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Abstract:

Recent studies have debated the impact of investor protection laws on firms' corporate policies. I exploit the passage of state investor protection statutes ("blue sky laws") in the U.S. in the early 20th century to estimate the effects of investor protection law on firm financing decisions and investment activity. Regression estimates indicate that the passage of investor protection statutes causes firms to pay out greater dividends, issue more equity, and grow in size. The introduction of investor protection law is also associated with improvements in operating performance and market valuations. Additional analysis suggests that alternative hypotheses for the measured changes in corporate policy and performance have limited explanatory power. Overall, the evidence is strongly supportive of theoretical models which predict that investor protection laws have a significant impact on firm financing and investment policy.

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Introduction

Recent studies in corporate governance have debated the importance of legal institutions in shaping financial development. One particular aspect of the legal environment which has received significant attention is the role of legal protection of investors from managerial expropriation. Both the theoretical and the empirical literature reach mixed conclusions on the impact of investor protection laws on firm financing and investment decisions. On one hand, a number of theories predict that investor protection laws have a significant impact on corporate policies. These theories are supported by empirical studies which find cross-country differences in firm financing and investment patterns. These differences are attributed to heterogeneous investor protections engendered by disparate legal origins (La Porta, Lopez-de-Silanes, Shleifer, and Vishny (henceforth LLSV) 1997, 1998, 2000b, 2002, among others). Some scholars, on the other hand, argue that cross-country differences in financing and investment patterns do not adequately capture the causal impact of legal development on corporate policy (Rajan and Zingales 2003, Pagano and Volpin 2001, 2005). Additionally, various studies examine time-series variation in investor protection laws within countries such as the U.K. and Italy and find that legal protection of investors has little impact on ownership dispersion and financial development (Franks, Mayer, and Rossi 2009, Aganin and Volpin 2005).

Empirical identification of the impact of investor protection law on firm financing and investment decisions requires a setting in which legal investor protections are well-defined and vary independently of factors that are otherwise correlated with firms' decisions on corporate policy. I exploit the staggered passage of state investor protection statutes, also known as the blue sky laws, in the United States during the early 1900's to identify the causal impact of investor protection laws on the financing and investment decisions of firms in the mining industry. I compare the impact of investor protection laws on the financing and investment decisions of firms in states which passed blue sky laws during the sample period to contemporaneous changes in the corporate policies of firms in other states. I also evaluate the relative operating performance and market valuations of firms during the sample period.

This paper addresses several critical limitations of previous empirical studies on the impact of investor protection law on firm corporate policy and performance. First, it focuses on cross sectional and time series variation in investor protection laws within a single common law country (legal origin is fixed across states). Second, I evaluate the impact of laws that were specifically aimed at reducing expropriation of shareholders by corporate insiders in an environment where investor fraud was rampant (Seligman 2003). The legal protections engendered by the laws are arguably more closely linked to the theories developed by LLSV (1997) than the measures of investor protections used in many other studies (LLS 2008, Coffee 2001).[1] Third, I exploit exogenous variation in investor protection laws that is likely independent of unobserved variables which otherwise impact firm financing and investment decisions. Examples of such variables are political lobbying efforts, changes in investment opportunities, and unobserved economic and financial shocks (an identification assumption explored in greater detail below).

Estimates of the impact of state investor protection statutes suggest that greater investor protections cause sample treatment firms to increase dividend payouts to shareholders by approximately 10%. Treatment firms also raise issuances of common stock outstanding by at least 11%. Additionally, the blue sky laws cause firms to increase total levels of physical capital by at least 36% while total firm assets increase by at least 17%. The results are strongly supportive of theoretical models which predict that investor protection laws have a significant impact on the financing and investment policies of firms (LLSV 1997, 1998, LLS 2008, Shleifer and Wolfenzon 2002). Improvements in the legal protection of investors are also associated with increases in operating performance; return on assets (ROA) generally increases by at least 6% for sample firms while market-to-book valuations, measured by market capitalization to cash flow ratios, increase significantly.

The causal interpretation of the regression estimates relies on the identification assumption that the passage of the investor protection statutes by various states in the U.S. during the early 20th century is independent of other factors which would otherwise

[1] In particular, the early 20th century was characterized by widespread securities fraud in the mining industry (Mahoney 2003).

impact corporate policy and performance. This assumption is supported by numerous sources. Furthermore, various hypotheses suggesting a violation of this assumption are empirically tested and found to lack significant explanatory power.

First, as discussed further in Section 2, there is ample anecdotal evidence that the passage of the laws is largely due to increased securities fraud which took place at the turn of the century (Seligman 2003). The laws appear to be driven exclusively by the public interests of security market participants at the time; this claim is further corroborated by the fact that eventually all states (except Nevada) chose to adopt blue sky laws. It is possible, however, that the laws may have been influenced by the political lobbying efforts of incumbent firms. As discussed by Rajan and Zingales (2003), many investor protection laws could simply result from incumbent firms who wish to reduce competition from potential market entrants by raising the costs of external financing. As a consequence of reduced entry by competitors, incumbent firms would increase profits due to their improved monopoly power in product markets. This hypothesis would predict that the passage of investor protection laws would allow incumbent firms to increase profits by producing fewer quantities of goods at higher prices. Using product market data, however, I find that the passage of the blue sky laws instead led to increases in the quantities of goods sold, while the prices of goods remained unchanged or lower; i.e. product markets appear to be more competitive after the laws are passed. Thus, the passage of the blue sky laws does not appear to be driven by product market gains for incumbent firms.

Second, I present two sets of analyses which examine whether the passage of the blue sky laws appears to be correlated with either gradual or immediate changes in unobservable investment opportunities.[2] I find that there are no significant pre-existing trends in corporate policy and performance prior to the passage of the blue sky laws. The estimated impact of the laws is significant only after the laws are passed. I also examine the effects of the blue sky laws on firms located in Michigan and West Virginia. Both states initially passed blue sky laws in 1913, however, the laws were declared unconstitutional by federal courts soon thereafter. The states then passed modified

[2] The analyses are also relevant for addressing the importance of other potential unobserved covariates that could be correlated with the passage of the laws.

versions of the laws in 1915 which were upheld by the Supreme Court. I observe changes in corporate policy and performance for firms in these two states only after the laws are passed in 1915 – not after the laws are passed initially in 1913. The evidence suggests that regression estimates do not suffer from biases resulting from unobserved changes in investment opportunities or other unobserved factors that are correlated with the passage of the blue sky laws. Third, I examine whether firms respond to the passage of the investor protection statutes by relocating their states of incorporation. All but one sample firm, however, do not change location during the sample period, suggesting that regression estimates do not suffer from selection bias.

I also test whether the observed impact of the blue sky laws occurs through the channel of reduced expropriation risk or through the channel of reduced adverse selection in security dealer markets. The blue sky laws provided investors with a legal basis for recovering damages from firms that were deemed to be fraudulent following public security issuances. In addition, many laws also required that securities (and securities dealers) be registered with the government prior to any public issuance. It is therefore possible that the impact of the blue sky laws is manifest through the channel of reduced adverse selection at the pre-clearance stage rather than through the reduced risk of expropriation by insiders conditional on securities already being issued. This hypothesis is evaluated using a subsample of data consisting of firms in states which passed laws exclusively centered on ex-post fraud reduction rather than ex-ante screening of securities. I find a significant impact of the laws on corporate policy for this set of firms, suggesting that the blue sky laws had an impact through reduced expropriation risk by insiders.

This paper contributes to two related strands of literature. First, this paper utilizes a unique dataset and empirical approach to provide evidence that investor protection laws have a significant causal impact on corporate policy and performance. The findings add to research that examines the linkages between legal institutions and financial development.[3] The results are strongly supportive of the theoretical models developed by

[3] Papers in this area include Benmelech and Bergmann (2008), Carlin and Gervais (2008), Demirguc-Kunt and Maksimovic (1998), Fisman and Love (2004), Foley and Greenwood (2009), Glaeser, Johnson, and Shleifer (2001), King and Levine (1993), Levine (1997), La Porta, Lopez-de-Silanes, and Shleifer (2003, 2007), Rajan and Zingales (1998), and Wurgler (2000), among many others.

LLSV (1997, 1998) (among others). Second, this paper contributes to recent debate over the development of the emerging financial markets of Eastern European and Asian countries by highlighting the potential outcomes of reforms aimed at protecting investors from securities fraud (Klapper and Love, 2002).

The remainder of the paper proceeds as follows. Section 2 contains institutional background describing the blue sky laws and the various political economy explanations for their passage. Section 3 describes the data. Section 4 contains the analysis. Section 5 concludes.

Section 2: Institutional Background

The blue sky laws were securities fraud statutes passed by various states between 1911 and 1931 to prevent investor expropriation by insiders such as managers and securities dealers. The laws required that security issuers and dealers register with state governments (typically the state banking departments) prior to issuing public securities, and receive approval from the government before selling any securities in the state (Mahoney 2003). Perhaps most importantly, the laws also provided investors with a legal basis and cause of action for recovering assets fraudulently expropriated by security issuers or salesmen, even after a significant passage of time between the initial security purchase and the time of the alleged expropriation (Read and Washburn 1921, Virginia Law Review 1937).

