Internet Appendix for

“Director-Liability-Reduction Laws and Conditional Conservatism”

We report several validation and specification checks in this Appendix. Our results are robust to (1) using Byzalov and Basu’s [2016] asymmetric timeliness of accruals model, (2) controlling for the enactment of antitakeover laws and state-level macroeconomic characteristics, (3) the placebo test suggested by Patatoukas and Thomas [2016], (4) including additional firm characteristics as controls, (5) multiple alternative sample-selection criteria, (6) self-selection for state of incorporation, and (7) including state-specific linear time trends in the fixed effects structure of equation (3). We also verify the parallel trends assumption for our setting.

1. ACCRUAL ASYMMETRY AND DISAGGREGATED BAD NEWS INDICATORS

Byzalov and Basu [2016] show that accruals exhibit an asymmetry with respect to disaggregated non-return bad news indicators including negative sales growth, negative employee growth, and negative CFO.\(^1\) Their model extends the Dechow and Dichev [2002], Ball and Shivakumar ([2005]; [2006]), and Allen, Larson, and Sloan [2013] models as follows:

\[
ACC_{i,t} = \alpha_i + \omega_t + \beta_1 SGR_{i,t} + \beta_2 EGR_{i,t} + \beta_3 CF_{i,t-1} + \beta_4 CF_{i,t} + \beta_5 CF_{i,t+1}
+ \beta_6 DS_{i,t} + \beta_7 DE_{i,t} + \beta_8 DC_{i,t-1} + \beta_9 DC_{i,t} + \beta_{10} DC_{i,t+1} + \beta_{11} SGR_{i,t}
\times DS_{i,t} + \beta_{12} EGR_{i,t} \times DE_{i,t} + \beta_{13} CF_{i,t-1} \times DC_{i,t-1} + \beta_{14} CF_{i,t}
\times DC_{i,t} + \beta_{15} CF_{i,t+1} \times DC_{i,t+1} + \epsilon_{i,t} \tag{A1}
\]

where \(ACC\) is accruals scaled by beginning-of-year total assets, \(SGR\) is sales growth, \(EGR\) is employee growth, \(CF\) is operating cash flow scaled by beginning-of-year total assets, \(DS\), \(DE\), and \(DC\) are indicators for negative \(SGR\), \(EGR\), and \(CF\), respectively. Byzalov and Basu [2016] predict and find that conservatism leads to positive \(\beta_{11}\), \(\beta_{12}\), \(\beta_{14}\), and \(\beta_{15}\) and negative \(\beta_{13}\).

\(^1\) Patatoukas and Thomas [2011], [2016] argue that the conservatism measure from the Basu [1997] model is biased because of the return distribution. Because the Byzalov and Basu [2016] model examines non-return indicators, it should not suffer from this alleged bias. In addition, Collins, Hribar and Tian [2014] report that the Patatoukas and Thomas [2011] bias is concentrated in CFO and does not affect the operating accruals that we analyze here.
We apply the Byzalov and Basu [2016] model by interacting every term except the fixed effects with \(POST\). We also use a similar fixed-effects structure to equation (3) in the main text for this model (i.e., the state- and year- fixed-effects structure is added for \(\beta_j, j = 1, 2, \ldots, 15\)). We predict that, because conditional conservatism decreases after the new laws, the coefficients of the additional interaction terms will be opposite in sign to those in the Byzalov and Basu [2016] model. We require all our observations to have sales revenue and number of employee data. Following Banker, Basu, Byzalov, and Chen [2016], we also eliminate observations with more than 50 percent change in either sales or number of employees to remove firm-years that potentially had non-articulating transactions such as mergers & acquisitions and significant divestitures that systematically impact accruals through channels other than conservatism (Hribar and Collins [2002]).

Table A1 reports the estimation results. We find that the coefficient signs are all consistent with our predictions. Three out of five coefficients are statistically significant at the 5 percent level or better. The coefficients are not all statistically significant perhaps because the bad news indicators often occur simultaneously, which can cause high multicollinearity. For example, the correlation between \(POST \times SGR \times DS\) and \(POST \times EGR \times DE\) is 0.52.

