		−1.334***		−1.291***		−1.353***
		(0.031)		(0.028)		(0.031)
ROA		0.500***		0.324***		0.304***
		(0.041)		(0.038)		(0.040)
Zero Innovation (0/1)	−0.015	0.004	0.112***	0.087***	0.061***	0.040***
	(0.012)	(0.011)	(0.007)	(0.006)	(0.004)	(0.003)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,908	76,908	76,908	76,908	76,908	76,908
Adj. R^2	0.602	0.660	0.613	0.665	0.600	0.654

Table 5: **Replicating Non-Delaware Effects of COW**

We replicate the panel regression in FHT Table 11, Panel A, which excludes Delaware-incorporated firms. We winsorize all continuous variables at the 1st and 99th percentiles. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dependent variable:	*ln(Tobin's q)*					
Innovation measure:	*R&D spending*		*Dollar value of patents*		*Number of patents*	
	(1)	(2)	(3)	(4)	(5)	(6)
Innovation	0.478***	1.055***	0.861***	0.773***	0.184	0.249
	(0.111)	(0.182)	(0.109)	(0.090)	(0.199)	(0.194)
COW	−0.011	−0.022	−0.001	−0.005	−0.014	−0.015
	(0.046)	(0.043)	(0.044)	(0.040)	(0.042)	(0.038)
COW × *Innovation*	−0.204	−0.111	−0.123	−0.066	−0.265	−0.278
	(0.180)	(0.209)	(0.166)	(0.130)	(0.590)	(0.472)
Firm controls:						
ln(Assets)		−0.050***		−0.073***		−0.064***
		(0.009)		(0.009)		(0.009)
Leverage(Mkt)		−1.273***		−1.238***		−1.294***
		(0.052)		(0.054)		(0.050)
ROA		0.604***		0.435***		0.427***
		(0.108)		(0.081)		(0.085)
Zero Innovation (0/1)	−0.027	0.001	0.104***	0.080***	0.059***	0.042***
	(0.030)	(0.029)	(0.017)	(0.015)	(0.013)	(0.011)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,271	25,271	25,271	25,271	25,271	25,271
Adj. R^2	0.609	0.674	0.621	0.679	0.608	0.668

Table 6: **Valuation Effects by Firm-Level COW Implementation**

We repeat the panel regression in Table 4, but add an additional interaction dummy *IMP* marking firms that implement the COW laws at the firm level. We winsorize all continuous variables at the 1st and 99th percentiles. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dependent variable:	*ln(Tobin's q)*					
Innovation measure:	*R&D spending*		*Dollar value of patents*		*Number of patents*	
	(1)	(2)	(3)	(4)	(5)	(6)
Innovation	0.551***	0.922***	0.873***	0.773***	0.557***	0.446***
	(0.052)	(0.068)	(0.037)	(0.036)	(0.077)	(0.083)
COW	−0.014	−0.013	−0.010	−0.007	−0.019	−0.016
	(0.027)	(0.032)	(0.025)	(0.032)	(0.024)	(0.031)
COW × *Innovation*	−0.034	0.010	−0.081	−0.080	0.415*	0.300
	(0.120)	(0.111)	(0.066)	(0.063)	(0.246)	(0.218)
IMP × *Innovation*	0.370*	0.134	0.114**	0.113**	0.067	0.196
	(0.206)	(0.165)	(0.051)	(0.047)	(0.300)	(0.323)
IMP × *COW*	0.028	0.082	0.051	0.092	0.052	0.096
	(0.068)	(0.069)	(0.055)	(0.060)	(0.063)	(0.063)
IMP × *COW* × *Innovation*	1.009*	0.909*	0.254	0.244	−0.821	−0.970
	(0.546)	(0.460)	(0.213)	(0.187)	(0.795)	(0.755)
Post-dot-com effects:						
Delaware × *PostDotcom*	−0.035	−0.032	−0.033	−0.030	−0.031	−0.028
	(0.034)	(0.038)	(0.032)	(0.037)	(0.034)	(0.038)
Innovation × *PostDotcom*	0.046	0.019	−0.089	−0.061	−0.653***	−0.575***
	(0.125)	(0.114)	(0.062)	(0.060)	(0.240)	(0.209)
IMP × *PostDotcom*	0.018	0.018	0.025	0.028	−0.012	−0.006
	(0.047)	(0.051)	(0.046)	(0.050)	(0.047)	(0.050)
IMP × *Delaware* × *PostDotcom*	−0.057	−0.089	−0.086	−0.099	−0.064	−0.080
	(0.085)	(0.090)	(0.075)	(0.084)	(0.086)	(0.091)
IMP × *Innovation* × *PostDotcom*	−1.467**	−1.232**	−0.447**	−0.419**	−0.113	0.081
	(0.616)	(0.527)	(0.213)	(0.188)	(0.909)	(0.907)
Firm controls:						
ln(Assets)		−0.060***		−0.080***		−0.075***
		(0.005)		(0.005)		(0.005)
Leverage(Mkt)		−1.334***		−1.291***		−1.353***
		(0.031)		(0.028)		(0.030)
ROA		0.500***		0.324***		0.305***
		(0.041)		(0.038)		(0.040)
Zero Innovation (0/1)	−0.015	0.004	0.112***	0.087***	0.061***	0.040***
	(0.012)	(0.011)	(0.007)	(0.006)	(0.004)	(0.003)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,908	76,908	76,908	76,908	76,908	76,908
Adj. R^2	0.602	0.660	0.613	0.665	0.600	0.654