Prior to the Securities Act of 1933 and the formation of the SEC with the Securities Exchange Act of 1934, federal securities regulation in the United States was largely nonexistent (Seligman, 2003). In addition, there was little in the way of state securities market regulation prior to the passage of the blue sky laws (Macey and Miller 1991). Different states in the U.S. passed blue sky statutes at different times, exhibiting heterogeneity in the requirements that were placed upon securities issuers. The first investor protection law was passed in Kansas in 1911, the second in Arizona in 1912. In 1913, many states such as California, Maine, Missouri, Montana, and Texas passed blue sky laws. Two states, Michigan and West Virginia, passed laws in 1913, which were soon after declared unconstitutional by lower federal courts (*Alabama &c. Co. v. Doyle*, 201 Fed 173; *Compton v. Allen*, 216 Fed. 537; *Bracey v. Darst*, 218 Fed. 482). These

states later proposed and passed modified versions of the original laws in 1915. The modified statutes were upheld by the Supreme Court (*Hall v. Geiger-Jones,* 242 U.S. 539; *Caldwell v. Sioux Falls Co.*, 242 U.S. 559; *Merrick v. N.W. Halsey & Co.*, 242 U.S. 568). Almost all states in the sample passed blue sky laws that required registration and pre-clearance of securities prior to any public offerings. Maine was the lone exception; though its laws did provide a basis for claiming damages in the case of ex-post fraud (Mahoney 2003).[4]

The motivation for passing the investor protection statutes is largely attributed to rampant securities fraud at the turn of the 20th century (Seligman 1983, Reed and Washburn 1921, Mulvey 1914). Many scholars claim that a large number firms and securities salesmen took advantage of "naïve" investors by selling them securities backed by little more than a promise of extraordinary returns. In fact, the colloquial name of the 'blue sky' laws is attributed to the opinion of Justice Joseph McKenna in *Hall v. Geiger-Jones*:

> The name that is given to the law indicates the evil at which it is aimed – that is, to use the language of a cited case, "speculative schemes which have no more basis than so many feet of blue sky;" or, as stated by counsel in another case, "to stop the sale of stock in fly-by-night concerns, visionary oil wells, distant gold mines, and other like fraudulent exploitations."

The types of firms targeted by the laws were largely companies in the mining, oil, and gas sectors (Reed and Washburn 1921). Some states, such as North Dakota (Comp. Law, Secs. 4989-4994) and Connecticut (Conn. Gen. St., Sec. 3461-3464) went so far as to introduce statutes aimed specifically at regulating the sale of mining securities. Firms in these industries were among the most likely to approach investors with intangible assets and highly speculative business plans that would later turn out to be facades for fraudulent operations (Mahoney 2003). Examples of such fraud are plentiful. The following accounts from Nation's Business (1922) describe common occurrences among duped investors:

[4] The institutional features of the blue sky laws in Maine, Michigan, and West Virginia are utilized in the analysis to test the validity of a number of alternative hypotheses.

One man was induced to invest in a Mississippi oil company by a friend who had gone to work for the concern. Through the Investors' Protective Committee he finally learned the truth about the fake stock and further, "that there had never been a barrel of oil found in Mississippi," and that the company from which the securities were bought could not even be located.

In another instance, one woman wrote:

A year ago last July, 1920, a sleek, smooth-talking agent came to my house and began to talk oil to me, and he said my money would be giving me big dividends just as soon as the wells were operating….Well, the last I have heard is they cannot go on with the drilling until the investors come forward with another 20 percent cash payment on their investment, otherwise it's a foregone conclusion of the whole matter.

To combat such behavior, state legislators developed the blue sky laws to prevent fraudulent security issuances. The first law was passed by Kansas in 1911, promoted in large part by J. N. Dolley, the Kansas state banking commissioner. Dolley claimed in a set of newspaper articles in 1910 that Kansas widows were duped into purchasing fraudulent securities and that legislation was required to "remove these financial cancers entirely from [the] state" (Macey and Miller 1991). After Kansas approved its investor protection statute in 1911, many other states quickly followed suit; eventually all other states (excluding Nevada) passed laws similar to Kansas from 1912 to 1931.

A typical blue sky law would require a firm to submit information about its operations and financial characteristics prior to issuing securities. The types of information collected by regulators would include a listing of officers, board of directors, historical financial information, state of incorporation, properties, locations of headquarters and operating units. This data was collected to verify legitimacy of the offering and provide public records of company activity should the firm be liable for committing fraud after a security issuance. Firms would also be required to provide additional information at the request of state officials, either at the time of the proposed offering or any time thereafter. An example of such information was the proposed use of capital raised through the security issue. In addition, firms would be subject to appraisals, audits, and investigations of properties by state officials. Such reviews would be at the expense of the issuer rather than the government. If the security offering was approved by officials, the firm would receive a permit to sell securities to the public

within the state. Finally, the laws provided a cause of action for investors who claimed fraud by security issuers. If a firm was found liable by courts to have committed securities fraud, by absconding investor funds or misusing assets for example, then investors would be able to recover damages from the firm via judicial proceedings (Reed and Washburn 1921, Virginia Law Review 1937).

There is ample evidence that the laws had a binding effect on the behavior of security issuers both large and small. Mulvey (1914) performed an audit study of the regulatory actions of the state banking commission of Kansas, the earliest adopter of the blue sky laws, and found that within 2 years of the adoption of the law in 1911, Kansas had already denied the applications of 62 firms to sell their securities in Kansas. Additionally, the Kansas state banking commissioner issued a report in 1912 stating that between "fourteen and fifteen hundred companies have been investigated by this department since the enactment of this law, and of this, less than one hundred have been granted permits to sell their securities in Kansas" (Mulvey, 1914).

There were many reasons why security offerings were denied. For example, in 1924, the Continental Gas and Electric Corp., a 12 year old firm, wished to issue securities to purchase a controlling interest in the Kansas City Power and Light Company. Continental planned to raise at least $5,000,000 worth of stock in order to consummate the purchase. The commissioner of banking in Missouri, however, raised objections to the value of the Kansas City Power and Light Co., arguing that after the deduction of intangible items, cost of financing, and reserves for depreciation, the value of Kansas City Power and Light was too low to justify the proposed offering. As a result, Continental's application to issue stock in Missouri was denied in order to protect the public from an unsafe offering (Barron's, 1924).

The blue sky laws did not just impact firms at the security issuance stage. Often times, the laws would be invoked during court proceedings by investors who sought to recover damages from firms well after securities offerings (Reed and Washburn 1921, Virginia Law Review 1937). One example of such a case is *Edward v. Ioor* (205 Mich. 617), in which the court ruled in favor of the plaintiff who wished to receive compensation for activity committed by a firm in violation of the blue sky laws. Another example is *Kneeland v. Emerton* (280 Mass. 371).

The various ways in which the blue sky laws applied to securities offerings reflects a significant improvement in the legal protection of investors from securities fraud. Anecdotal evidence and academic research points to the reduction of securities fraud as the chief aim of regulators who passed the blue sky laws. Some argue, however, that it is possible there were other motivations for the passage of the statutes. For example, the laws may have been the outcomes of political processes that did not fully reflect the public interests of state constituents. As discussed by Macey and Miller (1991), it is conceivable that the laws were promoted by the private interests of small banks who wished to reduce competition with securities salesmen for depositors' funds. Macey and Miller (1991) posit that mandatory registration of securities and securities salesmen increased costs of security issuance. In response, firms would pass these increased costs onto investors, who would therefore prefer to invest in bank deposits rather than corporate securities. This theory would suggest that the passage of the blue sky laws would be associated with deleterious effects on firm corporate policies and performance.[5]

Another potential motivation for the passage of the blue sky laws is a variant of the political economy hypothesis developed by Rajan and Zingales (2003).[6] Rajan and Zingales (2003) argue that some of the differences in investor protection levels across countries can be attributed to the political influence of industrial incumbent firms that promote capital market regulations as means of limiting product market entry by competitors. In the context of the blue sky laws, this hypothesis would suggest that incumbent firms in various states were instrumental at promoting the passage of state investor protection statutes. While anecdotal evidence does not suggest that such lobbying took place (at least overtly), this hypothesis is directly tested in Section 4.

Section 3: Data

The dataset is constructed using several sources of information. First, I identify all mining firms which appear in the monthly "Banking and General Quotation" section of the December, 1915 issue of the Commercial and Financial Chronicle (CFC). I choose

[5] As discussed in more detail in Section 4, however, these predictions are rejected by the data.
[6] See Volpin and Pagano (2001, 2005) for similar models.

this set of firms for three reasons. First, this list of firms has publicly traded stock with published prices, allowing me to track share prices using publicly available information over time. Second, I start with a list of firms appearing in the 1915 issue of the CFC because firms that are publicly traded in this year are likely to maintain stock price and financial statement information for at least two years before and after 1913 (the year in which most states pass their respective investor protection statutes). Third, as discussed in Section 2, I focus on the mining industry because firms in this sector were considered the most likely to commit shareholder expropriation (Mahoney 2003, Seligman 2003) and are therefore a natural starting point for estimating the impact of investor protection laws on corporate policy. The CFC provides par values of stock, as well as bid and ask quotes for shares. I collect stock prices for this sample of firms from the December issue of the Commercial and Financial Chronicle from 1908 to 1917.