2. INTERACTION BETWEEN TIMING OF ANTITAKEOVER LAWS AND DIRECTOR-LIABILITY-REDUCTION-LAW ENACTMENTS

Jayaraman and Shivakumar [2013] predict and find that business combination laws, a type of antitakeover law, lead to an increased supply of conditional conservatism to counter an amplified agency problem between managers and shareholders/debtholders. Because many business combination laws were enacted during our sample period, we re-estimate our regression by controlling for these laws as follows:
\[ EARN_{i,t} = \alpha_t + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} + POST_{i,t} \times \left( \beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t} \right) + BC_{i,t} \times \left( \beta_8 + \beta_9 RET_{i,t} + \beta_{10} NEG_{i,t} + \beta_{11} RET_{i,t} \times NEG_{i,t} \right) + \epsilon_{i,t} \]  

where \( BC \) is an indicator that equals one if the observation is after the passage of a business combination law and equals zero otherwise. We predict that \( \beta_7 \) is negative and \( \beta_{11} \) is positive. To make our results comparable to those of Jayaraman and Shivakumar [2013], we follow their sample selection criteria and use a subsample that (1) stops at 1995, (2) drops the business combination law-enactment-year observations, and (3) ensures that each firm has data from at least one year before and one year after the business combination law enactments.

Table A2 reports the results of the regression model (A2). In Columns I and II, we estimate two specifications, one with only the year-fixed effects structure for the two-way Basu coefficient (i.e., \( RET \times NEG \)) to be consistent with Jayaraman and Shivakumar [2013] and the other with the complete fixed-effects structure as in equation (3) in the main text, because Cheng, Duru, and Zhao [2017] report that the Jayaraman and Shivakumar results disappear if the state-fixed effects structure is used. We find that \( \beta_7 \) is negative and significant for both specifications, consistent with our main results. Furthermore, because Karpoff and Wittry [2018] find that the business combination laws may not be the only effective antitakeover laws, we extend model (9) to control for the enactment of four other types of antitakeover laws (i.e., control share acquisition law, fair price law, directors’ duties law, and poison pill law) in Column III. Also, to ensure that the results are not driven by the first-generation antitakeover laws, we exclude observations before 1983 and find consistent results again.\(^2\) Consistent with Jayaraman and Shivakumar [2013], we find that conditional conservatism increases after the enactments of business combination laws in Columns I and III. Conditional conservatism does not increase in Column II, consistent with Cheng, Duru,

\(^2\) In a robustness check, we further exclude the firms that lobbied for the antitakeover laws as identified by Karpoff and Wittry [2018, Table 3] and find consistent results.
and Zhao [2017]. However, the changes in conditional conservatism are not statistically significant following the enactments of other types of antitakeover laws (untabulated).

Related to the above analysis but more generally, we also consider the possible endogenous timing of law changes. The endogeneity may arise from two sources, reverse causality and correlated omitted variables. We argue that reverse causality is unlikely because the states enacted the laws to lower non-officer-directors’ litigation risk and D&O insurance premium, not to respond to or facilitate aggressive accounting. If the potential correlated omitted variables are time-invariant for each state or have a homogeneous impact on the entire market for each period, our fixed-effects structure controls sufficiently. However, there may be other state-level time-varying variables such as state gross domestic product (GDP) that simultaneously affect law enactment and conditional conservatism. To address this alternative explanation, we follow Jenkins [1995] and estimate a logit hazard model to examine whether the timing of the law enactments can be predicted by state-level time-varying variables. The dependent variable is an indicator of whether the director-liability-reduction law was enacted for a particular state-year combination. Once a state enacts the law, all future observations of that state are dropped from the sample. The hazard rate $h$ (i.e., the probability that the director-liability-reduction law is enacted in that year, conditional on the law not been enacted yet) for state $j$ and year $t$ is as follows:

$$h_{j,t} = \text{Logit}(\omega_t + \sum_{k=1}^{K} \left( y_k \bar{X}_{k,j} + \delta_k (X_{k,j,t} - \bar{X}_{k,j}) \right))$$  

(A3)

where $X_{k,j,t}$ is the $k$th state-level variable for state $j$ in year $t$, $\bar{X}_{k,j}$ is the within-state average of this variable, and $\omega_t$ represents year fixed effects.$^3$