Table 7: **COW Effects on Inventor Mobility and Productivity**

Panels A and B replicate FHT Table 3 using a sample consisting of 6,115,363 inventor-employer-year observations for 797,524 unique inventors employed in the United States from 1996 to 2018, with the fixed effects and standard error clustering as used in FHT software codes. Panel C applies the fixed effects and standard error clustering as described in their paper. Panel D repeats the analyses in Panel C, using a sample that excludes inventors from private firms. Inventors' employers are determined by the assignees of inventors' patents. We use the patent dataset from Kogan et al. (2017) to determine if the assignee is a public firm. *Move (0,1)* is a dummy that is one if an inventor changes employers and zero otherwise. An inventor is considered to have changed employers when two consecutive patents filed by the same inventor display different assignees. The timing for the employer change is determined by the mid-point of the respective patent application years. The dummy variable *Move to a startup (0,1)* identifies inventors who switch to startup employers. A startup is defined as a private company whose the first patent grant is invented by the focal inventor following the change of the employer. Superstar inventors are defined as inventors whose granted patents in the sample collectively receive more than 90 citations. *Superstar Move (0,1)* and *Superstar Move to a startup (0,1)* respectively indicate employer changes by superstar inventors and superstar inventors specifically moving to startups. *Number of Patents* denotes the number of patents filed by an inventor in a given year and *Number of Citations* refers to the future citations received by these patents. *Generality* and *Originality* separately measure patent generality and originality. For fixed effects, CPC section and CPC subclass are used to measure the technology sector for different specifications. They are defined at the firm level and based on the most dominant CPC section or subclass in which the firm filed the largest number of patents over the past five filing years.

Panel A: Summary Statistics

Variable	Mean		SD		Q1		Median		Q3	
	Rep. (1)	FHT (2)	Rep. (3)	FHT (4)	Rep. (5)	FHT (6)	Rep. (7)	FHT (8)	Rep. (9)	FHT (10)
COW	0.338	0.333	0.473	0.471	0	0	0	0	1	1
Inventor mobility (0/1)										
Move	0.129	0.128	0.335	0.335	0	0	0	0	0	0
Move to a startup	0.013	0.012	0.113	0.139	0	0	0	0	0	0
Superstar move	0.079	0.079	0.270	0.270	0	0	0	0	0	0
Superstar move to a startup	0.008	0.007	0.087	0.108	0	0	0	0	0	0
Innovation productivity										
Number of patents	0.947	0.946	1.970	1.977	0	0	1	1	1	1
Number of citations	13.984	14.053	99.080	99.339	0	0	0	0	5	5
Generality	0.148	0.148	0.250	0.250	0	0	0	0	0.282	0.282
Originality	0.213	0.213	0.273	0.273	0	0	0	0	0.470	0.471

Table 7, continued

Panel B: Replication results following FHT software code

Dependent variables:	Inventor mobility (0/1)				Innovation productivity			
	Move	*Move to a startup*	*Superstar move*	*Superstar move to a startup*	*Number of patents*	*Number of citations*	*Generality*	*Originality*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
COW	0.015***	0.004***	0.003	0.001***	−0.007***	−0.007***	0.001	0.003***
	(0.006)	(0.000)	(0.003)	(0.000)	(0.001)	(0.002)	(0.001)	(0.001)
Inventor FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CPC Section × Year FEs	Yes	No	No	No	Yes	No	Yes	Yes
CPC Subclass × Year FEs	No	Yes	Yes	Yes	No	Yes	No	No
S.E. Cluster	Firm	Firm	Firm	Firm	NA	NA	NA	NA
Observations	6,115,363	6,115,363	6,115,363	6,115,363	3,732,496	3,732,496	3,732,496	3,732,496
Adj. R^2	0.141	0.070	0.141	0.024	0.260	0.319	0.188	0.190