For each firm in the December, 1915 CFC mining stock quotation list, I then collect background characteristics and financial statement information in the 1913, 1916, and 1918 volumes of Poor's Manual of Industrial Securities. I also identify mining firms in the 1916 and 1918 Poor's volume for which I can collect similar information from 1911 to 1917. I collect brief descriptions of the company (such as products produced), state and year of incorporation, location of operations, names of officers and directors, name of exchanges on which its stock is traded, historical balance sheet and income statement data. I confirm whether each firm is in the mining industry from the company's name and from the company's business description. Each firm has varying amounts of information, both across accounting variables, as well as over time. Thus, the final dataset is an unbalanced panel of firm balance sheet data, income statement information, outstanding shares, and stock price information.

Many firms in the mining industry also publicly disclose "statements of operations" in the Poor's manuals. I use these statements to assemble product market data. I identify every firm in the 1916 Poor's manual that discloses information on the types of minerals or metal ores it produces, the quantity it sells each year, and the market prices of its products. I then augment the time series of information for these firms using the 1913 and 1918 Poor's manuals.

Given the data collection methods and institutional features of the blue sky laws, it is possible that there are several sources of selection bias in the regression estimates. First, because data are collected from Poor's manuals, it is likely that sample firms represent relatively larger, older companies in the U.S. since financial information about such companies was likely to be more accessible than that of smaller, younger firms. This source of bias is unlikely to be problematic, however, since larger, older firms would likely be impacted by the blue sky laws less than smaller, younger firms due to the likelihood of larger, older firms already having mitigated investors' concerns of expropriation through established reputations. Thus, such sample selection would likely cause the regression analysis to underestimate the true impact of the laws on the average firm in the population during the sample period.

Second, in response to the passage of the blue sky laws, it is conceivable that some firms would relocate their operations to new states of incorporation. For example, a poorly performing firm belonging to a state which passes a blue sky law may choose to reincorporate in a state without a blue sky law to avoid securities registration costs and securities fraud liability. Such behavior, however, is unlikely to be relevant for firms in our sample. Almost all sample firms are incorporated well before 1913 and do not reincorporate anywhere else during the sample period. Therefore, it is unlikely that regression estimates are biased by sample firm reincorporation decisions.[7]

Table 1 contains descriptive statistics of sample firm characteristics. There are a total of 108 unique firms with balance sheet data for the years 1899 to 1918, yielding a dataset of 887 firm-year observations.[8] As Panel A indicates, the average year of incorporation is 1899; almost all sample firms are incorporated several years before their respective states of incorporation pass investor protection laws. The average age of a firm in the sample is 13 years. Panel B summarizes sample firm balance sheet and income statement characteristics in 1911, separately for firms located in states which pass

[7] It is also unlikely that the estimates are biased by a "Delaware effect" (Daines 2001). As explained by Subramaniam (2004), Delaware's charter laws became an issue primarily in the 1960s after a number of law changes were passed which caused Delaware to become the most popular destination for much of the incorporation activity at the time. While today around half of all U.S. firms are incorporated in the U.S., less than 10% of sample firms are incorporated in Delaware. To further verify that Delaware incorporated firms do not bias the regression estimates, I run all regression in Section 4 with Delaware firms removed from the sample; all regression results remain the same.

[8] The vast majority of firms have data from 1910 to 1917.

blue sky laws during the sample period and firms located in states which pass blue sky laws after the sample period (i.e. after 1918). As explained in the Analysis section, because the laws are staggered across time and eventually passed by all sample states (except Nevada), the breakdown of firms into these two groups is somewhat artificial. In the estimation, any firm incorporated in a state which has not yet passed a law can be considered a control firm, while any firm incorporated in a state where a law has been passed can be considered a treatment firm. Statistics from 1911 are presented to summarize firm characteristics prior to any law being passed by any sample state; the first sample state to pass a law is Arizona in 1912. P-values from t-tests comparing firm characteristics between the two groups are also presented. Panel C of Table 1 contains a listing of sample states and the years in which they passed blue sky laws.

The average firm in states which pass laws after 1918 produces $2.4 million in sales in 1911, while the average firm in states which pass laws during the sample period generates revenues of $2.5 million. Both the level of sales and one-year sales growth rates between the two groups are statistically indistinguishable. The two sets of firms have approximately 76% of their assets in Plant, Property, and Equipment (book value of PPE is $7.0 million on average, while book value of assets is $9.3 million). Additional firm characteristics such as age, dividends-to-sales, return on assets, and market capitalization are statistically similar across both groups. The market capitalization of firms in the latter group appears somewhat smaller in magnitude. However, the large standard errors associated with the figures illustrate that the differences are due to the presence of several large outliers in the control sample; t-tests indicate that both sets of firms have statistically similar levels of market capitalization.[9] While the differing absolute values of market capitalization between the two sets of firms may suggest differences between the sets of firms, these differences are unlikely to be problematic for the estimation. First, it is important to note that the staggered nature of passage of the laws is utilized in the empirical framework to allow firms in states which pass blue sky laws during the sample period to serve as control firms in the estimation (in addition to firms in states which pass laws after 1918). Second, all results hold when the sample is

[9] Comparison of group medians using a Wilcoxon rank-sum test also indicates insignificant differences in market capitalization between the two groups.

restricted to firms in states which pass the laws during the sample period. Overall, the data suggest that there are no significant differences across firms in states which pass laws during the sample period.[10]

Section 4: Analysis

The effects of the blue sky laws on the financing and investment decisions of sample firms are estimated using the following Ordinary Least Squares (OLS) regression model:

$$\textit{Dependent Var.} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it} \quad (1)$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. In the subsections which follow, there are a number of different dependent variables which reflect various aspects of firms' corporate policies: $Dividends_{it}/Sales_{it}$, $Ln(Shares\ Oustanding_{it})$, $Ln(PPE_{it})$, $Ln(Assets_{it})$, ROA_{it}, and $Market\ Cap_{it}/Cash\ Flow_{it}$. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an Investor Protection (IP) Law by year *t*. $Ln(Age)_{it}$ is the log of the age of firm *i* in year *t*. This variable is added to control for differences in corporate policy related to firm age, as investment in newly formed firms typically begins at a higher rate than established firms. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. $SalesGrowth_{it}$ is the percentage change in sales of firm *i* from year *t-1* to year *t*. Following LLSV (2002), growth in sales is used as a proxy for investment opportunities.[11] It is worth noting that this measure of investment opportunities is likely endogenous with the passage of the laws, and therefore its exclusion from the specifications is preferred to its inclusion. However, this measure is included in some specifications to show that the results are not significantly affected by controlling for investment opportunities.

$IPLaw_{it}$ is effectively a difference-in-difference estimator of the impact of investor protection law on the dependent variable of interest. In this framework, any firm

[10] This issue is further addressed in the Analysis section.

[11] Because information in the liabilities portion of firms' balance sheets is poorly disclosed for mining firms in Poor's Manuals, it is difficult to precisely estimate book equity and debt, and therefore measure Tobin's Q or Market to Book Equity Ratios as alternative measures of investment opportunities.

incorporated in a state which has not yet passed a law can be considered a control firm, while any firm incorporated in a state where a law has been passed can be considered a treatment firm. This assignment into treatment and control groups is due to the staggered passage of the laws across states, all of which eventually adopt investor protection statutes except for Nevada. Firm fixed effects are included in all specifications to ensure that static firm-specific differences across firms do not account for patterns in investment and financing. Year fixed effects, which control for year-specific changes in investment and financing, are included in most regressions. Finally, standard errors in all regression are clustered by state in order to control for residual correlations of the error terms across firms within a given state.

4.1 Financing Decisions

4.1.1 Payout Policy

The impact of investor protection statutes on firm dividend payouts to stockholders as a fraction of total sales, $Dividend_{it} / Sales_{it}$, is estimated using the following specification:

$$Dividend_{it} / Sales_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(Firm_i) + \beta_4(Year_t) + \varepsilon_{it} \qquad (2)$$

where subscripts *it* uniquely identify an observation for firm *i* in year *t*. $Dividend_{it} / Sales_{it}$ is the ratio of total dividends to sales of firm *i* in year *t*. All other variables are defined as in Specification 1.