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$^3$ We do not include state fixed effects in the hazard rate function because they will “perfectly explain” the timing of law enactments (i.e., each state fixed effect explains the law enactment timing for each state) and cause all the other coefficients to remain unidentified.
Equation (A3) distinguishes a time-invariant component of \( X \) (i.e., average of \( X \), denoted as \( \bar{X} \)) and a time-varying component of \( X \) (i.e., \( X - \bar{X} \)). The coefficient of interest is \( \delta_k \). In other words, if \( \delta_k \) is statistically significant, we should control for the variable in our regression to avoid potential endogeneity. In contrast, \( \omega_t \) and \( \gamma_k \) are less important for us because the market-wide trend and time-invariant component of state-level variables are already controlled for by the fixed-effects structure. We select variables that may have a strong impact on law enactment, including (1) log of the number of public firms incorporated in the state, (2) log of the number of public firms headquartered in the state, (3) business combination law dummy, and more generic variables, including (4) log of total GDP and (5) log of per capita GDP.\(^4\) To estimate the model, we construct a sample starting in 1985, the year when the *Smith v. Van Gorkom* suit took place. In untabulated tests, we find that all the coefficients are statistically insignificant except for the average of log of the number of public firms incorporated in the state (\( p \)-value = 0.078). This finding suggests that states with more incorporated firms are more likely to enact the director-liability-reduction laws early, presumably because the D&O insurance crisis affected more firms in those states, and hence, enacting the laws became more urgent. Because the fixed-effects structure already controls for the average log of the number of public firms incorporated in the state, it will not bias our main findings. Finally, we also explicitly control for these five variables as well as their interactions with \( RET, NEG \), and \( RET \times NEG \) in our regression. We find that our results are robust.

3. PARALLEL TRENDS

To verify that the parallel trends assumption holds, we examine when the change in conditional conservatism occurred (cf. Autor [2003]; Acharya, Baghai, and Subramanian [2014]).

\(^4\) We obtain state-level GDP and population data from U.S. Bureau of Economic Analysis website https://www.bea.gov/regional/ (downloaded on 11/09/2017).
Specifically, we set the conditional conservatism five or more years before the enactment of the laws as a benchmark and examine whether conditional conservatism decreased before the enactment of the laws as follows:

\[
EARN_{i,t} = \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} \\
+ POST_{i,t} \times (\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}) \\
+ \sum_{\tau=1}^{4} PRE_{\tau,i,t} \times (\beta_{8,\tau} + \beta_{9,\tau} RET_{i,t} + \beta_{10,\tau} NEG_{i,t} + \beta_{11,\tau} RET_{i,t} \times NEG_{i,t}) + \epsilon_{i,t}
\]

(A4)

where \(PRE_\tau\) is an indicator variable that equals one if the observation is from \(\tau\) \((\tau = 1, 2, 3, 4)\) year(s) before law enactment and equals zero otherwise.

If the parallel trends assumption holds, the coefficient \(\beta_{11,\tau}\) should not be statistically significant (i.e., conditional conservatism does not differ between firms in different states before the laws are enacted). Table A3 reports the results of regression (A4). The coefficient \(\beta_{11,\tau}\) is statistically insignificant for every \(\tau\), which is consistent with the parallel trends assumption.

4. PATATOUKAS AND THOMAS [2016] PLACEBO TEST

In our main analysis, we follow Ball, Kothari, and Nikolaev [2013] and use firm fixed effects to resolve the measurement bias problem discussed by Patatoukas and Thomas [2011]. We also examine the asymmetric timeliness of accruals because Collins, Hribar, and Tian [2014] point out that the bias problem can also be resolved when the dependent variable of the Basu [1997] model is replaced by accruals (scaled by beginning-of-year market value of equity). Moreover, our analysis based on the Byzalov and Basu [2016] model is also unaffected by the bias because this model tests non-return indicators, while Patatoukas and Thomas [2011] suggest that the bias stems from returns.