Panel C: Regressions with reported fixed effects and standard error clustering

Dependent variables:	Inventor mobility (0/1)				Innovation productivity			
	Move	*Move to a startup*	*Superstar move*	*Superstar move to a startup*	*Number of patents*	*Number of citations*	*Generality*	*Originality*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
COW	0.015***	0.005***	0.004	0.002***	−0.007	−0.005	0.001	0.003
	(0.006)	(0.000)	(0.004)	(0.000)	(0.007)	(0.012)	(0.003)	(0.003)
Inventor FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CPC Section × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	6,115,363	6,115,363	6,115,363	6,115,363	3,732,496	3,732,496	3,732,496	3,732,496
Adj. R^2	0.141	0.065	0.137	0.021	0.260	0.298	0.175	0.177

Panel D: Regressions using inventors from public firms only

Dependent variables:	Inventor mobility (0/1)				Innovation productivity			
	Move	*Move to a startup*	*Superstar move*	*Superstar move to a startup*	*Number of patents*	*Number of citations*	*Generality*	*Originality*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
COW	−0.007	−0.000	−0.003	0.000	−0.004	0.008	−0.001	0.004
	(0.010)	(0.000)	(0.007)	(0.000)	(0.010)	(0.017)	(0.004)	(0.004)
Inventor FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CPC Section × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	3,420,660	3,420,660	3,420,660	3,420,660	2,192,476	2,192,476	2,192,476	2,192,476
Adj. R^2	0.130	0.165	0.125	0.111	0.275	0.314	0.185	0.196

Table 8: **Takeover Defenses**

This table reports changes in the takeover defense index around firms' implementation of COWs. Panel A presents summary statistics, and Panel B reports regression results. *E-index* is obtained from Bebchuk et al. (2009). The dummy variable *IMP* identifies firms that have implemented COWs. All continuous variables are winsorized at the 1st and 99th percentiles. Control variables and fixed effects are the same as those in Table 4. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Summary Statistics

Variable	Obs.	Mean	SD	Q1	Median	Q3
IMP	12,278	0.093	0.290	0	0	0
COW	12,278	0.446	0.497	0	0	1
$E\text{-}index$	12,278	2.302	1.304	1	2	3
$ln(Assets)$	12,278	7.221	1.410	6.219	7.068	8.084
$Leverage(Mkt)$	12,278	0.153	0.144	0.030	0.121	0.233
ROA	12,278	0.134	0.107	0.089	0.136	0.190

Panel B: Regression Results

Dependent variable	*E-index*	
	(1)	(2)
COW	0.060	0.058
	(0.129)	(0.133)
$IMP \times COW$	-0.398^{***}	-0.405^{***}
	(0.117)	(0.118)
$Delaware \times PostDotcom$	Yes	Yes
$HighR\&D \times PostDotcom$	Yes	Yes
$IMP \times PostDotcom$	Yes	Yes
$IMP \times HighR\&D \times PostDotcom$	Yes	Yes
$IMP \times Delaware \times PostDotcom$	Yes	Yes
Firm controls	No	Yes
Firm FEs	Yes	Yes
HQ State×Year FEs	Yes	Yes
Industry×Year FEs	Yes	Yes
Observations	12,278	12,278
Adj. R^2	0.879	0.879

Table 9: **Director Overlap**

This table reports panel regressions of intra-industry board overlap on state-level COW legislation and their firm-level implementation. Panel A presents summary statistics, and Panel B reports regression results. We measure board overlap alternatively as (i) *% Intra-industry Board Overlap* defined as the number of external board seats a firm's directors hold within the same three-digit SIC industry relative to the firm's board seats and as (ii) *# Intra-industry Board Overlap* as the number of external board seats of the firm's directors. *HighR&D* is a dummy variable indicating firms whose R&D expenditure relative to asset size ranks in the top quintile of all firms with non-zero R&D, based on data from the start of the sample period. For firms entering the sample later, their initial year's R&D data is used. All continuous variables, including both outcome variables, are winsorized at the 1st and 99th percentiles. Control variables and fixed effects are the same as those in Table 4, Panel B. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Summary Statistics

Variable	Obs.	Mean	SD	Q1	Median	Q3
IMP	55,565	0.131	0.337	0	0	0
COW	55,565	0.616	0.486	0	1	1
% Intra-industry Board Overlap	55,565	0.076	0.183	0	0	0
# Intra-industry Board Overlap	55,565	0.560	1.377	0	0	0
ln(Assets)	55,565	5.879	1.956	4.456	5.811	7.224
Leverage(Mkt)	55,565	0.137	0.154	0.003	0.087	0.217
ROA	55,565	0.060	0.217	0.039	0.109	0.166