Table 2 contains the regression results. Columns 1-3 present estimates of Specification 2 using dividends to common stock holders as the measure of total dividends in a given year. The results indicate that the passage of the blue sky laws is associated with an economically large and statistically significant increase in the percentage of sales that are distributed as dividends. Column 1 indicates a percentage increase of 2.6%, however when controlling for year fixed effects and firm age, the estimate is even larger, on the order of 9.3% - 9.8%. The increase in the size of the estimate is not surprising, as there are likely time effects in firm sales and dividends across firms. The positive coefficient on firm *Age* indicates that older firms also pay

greater dividends, consistent with prior research. Columns 4-5 present estimates using the sum of dividends to common stock and preferred stock as the measure of total dividends in a given year. Though only seven firms in the sample offer preferred dividends in addition to common stock dividends, when such dividends are accounted for, the estimate of the blue sky law impact increases to 10.1%-10.5%. The slight increase in coefficient estimates implies that preferred dividends increase in response to the laws. It is worth noting that since preferred stock holders have greater cash flow rights than common stock holders, it is likely that they are less subject to insider expropriation than common stock holders, therefore the impact of preferred dividends is likely to be positive, but smaller than the impact of investor protection laws on common dividends. The results in Column 4-5 support this conjecture; furthermore, in results not reported here, when only preferred dividends-to-sales ratios are the dependent variable in Specification 2, the estimated coefficient for *IPLaw* is 0.13 with a robust standard error of 0.18.

The economic and statistical significance of the coefficient estimates for *IPLaw* are consistent with the theory that increased investor protections cause firms to increase dividend payments to investors. The estimates provide strong evidence consistent with the "outcome model" of LLSV (2000b). The outcome model of dividends predicts that when minority shareholders are better protected from expropriation by insiders, they are able to put more pressure on managers to disgorge cash and increase dividend payments. As discussed in Section 2, much of the fraud which took place in the mining industry during this time period stemmed from investors being falsely promised extraordinary dividends in return for the stock investments. When the blue sky laws were passed, the outcome theory suggests that investors were able to receive cash from operations that would be potentially expropriated by managers under the guise of legitimate excavation purposes. The results in Table 2 indicate that investors indeed received greater dividend payments from firms, consistent with the predictions of the model.

4.1.2 Equity Issuance

The impact of investor protection statutes on firm common stock outstanding, $Ln(Shares_{it})$, is estimated using the following specification:

$$Ln(Shares_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it} \quad (3)$$

where subscripts *it* uniquely identify an observation for firm *i* in year *t*. $Ln(Shares_{it})$ is defined as the log of common shares outstanding of firm *i* in year *t*.[12] All other variables are defined as in Specification 1.

Regression estimates are presented in Table 3. Column 1 indicates that outstanding equity increases by 14% once a blue sky law is passed.[13] When year fixed effects are included in Columns 2 and 3, and *Age* is included in Column 3, the estimate for *IPLaw* changes little, suggesting that equity issuance during the sample period is not significantly affected by aggregate fluctuations across years or firm age. When sales growth is controlled for, as in Columns 4 and 5, the estimated impact of the laws on equity issuance remains economically large and statistically significant. Across all specifications, the estimated impact of the blue sky laws on common stock outstanding is an increase of approximately 12-14%.

Overall, the findings are strongly supportive of theories which predict greater investor protections lead to increased participation in equity markets (LLSV 1997, Shleifer and Wolfenzon 2002). The evidence is also consistent with the hypothesis that increased investor protection from insider expropriation encourages firms to seek more outside financing because investors become more willing to partake in capital investment. This particular interpretation of the evidence stems from the assumption that new issuances of equity go to outside investors rather than insiders such as firm managers or directors. This assumption is motivated in two ways. First, the blue sky laws generally did not apply to issuances of securities to insiders such as managers or pre-existing stock holders, suggesting that if observed equity issuances were made to these groups, one

[12] Stock splits in the sample are rare. One treatment firm appears to have reduced shares outstanding by half after a law is passed. I do not make any adjustment for this observation, which slightly reduces the magnitude of the estimated coefficient for *IPLaw*.

[13] The estimated percentage change in the dependent variable given the passage of a blue sky law is given by $100*(\exp(\beta_1)-1)$., if the regression specification is estimated using the log of the dependent variable.

should not observe a change in equity outstanding in response to the passage of the laws (Reed and Washburn 1921). Second, the findings of Holderness, Kroszner, and Sheehan (1999) indicate that managerial stock ownership during the early part of the twentieth century was limited (at least relative to current times), suggesting that increases in common stock outstanding were unlikely to be realized by insiders exclusively.

The results are interesting given the institutional details surrounding the mining industry during the sample period. As discussed in Section 2, much of the fraud which took place in the mining industries stemmed from investors purchasing equity securities that did not offer any return due to insiders absconding with investment outlays. When the blue sky laws were passed, prohibiting such equity from being issued to the public, firms were ostensibly better able to raise stock from investors who were now protected from securities fraud. The findings in Table 3 confirm this hypothesis.

4.2 Firm Size

4.2.1 Physical Capital

The impact of investor protection statutes on firm size is estimated using two measures of size utilized in previous research: plant, property, and equipment (PPE) and book value of assets.[14] The impact of the blue sky laws on $Ln(PPE_{it})$ is estimated using the following specification:

$$Ln(PPE_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it} \quad (4)$$

where the dependent variable, $Ln(PPE_{it})$ is the log of the Plant, Property, and Equipment of firm *i* in year *t*. All independent covariates are the same as in Specification 1. Regression results are presented in Table 4.

The coefficient of *IPLaw* measures the approximate elasticity of physical capital to the passage of a blue sky law. Column 1 indicates the elasticity of physical capital to the passage of an investor protection law is approximately 48% (the point estimate of *IPLaw* is 0.394). When year fixed effects are included in Column 2, the point estimate

[14] The findings are quantitatively and qualitatively similar if market capitalization is used as an additional measure of firm size.

for *IPLaw* is .317, which is still economically large and statistically significant. The decrease in the coefficient illustrates the importance of aggregate fluctuations over time in investment. Column 3 illustrates that *Age* has little power in explaining firm investment in physical capital. The inclusion of *Sales Growth* in Columns 4-5 suggests that PPE is an expectedly increasing function of investment opportunities. Furthermore, the point estimate of *IPLaw* increases to approximately 0.38.[15] Overall, the various specifications in Table 4 indicate an economic and statistically significant impact of the blue sky laws on mean levels of physical capital.

4.2.2 Total Assets

The impact of investor protection statutes on firm assets, measured by $Ln(Assets_{it})$, is estimated using the following specification:

$$Ln(Assets_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it} \quad (5)$$

where the dependent variable, $Ln(Assets_{it})$ is the log of the book value of assets of firm i in year t. All independent covariates are the same as in Specification 1. Regression results are presented in Table 5.

The coefficient of *IPLaw* measures the approximate elasticity of firm assets to the passage of a blue sky law. Column 1 illustrates that the laws are associated with a 41% increase in firm assets. When year fixed effects and *Age* are included in Columns 2-3, the point estimate for *IPLaw* decreases to 0.16, which is statistically significant at the 10% level. The inclusion of Sales Growth in Columns 4-5 causes point estimates of *IPLaw* to rise to approximately 0.23[16].

The results in Tables 4 and 5 indicate that the blue sky laws have a significant impact on firm investment in physical capital and total firm size. The elasticity estimates of 41%-48% are somewhat large in economic magnitude, however, this is not

[15] As mentioned earlier, however, it is possible that *Sales Growth* is endogenous with the laws, thus causing point estimates of *IPLaw* in Columns 4-5 to overstate the impact of the blue sky laws on PPE. The main conclusion to be drawn from Columns 4-5 is that the inclusion of a proxy for investment opportunities does not minimize the impact of the laws on investment in physical capital.

[16] This change, however, is subject to the same caveats as those mentioned in Section 4.2.1.

unreasonable given the nature of the mining industry during the early 1900's. Many of the sample firms are copper, silver, and gold mining operations, in which the most significant assets owned by firms are the properties used for mineral excavation. Casual observation of the data suggests that very little physical capital is devoted to machinery and equipment; the most valuable asset owned by a company is the land on which it operates. Capital infusions spurred by the blue sky laws likely caused mining firms to increase total assets through purchases of large tracts of land or quarries, allowing for sudden, significant changes in physical capital and total asset size. Such activity stands in contrast to industries where large adjustments to firm size come in the form of additional labor or machinery and equipment; comparably large, immediate changes in firm size for these industries in response to the blue sky laws would be unlikely.

Overall, the economically large and statistically significant coefficient estimates of *IPLaw* in Tables 4 and 5 are consistent with theories that predict investor protection laws lead to increased firm size (LLSV 1998, Shleifer and Wolfenzon 2002, LLS 2008). The impact of the investor protection law on firm size is robust to the choice of econometric specification and the choice of the firm size measure. In the context of the mining industry, the blue sky laws allowed firms to increase firm size through increased investment in physical capital and property used for mineral excavation. This is interesting in light of the fact that much of the pre-existing securities fraud in the mining industry centered on investors being misled about the productivity of mining and drilling properties, as discussed in Section 2.