However, Patatoukas and Thomas [2016] find that the Ball, Kothari, and Nikolaev [2013] and Collins, Hribar, and Tian [2014] approaches may not fully address their problem and suggest
a placebo test that we implement. Specifically, we replace the dependent variable of our model with $-1/P_{i,t-1}$, a placebo. Patatoukas and Thomas [2016] argue that we should not find a statistically significant coefficient for the three-way interaction term $RET_{i,t} \times NEG_{i,t} \times POST_{i,t}$ in the placebo regression because $-1/P_{i,t-1}$ is not related to the news in year $t$. Our results are consistent with this prediction ($p$-value = 0.538), and the magnitude of the coefficient is also much smaller (-0.063 vs. -0.221). Alternatively, we also include this placebo variable along with its interactions with the Basu slope coefficients in the main model and find that our results continue to hold.

5. OTHER ROBUSTNESS CHECKS

The paper’s hypotheses are still supported after the following robustness checks:

- Controlling for variation in the sources of demand for conditional conservatism (Ball and Shivakumar [2005]; Khan and Watts [2009]) by including size, book-to-market ratio, leverage, and their interactions with all the terms in the Basu [1997] model.

- Including change in revenue (defined as the difference between current-period sales and prior-period sales scaled by beginning-of-year market value of equity), sales decline dummy variable (equals 1 if change in sales is negative), and their interaction, to control for earnings asymmetry induced by cost stickiness (Banker, Basu, Byzalov and Chen [2016]).

- Eliminating Delaware-incorporated firms because a large fraction of U.S. public firms incorporate in Delaware to take advantage of its corporate law institutions and reforms (Romano [2006]), and may systematically differ from firms incorporated in other states.

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5 Patatoukas and Thomas [2016] report that $1/P_{i,t-1}$ exhibits “anticonservatism” and comoves with asymmetric timeliness measures but in an opposite direction. Therefore, we multiply their placebo by -1.
• Eliminating all the firms that were incorporated in states that enacted the law in 1986 (including Delaware) to get rid of first-mover effects. Alternatively, we include an indicator for first-mover states in the regression. We find that the reduction in asymmetry is slightly greater for first-mover states than late-mover states (-0.028) and significant at the 10 percent level.

• Separately analyzing the six states that relaxed culpability standards and the vast majority of states that allowed limited-liability charter amendments (Romano [2006]). We find that the change in conservatism following the two types of director-liability-reduction laws are both negative and significant but do not differ significantly from each other (p-value = 0.868), consistent with Bradley and Schipani’s [1989] finding that the two approaches differed little in practice.

• Eliminating all firms that were incorporated in states that also allowed limits on officers’ liability (Brook and Rao [1994]), because this would increase manager-shareholder agency conflicts. Conditional conservatism could fall further because managers now worry less about their litigation risk (Chung and Wynn [2008]), or increase because of demand from other stakeholders (LaFond and Roychowdhury [2008]; Jayaraman and Shivakumar [2013]). We manually check the corporate laws of each state and find that six states also let shareholders limit or eliminate officers’ liabilities as of 2019. We find that conditional conservatism falls significantly after both types of laws. The coefficient reduction is smaller for the six states that limited officers’ liability, but the difference is not statistically significant (p-value = 0.757).

• Including state-specific linear time trends in the fixed-effects structure of equation (3) in the main text (i.e., \( \beta_j = \sum_k (\delta_{j,k} State_k + \eta_k State_k \times t) + \sum_M \theta_{j,m} Year_m \)). Such trends can

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6 Damage caps approach is not considered separately because Virginia was the only state to adopt it in 1987, and because Virginia also adopted limited-liability charter amendments in the following year, 1988.

further control for any state-level linear time-varying factors that influence conditional conservatism.

- Including observations between 2003 and 2013 in our sample instead of ending our sample in 2002.

- Shrinking our sample period to reduce the potential for other confounding events. For this robustness check, we start the sample in 1983 to include at least three years of data before law enactments. We end the sample in 1995 because all states except Missouri and West Virginia had enacted the director liability laws by 1994. Although our sample size falls by 44 percent, the $\beta_7$ coefficient is very similar (-0.211 vs. -0.221) and statistically significant ($p$-value = 0.026).

- Including all firm-year observations during our sample period instead of requiring at least one observation both before and after a law enactment for each sample firm to control for industry composition changes due to relaxed stock-exchange-listing rules (Srivastava [2014]).

- Excluding observations from the insurance crisis years (1984 and 1985) as directors may have responded to the volatile D&O insurance market and their reduced insurance coverage.