Panel B: Regression Results

Dependent variables	*% Intra-industry Board Overlap*		*# Intra-industry Board Overlap*	
	(1)	(2)	(3)	(4)
COW	0.001	0.000	−0.016	−0.021
	(0.010)	(0.010)	(0.071)	(0.072)
COW × *HighR&D*	0.064***	0.065***	0.462***	0.470***
	(0.022)	(0.023)	(0.161)	(0.168)
IMP × *COW*	−0.014	−0.012	−0.160	−0.143
	(0.021)	(0.019)	(0.126)	(0.105)
IMP × *COW* × *HighR&D*	0.094***	0.093***	0.566***	0.553***
	(0.003)	(0.003)	(0.019)	(0.016)
Delaware × *PostDotcom*	Yes	Yes	Yes	Yes
HighR&D × *PostDotcom*	Yes	Yes	Yes	Yes
IMP × *PostDotcom*	Yes	Yes	Yes	Yes
IMP × *HighR&D* × *PostDotcom*	Yes	Yes	Yes	Yes
IMP × *Delaware* × *PostDotcom*	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Firm FEs	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes
Observations	55,565	55,565	55,565	55,565
Adj. R^2	0.765	0.765	0.773	0.774

Internet Appendix

Corporate Opportunity Waiver Laws
Did Not Produce Disloyal Managers

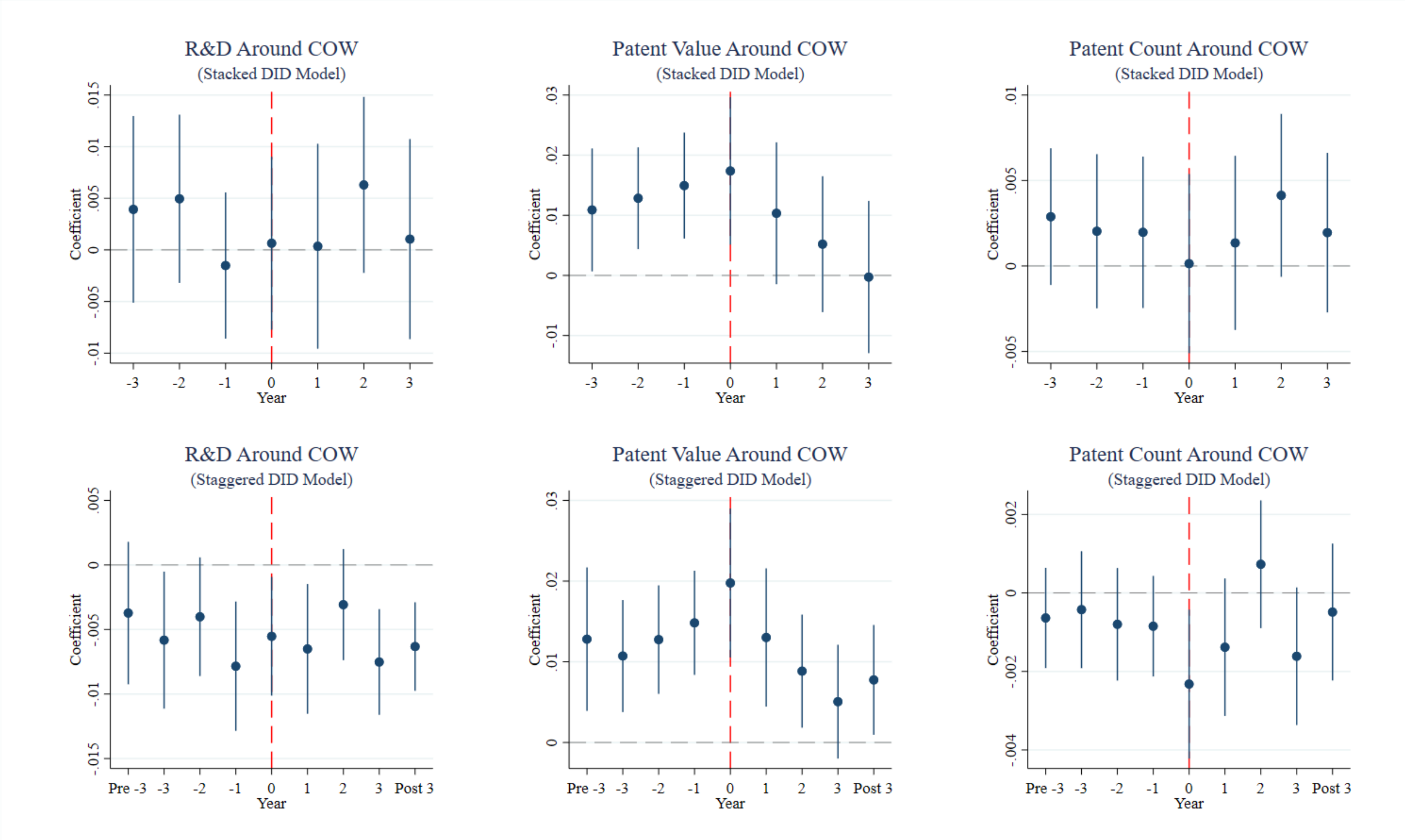

Figure A.1: The Effect of State COW Legislation on Corporate Innovation: Historical Incorporation State

This figure repeats Figure 1 but uses historical incorporation state data collected by Spamann and Wilkinson (2019) to replace the header incorporation state in Compustat. Vertical bars represent 90% confidence intervals. Standard errors are clustered at the level of the incorporating state.