4.3 Operating Performance and Valuation

The estimated effects of the blue sky laws on firm financing and investment decisions indicate that firms respond to the laws by paying out greater dividends, raising equity, and increasing firm size. The findings are consistent with various theories regarding the importance of investor protections on corporate policy. However, the changing nature of firm behavior in response to the laws per se does not indicate whether the increased activity by firms represents profitable activity. To investigate this issue, I estimate the impact of the blue sky laws on firm profitability and market valuations.

4.3.1 Return on Assets

The impact of investor protection statutes on operating performance, ROA_{it}, is estimated using the following specification:

$$ROA_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(Firm_i) + \beta_4(Year_t) + \varepsilon_{it} \quad (6)$$

where subscripts *it* uniquely identify an observation for firm *i* in year *t*. ROA_{it} is defined as the ratio of operating income before depreciation and amortization (EBITDA) to the book value of assets for firm *i* in year *t*. EBITDA is calculated using data on Sales and Total/Operating Expenses from firm income statements. For many firms, interest expenses, deprecation, and taxes are either explicitly reported or not disclosed (and are thus potentially components of "Total/Operating Expenses" listed in the income statements). It is unclear whether this measurement error induces any systematic bias in coefficient estimates. It is likely that any measurement error increases the size of coefficient standard errors, causing coefficient estimates of *IPLaw* to understate the impact of investor protection laws on operating performance (in terms of statistical significance). All other variables are defined as in Specification 1.

Regression estimates are presented in Table 6. Columns 1-2 indicate that the coefficient for *IPLaw* is positive and statistically significant, at approximately 0.064. The coefficient for *Age* is statistically insignificant and economically trivial. In results not reported here, the estimated coefficient for *IPLaw* remains economically large and statistically significant when the specification is estimated without year fixed effects. Overall, the results imply that the passage of investor protection law causes firms to improve their operating performance by 7%.

4.3.2 Market Capitalization to Cash Flow Ratios

The impact of investor protection statutes on market valuations, $MarketValue_{it}$ / $CashFlow_{it}$, following LLSV (2002), is estimated using the following specification:

$$MarketValue_{it} / CashFlow_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(Firm_i) + \beta_4(Year_t) + \varepsilon_{it} \quad (7)$$

where subscripts *it* uniquely identify an observation for firm *i* in year *t*. $MarketValue_{it}$ is defined as the product of shares outstanding and stock price at the end of year *t* for firm *i*. $CashFlow_{it}$ is defined as the sum of sales, net debt and equity issuance, minus the sum of operating expenses, net capital expenditures, and dividend payments for firm *i* in year *t*.[17] All other variables are defined as in Specification 1. *Sales Growth* is not included in the regression because Sales are used to construct *Cash Flow* measures.

Regression estimates are presented in Table 6. Columns 3-4 indicate that the coefficient for *IPLaw* is positive and statistically significant, ranging between 12.6 and 14.6. These estimates indicate that improvements in investor protection law are associated with relative increases in market value to cash flows. The coefficient for *Age* in Column 4 is economically large and statistically significant (55.6), suggesting that older firms have higher ratios of market value to cash flows. This may appear counterintuitive, as younger firms typically have higher market valuations due to greater growth options than older firms. However, in the context of the mining industry during the early 20th century, it is possible that older firms would have greater ability to expand existing operations and properties in order to excavate new mineral deposits, thereby causing them to have higher market-to-book valuations.

The dependent variable captures the market's valuation of a firm's outstanding equity given its cash flows. As discussed in LLSV (2002), this is a useful measure to examine the market's valuation of cash flows and expropriation risk; the higher the risk of expropriation, the lower the market value of equity conditional on a given level of cash flows. The evidence presented in Table 6 is strongly supportive of theories which predict that greater investor protections will lead to improved market valuations through reduced risk of insider expropriation (LLSV 2002).

The findings in Table 6 indicate the increased economic activity of firms in the mining industry in response to the blue sky laws does not reflect negative NPV pursuits, such as wasteful mineral exploration or empire building. The evidence is strongly supportive of theories that predict greater investor protections allow firms to better access

[17] Capital expenditures are not explicitly reported in the data, they are approximated by calculating annual changes in net PPE. Changes in net working capital are not included because of data availability, however, casual inspection of the data suggests that changes in net working capital likely have a marginal impact on total cash flows.

positive NPV projects and improve performance (LLSV 2002, Shleifer and Wolfenzon 2002). The collective findings suggest that once the blue sky laws are passed, firms in the mining industry are better able to access external financing and pursue positive NPV projects, such as mineral excavations in fruitful tracts and quarries. The performance implications of the blue sky laws are not just immediate changes in realized profits (Table 6, Columns 1-2), but also changes in the market valuations of equity relative to changes in cash flows (Table 6, Columns 3-4).

4.4 Alternative Hypotheses

A number of alternative explanations for the findings in Tables 2-6 are explored in the following subsections. First, I evaluate the extent to which the blue sky laws may represent the political lobbying efforts of incumbent mining firms during the sample period. Second, I examine whether the passage of the blue sky laws appears to be driven by unobservable changes in investment opportunities, manifest in either pre-existing trends or immediate changes in corporate policy.[18] As discussed below, the findings do not appear to be driven by political economy considerations or unobservable changes in investment opportunities. Third, I test whether the observed impact of the blue sky laws occurs through the channel of reduced expropriation risk or through the channel of reduced adverse selection in security dealer markets. The evidence is strongly indicative of reduced expropriation risk.

4.4.1 Political Economy Hypotheses

A significant amount of anecdotal evidence and academic research points to the prevention of securities fraud as the primary motivation for the passage of the blue sky laws (Seligman 1983, Reed and Washburn 1921, Mulvey 1914). It is possible, however, that the blue sky laws were passed in response to lobbying pressure by industrial incumbents. As discussed by Rajan and Zingales (2003), many investor protection laws could simply result from incumbent industrial firms who wish to reduce competition from potential market entrants by raising the costs of external financing, similar to the Macey

[18] The time trends analysis is also relevant for testing various political economy hypotheses as well as any hypotheses that would suggest reverse causality.

and Miller (1991) hypothesis. However, in contrast to the likelihood that banks' lobbying power for the blue sky laws would cause coefficient estimates of *IPLaw* to be understated (Macey and Miller 1991), lobbying power by industrial incumbents could cause coefficient estimates of *IPLaw* to be overstated. Such lobbying would imply that the passage of the blue sky laws could presumably increase the market power of incumbent firms by limiting entry by competitors. As a result, existing firms would find it easier to issue equity, grow in size, and increase profits due to improved monopoly power in product markets.[19]

To test this hypothesis, I collect data on the quantities and prices of metal ores produced by firms in the early 1900's. Because metal ores are homogenous products with little differentiation in characteristics, one can easily compare the prices and quantities of metal ores across different regions. If the blue sky laws were encouraged by industrial incumbents, one would expect to see incumbent firms having increased monopoly power in product markets. In a canonical model of imperfect product market competition, increased market power should lead to a decrease in the quantities of goods sold and an increase in prices, particularly by the types of well-established firms observed in this sample.

To measure the impact of the blue sky laws on the quantities and prices of ores sold by mining firms, I estimate the following baseline specification:

$$Ln(Quantity_{it})\ [OR]\ Ln(Price_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it} \qquad (8)$$

where subscripts *it* uniquely identify an observation for firm *i* in year *t*. $Ln(Quantity_{it})$ is defined as the log of ore quantities (tons or ounces) produced, while $Ln(Price_{it})$ is the log of ore prices (per ton or ounce) sold by firm *i* in year *t*. All other variables are defined as in Specification 1.

The results are presented in Table 7. Columns 1-3 present estimates of the impact of the laws on the quantities of Copper, Silver, and Gold ores, while Columns 4-6 present the impact of the laws on the prices of Copper, Silver, and Gold ores. The results on ore quantities indicate that the blue sky laws are associated with increases in quantities

[19] It is not clear, however, how improved market power would influence dividend payments.

of ores produced. The coefficient estimates of 0.43 for *IPLaw* in Column 2 and 0.66 for *IPLaw* in Column 3 are economically large. The results imply that the passage of the blue sky laws cause mean quantities of silver sold to increase by approximately 55%, while gold quantities almost double (94% increase). The magnitudes are extremely large, yet somewhat imprecise, particularly in the case of gold production. The impact on copper production is statistically insignificant, but the positive coefficient implies that copper production does not decrease significantly in response to the blue sky laws. Columns 4-6 illustrate that estimates of the laws' impact on ore prices are statistically insignificant. The measured coefficient on copper and gold prices is economically trivial, while the price effect for silver is qualitatively negative.

Overall, the laws appear to have a positive effect on the quantities of ores sold and an insignificant or negative impact on the prices of mineral ores; product markets appear to become more competitive after the investor protection laws are passed. The results contradict the hypothesis that the blue sky laws reflect increases in industrial product market power, which would cause a decrease in quantities and increase in prices of goods sold. The statistical estimates corroborate anecdotal evidence that suggests political lobbying by incumbent mining firms did not play a significant role in promoting the passage of the blue sky laws.