- Eliminating the 6.2 percent of the firms in our original sample that changed their state of incorporation after 1986, the year the first states enacted a director-liability-reduction law, to exclude any firms choosing states to incorporate in based on their director-liability-reduction laws.

- Excluding utility firms from our sample as they are regulated.


- Excluding firm-years that experienced non-articulating transactions such as mergers & acquisitions and divestitures (Hribar and Collins [2002]). Following Banker, Basu, Byzalov, and Chen [2016], we drop firm-year observations if the absolute change in sales revenue is greater than 50 percent from the prior year.
• Including law-enactment-year observations and treating them as post-law-enactment observations (i.e., \( \text{POST} = 1 \)).

References


PATATOUKAS, P., and J. THOMAS. “More Evidence of Bias in the Differential Timeliness Measure of


Table A1. Director-Liability-Reduction Law and Asymmetric Timeliness of Accruals to Disaggregated Bad News Indicators

<table>
<thead>
<tr>
<th>Dependent Variable: $ACC$</th>
<th>Prediction</th>
<th>I</th>
</tr>
</thead>
<tbody>
<tr>
<td>$POST$</td>
<td>0.001</td>
<td>(0.17)</td>
</tr>
<tr>
<td>$POST \times SGR \times DS$</td>
<td>$-0.055$</td>
<td>(-0.31)</td>
</tr>
<tr>
<td>$POST \times EGR \times DE$</td>
<td>$-0.139^{***}$</td>
<td>(-2.91)</td>
</tr>
<tr>
<td>$POST \times CF_{t-1} \times DC_{t-1}$</td>
<td>$0.377^{***}$</td>
<td>(4.22)</td>
</tr>
<tr>
<td>$POST \times CF_{t} \times DC_{t}$</td>
<td>$-0.530^{**}$</td>
<td>(-2.19)</td>
</tr>
<tr>
<td>$POST \times CF_{t+1} \times DC_{t+1}$</td>
<td>$-0.122$</td>
<td>(-1.05)</td>
</tr>
</tbody>
</table>

*Interactions between POST and Byzalov-Basu Terms*  
*Year Fixed Effects (Main)*  
*Firm Fixed Effects (Main)*  
*Year Fixed Effects (Byzalov-Basu Coefficients)*  
*State Fixed Effects (Byzalov-Basu Coefficients)*

<table>
<thead>
<tr>
<th>Observations</th>
<th>26,325</th>
</tr>
</thead>
<tbody>
<tr>
<td>$R^2$</td>
<td>0.805</td>
</tr>
</tbody>
</table>

This table presents regression results of accruals on sales growth, employee growth, cash flows, bad news indicators, post-director-liability-law indicator, and their interaction terms. The fifteen terms of the Byzalov and Basu model are subsumed by the fixed effects structure; thus their coefficients are not reported. All continuous variables are winsorized at the extreme percentiles. *, **, *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; $t$-statistics are reported in parentheses and are based on standard errors clustered at the state level.
Table A2. Controlling for Antitakeover Law Enactments

<table>
<thead>
<tr>
<th>Dependent Variable: EARN</th>
<th>Prediction</th>
<th>I</th>
<th>II</th>
<th>III</th>
</tr>
</thead>
<tbody>
<tr>
<td>POST</td>
<td>-0.011</td>
<td>-0.016**</td>
<td>-0.009</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-1.11)</td>
<td>(-2.15)</td>
<td>(-0.73)</td>
<td></td>
</tr>
<tr>
<td>POST × NEG</td>
<td>0.010**</td>
<td>-0.001</td>
<td>-0.009</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.45)</td>
<td>(-0.09)</td>
<td>(-0.39)</td>
<td></td>
</tr>
<tr>
<td>POST × RET</td>
<td>0.063***</td>
<td>0.082**</td>
<td>0.061</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7.09)</td>
<td>(2.07)</td>
<td>(1.22)</td>
<td></td>
</tr>
<tr>
<td>POST × NEG × RET</td>
<td>-</td>
<td>-0.154**</td>
<td>-0.250***</td>
<td>-0.235**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(-2.55)</td>
<td>(-3.09)</td>
<td>(-2.51)</td>
</tr>
<tr>
<td>BC</td>
<td>0.010**</td>
<td>0.005</td>
<td>0.008</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.60)</td>
<td>(0.97)</td>
<td>(1.47)</td>
<td></td>
</tr>
<tr>
<td>BC × NEG</td>
<td>0.003</td>
<td>-0.001</td>
<td>0.005</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.87)</td>
<td>(-0.21)</td>
<td>(0.52)</td>
<td></td>
</tr>
<tr>
<td>BC × RET</td>
<td>-0.012</td>
<td>-0.003</td>
<td>-0.016*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-1.30)</td>
<td>(-0.27)</td>
<td>(-1.71)</td>
<td></td>
</tr>
<tr>
<td>BC × NEG × RET</td>
<td>+</td>
<td>0.081***</td>
<td>0.024</td>
<td>0.067*</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(4.34)</td>
<td>(1.03)</td>
<td>(1.79)</td>
</tr>
</tbody>
</table>