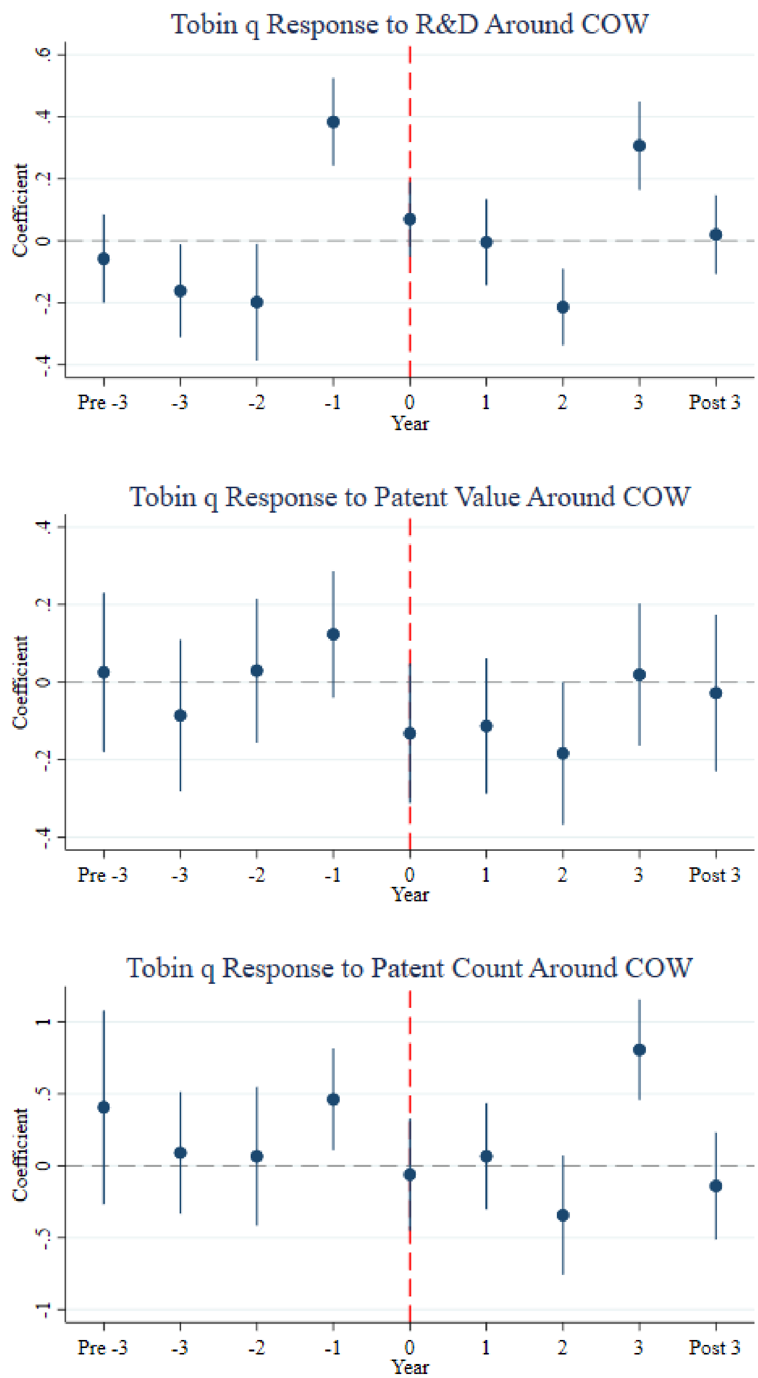

Figure A.2: The Dynamic Effect of COW Legislation Using Historical Incorporation State

This figure repeats Figure 2 but uses historical incorporation state data collected by Spamann and Wilkinson (2019) to correct the header incorporation state in Compustat. Vertical bars represent 90% confidence intervals. Standard errors are clustered at the level of the incorporating state.