4.4.2 Endogenous Timing of Regulatory Reform

This section presents estimates of the average changes in corporate policy and performance during several time periods around the passage of the blue sky laws (time trends analysis). The estimates are useful for evaluating the possibility that the investor protection statutes were passed either in response to or in conjunction with unobservable changes in investment opportunities in states that passed laws during the sample period. Examples of such changes in the mining industry could consist of growth in product demand, improvements in excavation technologies, new discoveries of mineral ore deposits, etc. If the laws were passed at the same time that investment opportunities improved in sample treatment states, then the regression estimates of Specifications 2-7

in Tables 2-6 would overstate the impact of the blue sky laws on corporate policy and performance.[20]

First, I repeat the analysis of Tables 2-6 by broadening the specifications estimated in each table. The new specification is the following OLS linear model:

$$\begin{aligned} \textit{Dependent Var.} = {} & \alpha + \beta_1(IPLaw_{it}\_Before_{it}(-2)) + \beta_2(IPLaw_{it}\_Before_{it}(-1)) + \\ & \beta_3(IPLaw_{it}\_After_{it}(0)) + \beta_4(IPLaw_{it}\_After_{it}(+1)) + \beta_5(IPLaw_{it}\_After_{it}(+2)) \\ & + \beta_7(Firm_i)) + \beta_8(Year_t) + \varepsilon_{it} \end{aligned} \qquad (9)$$

where subscripts *it* uniquely identify an observation for firm *i* in year *t*. $IPLaw_{it}\_Before_{it}(-2, -1)$ is an indicator for whether the observation for firm *i* in year *t* takes place (2 years, 1 year) before an investor protection law is passed in the state of incorporation for firm *i*. $IPLaw_{it}\_After_{it}(0, 1, 2+)$ is an indicator for whether the observation for firm *i* in year *t* takes place (0 years, 1 year, 2 or more years) after an investor protection law is passed in the state of incorporation for firm *i*. All other variables are defined as in Specification 1.

If there are underlying trends in the corporate policies of firms before the laws are passed, then the coefficients of the terms $IPLaw_{it}\_Before(-2)$ and *IPLaw_Before(-1)* should be positive and significant. However, as depicted in Table 9, the data contradict this hypothesis. Column 1 contains estimates of the coefficients when *Dividends/Sales* is the dependent variable in Specification 9. The coefficients for $IPLaw_{it}\_Before(-2)$ and *IPLaw_Before(-1)* are both economically small and statistically insignificant. The point estimate of *IPLaw_Before(-1)* is actually negative (-0.015). In contrast, the coefficients for $IPLaw_{it}\_After_{it}(0)$ and $IPLaw_{it}\_After_{it}(1)$ are both economically large and statistically significant; they are approximately 0.090 each. The coefficient for $IPLaw_{it}\_After_{it}(2+)$ is also economically large and positive, but statistically insignificant. The results imply that mean dividends-to-sales ratios increased in the years immediately following the passage of the blue sky laws. The magnitude of the increase in each year mirrors the total estimated impact of the laws on dividends/sales presented in Table 2. Additionally, the small size of the pre-law time period coefficients indicate that dividend payouts were not

[20] It is worth noting that sales growth, a commonly used proxy for investment opportunities (LLSV 2002), does not change significantly around the passage of investor protection laws.

already changing significantly prior to the passage of the blue sky laws. The estimates strongly support the causal interpretation of the results presented in Tables 2-8.

Across all other specifications in Table 9, the coefficients for *IPLaw_Before(-2)* and *IPLaw_Before(-1)* are statistically insignificant, while the coefficients for *IPLaw_After(0)*, *IPLaw_After(1)*, and *IPLaw_After(2+)* are positive, statistically significant, and economically large (both in absolute terms as well as relative to the coefficients for pre-period trends). The findings strongly support the identifying assumption that the passage of the blue sky laws was not endogenous with changes in unobservable investment opportunities or other pre-existing trends in corporate policy.

To further explore the possibility that the blue sky laws were passed as a result of changes in investment opportunities, a second set of tests is conducted. This set of analysis addresses the possibility that investment opportunities or other unobservable drivers of corporate policy in blue sky states changed immediately at the same times that the laws were passed in various states.[21] To explore the relevance of this scenario, I re-estimate the time-trends model (Specification 9) on a subsample of data. Specifically, I keep all firms located in states that do not pass blue sky laws during the sample period, and all firms incorporated in Michigan and West Virginia. As discussed in Section 2, the blue sky statutes in Michigan and West Virginia were initially passed in 1913, only to be declared unconstitutional by federal courts shortly thereafter (Reed and Washburn 1921). Both states subsequently revised their laws and passed modified blue sky statutes in 1915. If the laws were proposed and passed at the same that there were immediate changes in unobservable investment opportunities, then the impact of the laws should be observed starting in 1913, rather than 1915, for firms in Michigan and West Virginia.

Table 10 presents regression estimates of Specification 9 on this subsample of data. Across all specifications, the coefficients for *IPLaw_Before(-2)* and *IPLaw_Before(-1)* are negative or statistically insignificant. The lone exception is the coefficient for *IPLaw_Before(-2)* in the Dividends/Sales analysis, which is only marginally significant and likely inconsequential in light of the negative coefficient for

[21] This second set of analysis also provides a different means of evaluating the more general possibility of time trends in corporate policy prior to the laws being passed. For example, pre-period point estimates in Table 9, while economically small and statistically insignificant, do appear larger in magnitude for the year before the law than 2 years before the law.

IPLaw_Before(-1)). If the laws were coincidental with immediate changes in investment opportunities, then the coefficients for *IPLaw_Before(-2)* and *IPLaw_Before(-1)* should be positive and statistically significant. The findings in Table 10 strongly reject this hypothesis, thus further supporting the causal interpretation of the results in Tables 2-8.

4.4.3 Adverse Selection vs. Reduced Expropriation Risk

The blue sky laws provided investors with a legal basis for recovering damages from firms that were deemed to be fraudulent following public security issuances (Reed and Washburn 1921, Virginia Law Review 1937). In addition, many laws also required that securities be registered with the government prior to any public issuance. It is therefore possible that the impact of the blue sky laws estimated in Tables 2-6 is manifest through the channel of reduced adverse selection at the pre-clearance stage rather than through the reduced risk of expropriation by insiders conditional on securities already being issued.[22]

To evaluate this possibility, I repeat the analysis of Tables 2-6 on a selected set of firms. Specifically, I keep all control firms (i.e. all firms located in states that do not pass blue sky laws during the sample period as well as any pre-blue sky law observations for firms in states which pass the laws during the sample period), and treatment firms incorporated in Maine. The blue sky provisions in Maine were unique from the blue sky provisions in other states which passed laws during the sample period because Maine did not require pre-clearance of offerings, but did prohibit ex-post fraud (Mahoney 2003). If the channel through which the laws affected corporate policy is the reduction adverse selection at the pre-clearance stage rather than ex-post reduction in expropriation risk, then the impact of the laws in Maine should be statistically insignificant.

I estimate Specifications 2-5 using this subsample of firms. The results are presented in Table 8. Across all columns, the estimated coefficient of *IPLaw* is economically large and statistically significant. The impact of the Maine blue sky law on dividend payouts, equity outstanding, firm size, and performance quantitatively and qualitatively mirrors the estimated average impact of all blue sky laws on the full sample of firms. The findings are strongly supportive of the mechanism that the impact of the

[22] These hypotheses are not mutual exclusive; it is possible that reduced expropriation risk was manifest by higher quality securities being offered to the government for pre-clearance.

blue sky laws is through reduced risk of expropriation by insiders rather than reduced adverse selection in securities markets due to security pre-clearance requirements.

Conclusion

The findings in this paper address recent debate concerning the theoretical and empirical relevance of investor protection laws. While some research argues that legal protection of investors from insider expropriation is critical to a well designed system of corporate governance, others argue that investor protection laws have limited importance. This paper attempts to improve upon existing empirical strategies which rely primarily on either cross-country studies that utilize legal origin as a key source of variation in investor protection law, or identification strategies which rely primarily on time-series variation in country-wide regulatory reform.

Holding legal origin fixed, and exploiting both cross sectional and time series exogenous variation in the passage of investor protection laws across U.S. states in the early 20th century, I find that firms respond to the introduction of investor protection statutes by raising equity, increasing dividend payouts, and growing in size. The laws are also associated with improvements in accounting performance and market valuations. The evidence is strongly supportive of theories which predict that investor protection laws have a significant impact on corporate investment and financing policy. Additional analysis suggests that alternative explanations for the findings have limited support in the data. In particular, the patterns in investment and financing do not appear to be biased by political economy considerations, reduced adverse selection in securities dealers markets, or endogenous timing of the laws with changes in investment opportunities.