Year Fixed Effects (Main) | Yes | Yes | Yes |
Firm Fixed Effects (Main)  | Yes | Yes | Yes |
Year Fixed Effects (Basu Coefficients) | Yes | Yes | Yes |
State Fixed Effects (Basu Coefficients) | No | Yes | Yes |
Other Types of Antitakeover Laws | No | No | Yes |
Observations               | 23,227 | 23,227 | 16,504 |
R²                          | 0.597  | 0.610  | 0.514  |

This table presents regression results of earnings on stock return, negative stock return dummy, post director liability law dummy, business combination law dummy, and their interaction terms. Column III is generated using observations after (including) 1983 only, and the regression also controls for the impact of other types of antitakeover laws. The three terms of the Basu model (i.e., NEG, RET, NEG × RET) are subsumed by the fixed effects structure, thus their coefficients are not reported. All continuous variables are winsorized at the extreme percentiles. *, **, and *** indicate \( p < 0.10, p < 0.05, \) and \( p < 0.01, \) respectively, for a two-tailed test; \( t \)-statistics are reported in parentheses and are based on standard errors clustered at the state level.
### Table A3. Test for Parallel Trends Assumption

<table>
<thead>
<tr>
<th>Dependent Variable: EARN</th>
<th>Prediction</th>
<th>1</th>
</tr>
</thead>
<tbody>
<tr>
<td>$PRE_1 \times NEG \times RET$</td>
<td>0</td>
<td>-0.027 (-0.57)</td>
</tr>
<tr>
<td>$PRE_2 \times NEG \times RET$</td>
<td>0</td>
<td>-0.045 (-0.98)</td>
</tr>
<tr>
<td>$PRE_3 \times NEG \times RET$</td>
<td>0</td>
<td>0.033 (0.72)</td>
</tr>
<tr>
<td>$PRE_4 \times NEG \times RET$</td>
<td>0</td>
<td>0.006 (0.14)</td>
</tr>
<tr>
<td>$POST \times NEG \times RET$</td>
<td>–</td>
<td>-0.234** (-2.54)</td>
</tr>
</tbody>
</table>

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$POST$, $POST \times NEG$, and $POST \times RET$</td>
<td>Yes</td>
</tr>
<tr>
<td>$PRE_i$, $PRE_i \times NEG$, and $PRE_i \times RET$</td>
<td>Yes</td>
</tr>
<tr>
<td>Year Fixed Effects (Main)</td>
<td>Yes</td>
</tr>
<tr>
<td>Firm Fixed Effects (Main)</td>
<td>Yes</td>
</tr>
<tr>
<td>Year Fixed Effects (Basu Coefficients)</td>
<td>Yes</td>
</tr>
<tr>
<td>State Fixed Effects (Basu Coefficients)</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>32,418</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.562</td>
</tr>
</tbody>
</table>

This table presents regression results of earnings on stock return, negative stock return dummy, post director liability law dummy, dummies for one to four years before law enactment, and their interaction terms. The three terms of the Basu model (i.e., $NEG$, $RET$, $NEG \times RET$) are subsumed by the fixed effects structure, thus their coefficients are not reported. All continuous variables are winsorized at the extreme percentiles. *, **, *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; $t$-statistics are reported in parentheses and are based on standard errors clustered at the state level.