Table A.1: **Discrepancies Between Journal Publication and Software Code**

Issues Identified	**Publication**	**Software Code Deposited at RFS**	**Software Code Provided by FHT for Figure 1**
Figure 1: Regression specification	As in our eq. (1): $Innovation_{i,c,t} = \sum_{T=-3}^{3} \alpha_T Treat_{i,c} \mathbf{1}(T) + \theta_{c,i} + \gamma_{c,s,t} + \delta_{c,j,t} + \epsilon_{i,c,t}$	N.A.	$Innovation_{i,c,t} = \sum_{T=-3}^{3} \alpha_T Treat_{i,c} \mathbf{1}(T) + Treat_{i,c} \times \theta_{c,i} + Treat_{i,c} \times \gamma_{c,s,t} + Treat_{i,c} \times \delta_{c,j,t} + \epsilon_{i,c,t}$
Figure 1: Outlier handling	Winsorize all continuous variables at 1st and 99th percentiles	N.A.	Trim R&D and Patent Dollar Value at 2.5th and 97.5th percentiles. No trimming or winsorization is applied to other continuous variables.
Table 2: Outlier handling	Winsorize all continuous variables at 1st and 99th percentiles	Trim R&D and Patent Dollar Value at 1st and 99th percentiles. No trimming or winsorization is applied to other continuous variables.	
Table 2: Sample period	1996-2017	1996-2018	
Table 2: Definition for size control	The natural log of the market value of equity	The natural log of total assets.	
Table 2: Definition for leverage	$\frac{DLTT+DLC}{AT-CEQ+CSHO \times PRCC\_F}$ The numerator is long-term debt ($DLTT$) plus current debt (DLC). The denominator is total book assets (AT) minus the book value of equity (CEQ) plus shares outstanding ($CSHO$) times close price at the end of fiscal year ($PRCC\_F$).	$\frac{DLTT+DLC}{DLTT+DLC+CSHPRI \times PRCC\_F}$ The numerator is long-term debt ($DLTT$) plus current debt (DLC) plus common shares used to calculate EPS ($CSHPRI$) times close price at the end of fiscal year ($PRCC\_F$).	
Table 2: Definition for Number of Patents	Weighting of each patent by the mean number of patents granted in the same year and technology class	No weighting adjustment is applied.	
Table 3: Sample selection	Inventors from public firms	Inventors from both public and private firms	

Issues Identified	Publication	Software Code Deposited at RFS	Software Code Provided Upon Our Request
Table 3: Definition for superstar inventor	Inventors in the top 25% of all sample inventors based on the number of patents granted by the USPTO.	Inventors whose patents granted by the USPTO have collectively received more than 90 citations. The threshold of 90 citations corresponds to the 83rd percentile among all sample inventors.	
Table 3: Standard error clustering	All regressions cluster standard errors at the firm level.	The stated standard error clustering is not applied in the regressions reported in FHT Table 3, Panel C, despite being applied in Panel B regressions of a similar nature.	
Table 3: Fixed effects	$CPC\ Section \times Year$ fixed effects for all regressions. CPC Section is labeled 'Tech sector'.	$CPC\ Subclass \times Year$ fixed effects are used in FHT's Columns 2–4 of Panel B and Column 2 of Panel C, whereas $CPC\ Section \times Year$ fixed effects are used in FHT's Column 1 of Panel B and Columns 1, 3, and 4 of Panel C. The CPC subclass represents a more granular technology classification than the CPC section. The inventor sample comprises over 600 CPC subclasses grouped into 9 CPC sections.	

Table A.2: **Variable Description**

Variable	Description
Firm or state level variables:	
COW	A dummy variable equal to one if a firm's incorporating state has enacted COW laws by the end of the firm's fiscal year, and zero otherwise. Source: CCM
Tobin's q	The natural log of market value of assets over book value of assets: $ln(\frac{AT-CEQ+CSHO\times PRCC\_F}{AT})$. The market value of assets is calculated as total assets (AT) minus book equity (CEQ) plus the market value of equity (shares outstanding $CSHO$ multiplied by share price $PRCC\_F$). Source: CCM
R&D Spending	R&D expenditure (XRD) divided by total assets (AT). Missing R&D value is set to zero. Source: CCM
Dollar value of patents	The total estimated dollar value of patents granted to a firm in a year divided by total assets reported in the same year. Source: Kogan et al. (2017) and CCM
Number of patents	The number of patents granted to a firm in a year divided by total assets reported in the same year. Source: Kogan et al. (2017) and CCM
Market value of equity	Shares outstanding $CSHO$ multiplied by share price $PRCC\_F$ in 2001 constant dollars. Source: CCM
Total assets	The value of total book assets (AT) in 2001 constant dollars. Source: CCM
Leverage(Mkt)	Total debt divided by the market value of assets. It is calculated as $\frac{DLTT+DLC}{AT-CEQ+CSHO\times PRCC\_F}$. Total debt is the sum of long-term debt ($DLTT$) and current debt (DLC). The market value of assets is calculated as total assets (AT) minus book equity (CEQ) plus the market value of equity (shares outstanding $CSHO$ multiplied by share price $PRCC\_F$). Source: CCM
ROA	Return on assets. It is calculated as operating income before depreciation ($OIBDP$) divided by total assets (AT). Source: CCM
Zero Innovation	A dummy variable that is equal to one if the corresponding innovation variable is zero and equal to zero otherwise.
Delaware	A dummy variable that is one if a firm is incorporated in Delaware and zero otherwise. Source: CCM
PostDotcom	A dummy variable that is equal to one if the year is 2000 or later.
IMP	A time-invariant dummy that is one if a firm has ever implemented COW throughout the sample and zero otherwise. Source: Self-collected data and Rauterberg and Talley (2017)
HighR&D	A time-invariant dummy that is one if a firm is among the top quintile of research expenditure relative to total assets and zero otherwise. The ranking is based on data from the start of the sample period. For firms entering the sample later, their initial R&D data is used. Source: CCM
% Intra-industry Board Overlap	The ratio of external board seats held by a firm's directors within the same three-digit SIC industry relative to the total number of the firm's board seats. Source: CCM and BoardEx
# Intra-industry Board Overlap	The absolute number of external board seats of the firm's directors. Source: CCM and BoardEx
E-index	Entrenchment index developed by Bebchuk et al. (2009).

Variable	Description
Inventor level variables:	
Move (0,1)	A dummy variable that is one if an inventor changes employers and zero otherwise. An inventor is considered to have changed employers when two consecutive patents filed by the same inventor display different assignees. The timing for the employer change is determined by the mid-point of the respective patent application years. Source: PatentsView
Move to a startup (0,1)	A dummy variable that is one if inventors switch to startup employers and zero otherwise. A startup is defined as a private company whose first granted patent is invented by the focal inventor following the employer change. See the definition for *Move (0,1)* on how the employer change is determined. Source: PatentsView
Superstar Move (0,1)	A dummy variable that is one if a superstar inventor changes employers and zero otherwise. Superstar inventors are defined as inventors whose granted patents in the sample collectively receive more than 90 citations. See the definition for *Move (0,1)* on how the employer change is determined. Source: PatentsView
Superstar Move to a startup (0,1)	A dummy variable that is one if a superstar inventor changes to startup employers and zero otherwise. Superstar inventors are defined as inventors whose granted patents in the sample collectively receive more than 90 citations. A startup is defined as a private company whose first granted patent is invented by the focal inventor following the change of the employer. See the definition for *Move (0,1)* on how the employer change is determined. Source: PatentsView
Number of patents	The natural logarithm of one plus the number of patents filed by an inventor at a firm in a given year. The number of patents filed by an inventor is normalized by the average number of patents filed by all inventors at firms in that year across the sample. Source: PatentsView
Number of citations	$\ln\big(1 + 1000 \times \sum_{i \in P_{j,t}} \frac{C_i}{\overline{N}_{s_i,t}}\big)$, where $i \in P_{j,t} := \{\text{patent } i \text{ filed by inventor } j \text{ in filing year } t\}$, C_i: forward citations of patent i; s_i: CPC section of patent i; $\overline{N}_{s_i,t}$: mean number of patents in section s_i in filling year t across all firms. Source: PatentsView
Generality	The average generality score of all patents filed by an inventor within a firm in a given year, based on the patent filing year. A patent's generality score is one minus the Herfindahl index across CPC sections of patents that cite the focal patent. The generality score is bias-corrected using the factor $N/(N-1)$, where N is the number of citations received by the focal patent. Source: PatentsView
Originality	The average originality score of all patents filed by an inventor within a firm in a given year, based on the patents' filing year. A patent's originality score is defined as one minus the Herfindahl index calculated across the CPC sections of the patents cited by the focal patent. The originality score is bias-corrected using the factor $N/(N-1)$, where N is the number of citations made by the focal patent. Source: PatentsView
CPC section	CPC section, such as 'A' for Human Necessitates. Source: PatentsView
CPC subclass	CPC subclass, such as 'A63B', which is narrower than CPC section. Source: PatentsView

Table A.3: Steps Towards COW Implementation Data

Below, we describe how we identify firms' COW adoption using EDGAR filings. Our approach is similar to that of Rauterberg and Talley (2017).

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1. **Retrieving regulatory filings from EDGAR:** We identify 9,138 unique firms with non-missing CIKs for 76,908 firm-year observations reported in Table 3. We then obtain all regulator filings of these firms during 1997-2018 from `https://www.sec.gov/Archives/edgar/full-index/`.

2. **Locating filings that may contain COWs:** We search each filing for paragraphs that contain (i) a word from the set {'renounce', 'waive', or 'disclaim'} and (ii) a phrase from the set {'business opportunity', 'corporate opportunity', or 'commercial opportunity'}. Variations in singular/plural forms, verb tenses, and letter casing are accounted for. A paragraph is defined as a block of text separated by '\n'. If a paragraph contains fewer than 100 words, we expand it by including the immediately preceding and following paragraphs to ensure sufficient context for evaluating COWs. After this step, 8,954 filings corresponding to 1,719 distinct firms remain. The paragraphs extracted from these filings are fed into the large language model (LLM) to determine whether they describe COW adoptions.

3. **Assessing COW using large language models:** We employ OpenAI's GPT-4o LLM to evaluate whether the paragraphs identified in the previous step contain COWs. The prompt we use is "*You are an expert in corporate law. Review the following text and determine if the company waives the corporate opportunity doctrine. Respond with exactly one word: ”Yes” if the company waives the doctrine. ”No” if it does not. Do not explain. Do not include any other text. Only respond with ”Yes” or ”No”.*" Based on the LLM assessment, 4,377 filings corresponding to 1,058 distinct firms contain COW adoption.

4. **Validation:** We manually verify all cases in which the LLM identifies a COW. Because our analyses seek to separate firms with at least one instance of COW adoption from those that never adopt COWs, we apply the following validation rule: for firms with multiple flagged filings, we review them one by one but stop the process as soon as one COW is confirmed. After verification, 1,024 distinct firms are confirmed to have at least one instance of COW adoption. We remain mindful of the possibility of false negatives. To evaluate this, we randomly select 500 filings in which the LLM do not identify a COW. Among these, we find that the LLM mis-identifies eight filings—associated with six distinct firms—that contain COWs, suggesting a false negative rate of 1.6% at the filing level. However, since firms typically disclose COWs in multiple filings, the LLM correctly identifies COW adoption for all six firms in their other filings. Therefore, the risk of false negatives at the *firm level* is likely much lower than the 1.6% *filing-level* rate. The firm-level error rate is also more relevant to our analyses.

5. **Compile the final firm-level COW list:** We compare our self-collected list of firms with COW adoption to the list compiled by Rauterberg and Talley (2017). Their list indicates 710 firms with COW adoption in our sample, which is fewer than 1,024 firms identified using our approach. Of these, 567 firms are identified by both methods. To understand the discrepancy involving the remaining 143 firms (=710-567), we go back to 8,954 filings identified in Step 2 and manually review all filings related to these 143 firms. We confirm that the majority of these firms appear to be misidentified by Rauterberg and Talley (2017)'s self-trained LLM model. Only 3 out of 143 firms are further identified. The large amount of mis-identification cases is unsurprising as Rauterberg and Talley (2017) used a relatively old AI model (Bert) and did not manually verify those COW cases identified by the model. Our final list includes 1,027 firms.

Table A.4: **Replicating Valuation Effects by Innovation Intensity: Historical incorporation**

This table repeats Panel B of Table 3 but uses historical incorporation state data collected by Spamann and Wilkinson (2019) to replace the header incorporation state in Compustat. The dependent variable is *Tobin's q* defined as the market value of assets divided by the book value of assets. The dummy variable *COW* is one if a firm's state of incorporation has passed the legislation of Corporate Opportunity Waivers by the fiscal year-end date, and zero otherwise. Three innovation proxies are (1) *R&D spending* defined as the R&D expenditure divided by total book assets; (2) *Dollar value of patents* defined as the nominal dollar value of new patents (Kogan et al., 2017) divided by total book assets, and (3) *Number of patents* defined as the yearly number of new patents scaled by total book assets. Control variables include the log of total assets, market leverage, return on assets, and a dummy variable indicating if the innovation variable is equal to zero. We winsorize all continuous variables at the 1st and 99th percentiles. The standard errors are clustered at the incorporating state level and are in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dependent variable:	*ln(Tobin's q)*					
Innovation measure:	*R&D spending*		*Dollar value of patents*		*Number of patents*	
	(1)	(2)	(3)	(4)	(5)	(6)
Innovation	0.569***	0.925***	0.825***	0.736***	0.400**	0.313**
	(0.051)	(0.076)	(0.068)	(0.054)	(0.168)	(0.123)
COW	−0.047***	−0.039***	−0.041***	−0.032***	−0.046***	−0.035***
	(0.012)	(0.010)	(0.011)	(0.010)	(0.011)	(0.009)
COW × *Innovation*	−0.018	0.005	−0.127**	−0.108**	−0.181	−0.230**
	(0.053)	(0.051)	(0.054)	(0.044)	(0.127)	(0.110)
Firm Controls:						
ln(Assets)		−0.060***		−0.080***		−0.075***
		(0.008)		(0.007)		(0.008)
Leverage(Mkt)		−1.333***		−1.290***		−1.354***
		(0.027)		(0.024)		(0.025)
ROA		0.500***		0.323***		0.304***
		(0.040)		(0.036)		(0.038)
Zero Innovation (0/1)	−0.015	0.004	0.112***	0.087***	0.062***	0.041***
	(0.014)	(0.012)	(0.009)	(0.007)	(0.006)	(0.003)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
HQ State×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,908	76,908	76,908	76,908	76,908	76,908
Adj. R^2	0.604	0.661	0.615	0.666	0.601	0.655