The results in this paper point to several avenues for additional research. First, it would be interesting to explore the additional implications of investor protection laws on the functioning of securities markets. Recent work by Wang (2008) has begun to explore this line of inquiry. A second avenue of research would be to more deeply explore the mechanisms underlying the enforcement of investor protection laws and identifying the extent to which investor protection laws are compliments or substitutes for other aspects of capital markets reform.

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Table 1

Sample Descriptive Statistics

This table presents descriptive statistics of sample firm characteristics. Panel A indicates the sample contains 108 firms in mining industries with observations taken across sample years 1899-1918 for a total of 887 firm-year observations. Mean age of firm refers to the average age of a firm in a typical firm-year observation. Panel B contains means and standard deviations (in parenthesis) of firm characteristics in 1911, separated by firms in states which pass laws during the sample period (<=1918) and firms in states which pass the laws after the sample period (>1918). Sales Growth is the percentage change in sales in 1911 relative to 1910. Assets and PPE (Plant, Property, and Equipment) are given in book values. Market Capitalization is number of shares outstanding times share price as of end of December, 1911. Dividend/Sales is the ratio of dividends on common stock to sales. ROA is the return on assets; it is defined as operating earnings divided by book value of assets. P-values refer to t-tests on firm characteristics between the two groups of sample firms. Panel C lists sample states and the years in which they pass blue sky laws.

Panel A: Sample Characteristics

Total Number of Firms	108
Sample Years	1899-1918
Total Number of Firm-Year Observations	887
Mean year of Incorporation	1899
Mean age (years)	13

Panel B: Sample Firm Characteristics

Year = 1911	Law Passed > 1918	Law Passed <= 1918	p-value
Sales	2,423,439	2,501,990	0.95
	(3,171,208)	(5,969,495)	
Sales Growth	0.28	0.45	0.67
	(1.44)	(1.42)	
Age	11.00	13.58	0.34
	(6.05)	(13.57)	
PPE	6,868,768	7,189,511	0.92
	(6,075,688)	(14,600,000)	
Assets	9,248,538	9,337,267	0.98
	(10,100,000)	(19,100,000)	
Market	30,600,000	11,100,000	0.27
Capitalization	(104,000,000)	(26,800,000)	
Dividends/Sales	0.29	0.20	0.28
	(0.31)	(0.26)	
ROA	0.15	0.08	0.52
	(0.18)	(0.40)	

Table 1 (continued):

Panel C: Sample States	
State	Year of Law Passage
Arizona	1912
Alabama	1919
California	1913
Canada	----
Colorado	1923
Delaware	1931
Maine	1913
Michigan	1915
Minnesota	1917
Missouri	1913
Montana	1913
New Jersey	1920
Nevada	----
New York	1921
Pennsylvania	1923
Texas	1913
Virginia	1916
West Virginia	1915
Wyoming	1919

Table 2:

Estimated Effects of Investor Protection Laws on Payout Policy

This table presents estimates of the effects of state investor protection laws on firm dividend payments. The baseline specification is an OLS linear model:

$$Dividend_{it} / Sales_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(Firm_i) + \beta_4(Year_t) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Dividend_{it} / Sales_{it}$ is the ratio of dividend payments on common stock to sales of firm *i* in year *t* (Columns 1-3). In Columns 4-5, $Dividend_{it} / Sales_{it}$ is the ratio of dividend payments on common stock plus dividends on preferred stock to sales of firm *i* in year *t*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an Investor Protection (IP) Law by year *t*. $Ln(Age)_{it}$ is the log of the age of firm *i* in year *t*. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variable: Dividend/Sales				
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.026*	0.093***	0.098***	0.101***	0.105***
	(0.014)	(0.025)	(0.030)	(0.025)	(0.030)
Age			0.114***		0.086
			(0.037)		(0.053)
Constant	0.240***	0.359**	0.176	0.369**	0.233
	(0.003)	(0.123)	(0.147)	(0.126)	(0.166)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.003	0.075	0.089	0.078	0.086
No. of firms	78	78	76	78	76
No. of obs.	539	539	533	539	533

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 3:

Estimated Effects of Investor Protection Laws on Equity Issuance

This table presents estimates of the effects of state investor protection laws on firm common stock outstanding. The baseline specification is an OLS linear model:

$$Ln(Shares_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(Shares)_{it}$ is the log of the number of outstanding shares of common stock of firm *i* in year *t*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an Investor Protection (IP) Law by year *t*. $SalesGrowth_{it}$ is the percentage change in sales of firm *i* from year *t-1* to year *t*. $Ln(Age)_{it}$ is the log of the age of firm *i* in year *t*. $Firm_t$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variable: Ln(Shares Outstanding)				
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.130***	0.129***	0.112***	0.114***	0.115***
	(0.017)	(0.032)	(0.031)	(0.035)	(0.036)
Age			-0.009		0.014
			(0.023)		(0.022)
Sales Growth				0.001	0.001
				(0.003)	(0.003)
Constant	12.574***	12.585***	12.553***	12.507***	12.478***
	(0.004)	(0.017)	(0.017)	(0.013)	(0.039)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.033	0.045	0.047	0.093	0.093
No. of firms	91	91	88	76	75
No. of obs.	705	705	683	482	479

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 4:

Estimated Effects of Investor Protection Laws on Firm Size (PPE)

This table presents estimates of the effects of state investor protection laws on firm plant, property, and equipment. The baseline specification is an OLS linear model:

$$Ln(PPE_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(PPE)_{it}$ is the log of Plant, Property, and Equipment of firm *i* in year *t*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an Investor Protection (IP) Law by year *t*. $SalesGrowth_{it}$ is the percentage change in sales of firm *i* from year *t-1* to year *t*. $Ln(Age)_{it}$ is the log of the age of firm *i* in year *t*. $Firm_t$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variable: Ln(PPE)				
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.394***	0.317**	0.327**	0.384***	0.383***
	(0.134)	(0.135)	(0.135)	(0.116)	(0.118)
Age			-0.094		0.115
			(0.149)		(0.303)
Sales Growth				0.004**	0.005**
				(0.002)	(0.002)
Constant	14.543***	14.559***	14.793***	14.338***	14.328***
	(0.039)	(0.069)	(0.402)	(0.252)	(0.840)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.109	0.148	0.148	0.178	0.179
No. of firms	95	95	93	83	82
No. of obs.	603	603	590	437	435

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 5:

Estimated Effects of Investor Protection Laws on Firm Size (Assets)

This table presents estimates of the effects of state investor protection laws on firm size. The baseline specification is an OLS linear model:

$$Ln(Assets_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(SalesGrowth_{it})) + \beta_4(Firm_i) + \beta_5(Year_t) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(Assets)_{it}$ is the log of the book value of assets of firm *i* in year *t*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an Investor Protection (IP) Law by year *t*. $SalesGrowth_{it}$ is the percentage change in sales of firm *i* from year *t-1* to year *t*. $Ln(Age)_{it}$ is the log of the age of firm *i* in year *t*. $Firm_t$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variable: Ln(Assets)				
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.347***	0.168*	0.163*	0.234**	0.233**
	(0.101)	(0.090)	(0.092)	(0.084)	(0.082)
Age			0.03		-0.027
			(0.065)		(0.120)
Sales Growth				0.005***	0.005***
				(0.001)	(0.002)
Constant	14.939***	15.283***	15.219***	14.900***	15.237***
	(0.030)	(0.025)	(0.114)	(0.128)	(0.360)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.124	0.207	0.206	0.294	0.294
No. of firms	99	99	97	87	86
No. of obs.	679	679	666	490	488

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 6:

Estimated Effects of Investor Protection Laws on Performance

This table presents estimates of the effects of state investor protection laws on firm accounting performance (ROA) and market valuations (market capitalization to cash flow). The baseline specification is an OLS linear model:

ROA_{it} [or] $MarketCap_{it}/CashFlow_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(ln(Age_{it})) + \beta_3(Firm_i) + \beta_4(Year_t) + \varepsilon_{it}$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. ROA_{it} is the ratio of operating income before depreciation and amortization (EBITDA) to the book value of assets of firm *i* in year *t* (Columns 1-2). $MarketCap_{it} / CashFlow_{it}$ is the ratio of market capitalization to cash flow of firm *i* in year *t* (Columns 3-4). $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an Investor Protection (IP) Law by year *t*. $Ln(Age)_{it}$ is the log of the age of firm *i* in year *t*. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. Regression results are presented for sample data winsorized at the 5th and 95th percentiles for *ROA* and *MarketCap/CashFlow*, separately. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dep. Var.: Return on Assets		Dep. Var: Market Cap to Cash Flow	
	(1)	(2)	(3)	(4)
IPLaw	0.064**	0.064**	14.564**	12.638**
	(0.030)	(0.029)	(5.716)	(4.937)
Age		-0.004		55.595***
		(0.090)		(17.848)
Constant	0.374***	0.383*	3.645	-121.685***
	(0.011)	(0.200)	(2.118)	(30.068)
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
R^2	0.175	0.174	0.041	0.065
No. of firms	88	86	66	65
No. of obs.	473	470	270	269

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 7:

Estimated Effects of Investor Protection Laws on Product Markets

This table presents estimates of the effects of state investor protection laws on quantities and prices of metal ores produced by mining firms. The baseline specification is an OLS linear model:

$Ln(Quantity_{it})$ [or] $Ln(Price_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it}$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(Quantity_{it})$ is the log of quantity (tons or ounces) of metal ores produced by firm *i* in year *t* (Columns 1-3). $Ln(Price_{it})$ is the log of price of ores produced by firm *i* in year *t* (Columns 4-6). $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. Each column in the table refers to a distinct ore: copper, silver, or gold. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Var.: Ln(Quantity)			Dependent Var.: Ln(Price)		
	Copper	Silver	Gold	Copper	Silver	Gold
	(1)	(2)	(3)	(4)	(5)	(6)
IPLaw	0.098	0.436**	0.661*	0.002	-0.031	0.001
	(0.159)	(0.180)	(0.324)	(0.016)	(0.044)	(0.218)
Constant	15.634***	13.138***	9.218***	2.431***	2.570***	2.612***
	(0.073)	(0.335)	(0.143)	(0.001)	(0.042)	(0.179)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.110	0.166	0.125	0.905	0.251	0.097
No. of firms	66	44	38	31	17	13
No. of obs.	498	281	259	176	80	79

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 8:

Analysis of Adverse Selection vs. Insider Expropriation Hypotheses

This table presents estimates of the effects of state investor protection laws on the dependent variables examined in Tables 2 through 6. The sample is restricted to firms located in Maine (subject to "ex-post" investor protection laws) and firms incorporated in states which have not passed a blue sky law in year *t (*all sample control firms). The baseline specification is an OLS linear model:

$$Dependent\ Var. = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. The dependent variables are $Dividends_{it}/Sales_{it}$, $Ln(Shares\ Oustanding_{it})$, $Ln(PPE_{it})$, $Ln(Assets_{it})$, ROA_{it}, and $Market\ Cap_{it}/Cash\ Flow_{it}$ for firm *i* in year *t* (Columns 1-6). $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variables:					
	Dividends/Sales	Ln(Shares Oustanding)	Ln(PPE)	Ln(Assets)	ROA	Market Cap/Cash Flow
	(1)	(2)	(3)	(4)	(5)	(6)
IPLaw	0.061***	0.110***	0.203**	0.110**	0.064***	24.746**
	(0.019)	(0.030)	(0.077)	(0.048)	(0.017)	(8.147)
Constant	0.352**	12.650***	14.609***	15.072***	0.303***	1.562
	(0.132)	(0.012)	(0.031)	(0.020)	(0.075)	(3.078)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.090	0.026	0.079	0.113	0.185	0.061
No. of firms	73	87	88	92	79	58
No. of obs.	466	603	515	566	386	222

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 9:

Analysis of Time Trends in Regression Estimates

This table presents estimates of the effects of state investor protection laws on the dependent variables examined in Tables 2 through 6, split into sub-periods before and after the passage of the laws. The baseline specification is an OLS linear model:

$$\textit{Dependent Var.} = \alpha + \beta_1(IPLaw_{it}\_Before_{it}(-2)) + \beta_2(IPLaw_{it}\_Before_{it}(-1)) + \beta_3(IPLaw_{it}\_After_{it}(0)) + \beta_4(IPLaw_{it}\_After_{it}(1)) + \beta_5(IPLaw_{it}\_After_{it}(2+)) + \beta_6(Firm_i) + \beta_7(Year_i) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. The dependent variables are $Dividends_{it}/Sales_{it}$, $Ln(Shares\ Oustanding_{it})$, $Ln(PPE_{it})$, $Ln(Assets_{it})$, ROA_{it}, and $Market\ Cap_{it}/Cash\ Flow_{it}$ for firm *i* in year *t* (Columns 1-6). $IPLaw_{it}\_Before_{it}(-2, -1)$ is an indicator for whether the observation for firm *i* in year *t* takes place (2 years, 1 year) before an investor protection law is passed in the state of incorporation for firm *i*. $IPLaw_{it}\_After_{it}(0, 1, 2+)$ is an indicator for whether the observation for firm *i* in year *t* takes place (0 years, 1 year, 2 or more) years after an investor protection law is passed in the state of incorporation for firm *i*. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variables:					
	Dividends/Sales	Ln(Shares Oustanding)	Ln(PPE)	Ln(Assets)	ROA	Market Cap/Cash Flow
	(1)	(2)	(3)	(4)	(5)	(6)
IPLaw_Before(-2)	0.009	-0.010	0.048	-0.054	-0.016	6.366
	(0.031)	(0.034)	(0.079)	(0.057)	(0.014)	(8.653)
IPLaw_Before(-1)	-0.015	0.028	0.120	0.036	-0.001	12.580
	(0.025)	(0.034)	(0.105)	(0.048)	(0.017)	(10.343)
IPLaw_After(0)	0.090***	0.111**	0.330*	0.164*	0.045	20.129
	(0.025)	(0.046)	(0.169)	(0.092)	(0.029)	(11.864)
IPLaw_After(1)	0.087*	0.149**	0.429*	0.157	0.073	25.956*
	(0.041)	(0.063)	(0.232)	(0.111)	(0.043)	(12.680)
IPLaw_After(2+)	0.098	0.186***	0.492*	0.173	0.061**	35.437**
	(0.067)	(0.056)	(0.263)	(0.130)	(0.028)	(13.460)
Constant	0.359**	12.585***	14.414***	15.005***	0.373***	0.582
	(0.123)	(0.019)	(0.175)	(0.111)	(0.011)	(2.120)

Table 9, continued:

Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.076	0.048	0.070	0.097	0.074	0.023
No. of firms	78	91	95	99	88	66
No. of obs.	539	705	603	679	473	270

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively

Table 10:

Analysis of Michigan and West Virginia Firms

This table presents estimates of the effects of state investor protection laws on the dependent variables examined in Tables 2 through 6. The sample is restricted to firms located in Michigan and West Virginia (where initial investor protection laws passed in 1913 were declared unconstitutional by federal courts; revised IP Laws were passed in 1915) and firms located in states that do not pass investor protection laws during the sample period. The baseline specification is an OLS linear model:

$$\text{Dependent Var.} = \alpha + \beta_1(IPLaw_{it}\_Before_{it}(-2)) + \beta_2(IPLaw_{it}\_Before_{it}(-1)) + \beta_3(IPLaw_{it}\_After_{it}(0)) + \beta_4(IPLaw_{it}\_After_{it}(1)) + \beta_5(IPLaw_{it}\_After_{it}(2+)) + \beta_6(Firm_i) + \beta_7(Year_i) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. The dependent variables are $Dividends_{it}/Sales_{it}$, $Ln(Shares\ Oustanding_{it})$, $Ln(PPE_{it})$, $Ln(Assets_{it})$, ROA_{it}, and $Market\ Cap_{it}/Cash\ Flow_{it}$ for firm *i* in year *t* (Columns 1-6). $IPLaw_{it}\_Before_{it}(-2, -1)$ is an indicator for whether the observation for firm *i* in year *t* takes place (2 years, 1 year) before an investor protection law is passed in the state of incorporation for firm *i*. $IPLaw_{it}\_After_{it}(0, 1, 2+)$ is an indicator for whether the observation for firm *i* in year *t* takes place (0 years, 1 year, 2 or more) years after an investor protection law is passed in the state of incorporation for firm *i*. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

	Dependent Variables:					
	Dividends/Sales	Ln(Shares Oustanding)	Ln(PPE)	Ln(Assets)	ROA	Market Cap/Cash Flow
	(1)	(2)	(3)	(4)	(5)	(6)
IPLaw_Before(-2)	0.060*	-0.023	-0.059	-0.200**	-0.021*	0.063
	(0.031)	(0.021)	(0.113)	(0.071)	(0.010)	(11.456)
IPLaw_Before(-1)	-0.073	-0.036	0.059	-0.035	-0.007	-2.967
	(0.050)	(0.023)	(0.120)	(0.0620)	(0.017)	(21.177)
IPLaw_After(0)	0.072*	0.060**	0.401**	0.207**	0.066**	14.104
	(0.031)	(0.026)	(0.167)	(0.085)	(0.025)	(10.894)
IPLaw_After(1)	0.126**	0.186***	0.826**	0.320**	0.205***	16.669***
	(0.042)	(0.027)	(0.324)	(0.115)	(0.054)	(4.106)
IPLaw_After(2+)	0.327**	0.189***	0.953	0.179	0.063	30.784**
	(0.120)	(0.042)	(0.603)	(0.243)	(0.037)	(11.611)
Constant	0.204***	12.427***	13.812***	14.505***	0.101***	-15.378
	(0.037)	(0.007)	(0.160)	(0.080)	(0.028)	(11.680)

Table 10, continued:

Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.138	0.039	0.175	0.195	0.172	0.081
No. of firms	50	63	61	63	55	42
No. of obs.	361	499	363	433	306	180

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively