The impact of investor protection law on corporate policy and performance: evidence from the blue sky laws

The Impact of Investor Protection Law on Corporate Policy and Performance: Evidence from the Blue Sky Laws

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Abstract:

Recent studies have debated the impact of investor protection law on corporate behavior and value. I exploit the staggered passage of state securities fraud statutes ("blue sky laws") in the United States to estimate the causal effects of investor protection law on firm financing decisions and investment activity. The statutes induce firms to increase dividends, issue equity, and grow in size. The laws also facilitate improvements in operating performance and market valuations. Overall, the evidence is strongly supportive of theoretical models that predict investor protection law has a significant impact on corporate policy and performance.

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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 that has received significant attention is the legal protection of investors from managerial expropriation. Both the theoretical and the empirical literature reach mixed conclusions on the impact of investor protection on firm decisions. On one hand, a number of theories predict that investor safeguards have a significant impact on corporate policies. These theories are supported by empirical studies that 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, 2002). On the other hand, some scholars 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). Moreover, various studies examine time-series variation in investor protection laws within countries such as the U.K. and Italy and find little impact of the laws 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 corporate policy requires a setting in which legal protections are well-defined and exhibit variation that is independent of factors otherwise correlated with firms' decisions. I exploit the staggered passage of state investor protection statutes, also known as the blue sky laws, in the U.S. during the early 1900's to identify the causal impact of investor protection law on firms in the mining industry. I compare the impact of the blue sky laws on companies in states which introduce statutes during the sample period to contemporaneous changes in companies located in other states. I also evaluate differences in the operating performance and market valuations among firms within these two groups.

This paper addresses several critical limitations of previous empirical studies on the impact of investor protection law on corporate policy and performance. First, it focuses on cross-sectional and time-series variation in investor protections within a single common law country; legal origin is fixed across states. Second, I evaluate the impact of statutes 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 promulgated by the laws are arguably more closely linked to the theories developed by LLSV (1997) than measures of investor protections used in many other studies (LLS 2008, Coffee 2001). Third, I exploit exogenous variation in investor protection laws that is likely independent of unobservable variables which otherwise impact firm financing and investment decisions (an identification assumption explored in greater detail below). Examples of such factors are political lobbying efforts, changes in investment opportunities, and unobserved economic and financial shocks.

Regression estimates indicate that the blue sky laws have significantly positive effects on corporate policy and performance. The statutes cause firms to increase their dividend payouts to shareholders by approximately 6.2% to 13%. In the early 1900's, investors in mining companies frequently looked to stock investments as dependable sources of dividend income but were often falsely promised extraordinary returns during security issuance. The blue sky laws appear to have given investors a legal fulcrum to exert pressure on firms to disgorge excess cash that could otherwise be expropriated by insiders. This finding is consistent with the "outcome model" of dividends and investor protections (LLSV 2000).

However, while firms pay out larger dividends to investors, they are also able to raise greater financing from capital markets, ostensibly because investors are less concerned about managers absconding issuance proceeds. Treated mining firms increase equity outstanding by 10% to 13.7% after the introduction of investor protection laws. In turn, sample companies use this capital to invest in PPE and grow in size; total levels of physical capital increase by at least 29% while firm assets increase by at least 19%. These results are not surprising given the nature of mining operations. The vast majority of assets in mining firms are devoted to Plant, Property, and Equipment (in particular, property), as the primary function of these firms is to extract minerals and metal ores from the land that they own. By raising additional capital from equity markets, mining companies are able to quickly procure tracts suitable for exploration and excavation.

To verify that these changes in corporate policy are profitable, I also examine the impact of the blue sky laws on operating margins and market valuations. Estimates illustrate that the laws on average lead to 6% growth in ROA, though this increase is only

realized two years after the laws are passed. The delay in profit realization likely stems from technological constraints on mining production during the sample period; it takes time for ores to be discovered, excavated, and sold. In contrast, market valuations rise almost immediately by at least 21.7%. This finding reflects the market's positive anticipation of subsequent dividend increases resulting from improved investor protection.

The results are consistent with anecdotal evidence that the blue sky laws had an immediate impact on fraudulent issuers and quickly made capital widely available to legitimate enterprises (Nation's Business 1917). More generally, the findings are strongly supportive of theoretical models that link investor protection law to various corporate policies. These models predict that stronger safeguards lead to greater interaction between firms and capital markets, increased investment, and improvements in overall value (LLSV 1997, 1998, LLS 2008, Shleifer and Wolfenzon 2002, Fulghieri and Suominen 2010).

The causal interpretation of the evidence relies on the identifying assumption that the passage of the blue sky laws is independent of unobserved factors that otherwise impact corporate policy. I support this assumption by citing numerous anecdotes in the academic literature and business press that describe the legislative environment in the early 1900's. As I describe in detail in Section 2, the motivation for introducing the laws is largely attributed to widespread securities fraud at the turn of the 20th century (Seligman 1983, Reed and Washburn 1921, Mulvey 1914). Firms and securities salesmen often took advantage of "naïve" investors by selling them securities backed by little more than a promise of high returns (Nation's Business 1917, 1922). The prevailing view of the blue sky laws is that the legislation aimed to reduce insider expropriation of investment proceeds that was commonplace after issuance. The empirical strategy thus provides an ideal setting for estimating the impact of investor protection law on corporate policy.

To further justify causation, I also perform a variety of analyses to evaluate alternative explanations for the findings, each of which effectively amounts to a violation of the identification assumption. First, I examine whether the blue sky laws were introduced in response to or in conjunction with unobservable changes in investment

opportunities or other factors related to corporate decisions. I find that there are no significant pre-existing trends in firm policy and performance prior to the passage of the laws. The estimated impact of the laws is significant only after the adoption of the laws. 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 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 introduced in 1913. The evidence suggests that regression estimates do not suffer from biases resulting from unobserved changes in factors that are correlated with corporate decision making.

Second, I examine the political economy of the blue sky laws and evaluate whether the laws appear to be driven by private lobbying interests. One alternative hypothesis, postulated by Macey and Miller (1991), is that the laws were passed at the behest of banks that competed with industrial firms for investor capital. They theorize that firms' increased costs of regulatory compliance would get passed onto investors, who would then redirect their capital from equity markets into banks. This theory has little explanatory power, however, because it would imply that the blue sky laws should have deleterious effects on corporate policy, rather than the positive effects documented in this paper.

Another alternative hypothesis is that the blue sky laws were a manifestation of political lobbying efforts by incumbent mining firms. While this supposition has not been mentioned in the existing literature, it is possible that increased costs of legal compliance reduced competition and entry by potential mining startups, allowing incumbent firms to more easily raise capital and grow in size. To test this theory, I analyze product market data on output quantities, prices, new firm incorporations, and capital stock authorization. I find that the blue sky laws are associated with increased output, similar or lower product prices, greater firm creation, and larger capital issuance. In contrast to this hypothesis, product markets do not appear to be less competitive after the passage of the blue sky laws, which suggests that the statutes were not simply the outcomes of industrial lobbying efforts.

Finally, I test whether the observed impact of the blue sky laws occurs through the channel of reduced expropriation risk or through the channel of securities pre-clearance by the government. The statutes 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 be registered with the government prior to any public offering. It is therefore possible that the impact of the blue sky laws is manifest through the reduction of adverse selection during securities pre-clearance rather than through the reduction of ex-post expropriation. I distinguish these channels by analyzing firms in Maine; Maine's law did not require security screening but did prohibit fraud after issuance. The findings indicate that the law in Maine has large effects that mirror the impact of the laws for the full sample of firms. Overall, the results suggest that the salient feature of the blue sky laws is the prohibition of investor expropriation by corporate insiders.

This paper makes several contributions. First, this study utilizes a new dataset and methodology to address debate regarding the impact of investor protection law on corporate policy and performance.[1] The findings also help distinguish among various theories that make disparate predictions regarding the links between investor protection law and corporate policies such as dividend payouts. The results are strongly supportive of the theoretical models developed by LLSV (1997, 1998) and others. Second, this paper contributes to recent policy discussion over the development of financial markets in emerging countries by highlighting the potential outcomes of governance reforms (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.

[1] Papers in this area include Carlin and Gervais (2010), Demirguc-Kunt and Maksimovic (1998), Fisman and Love (2004), Foley and Greenwood (2010), Glaeser, Johnson, and Shleifer (2001), Guiso, Sapienza, and Zingales (2004, 2008), 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.

Section 2: Institutional Background

The blue sky laws are securities fraud statutes that were passed by various states between 1911 and 1931. The motivation for the laws was to prevent investor expropriation by insiders, such as managers and securities dealers. The laws required that security issuers and dealers register with state governments prior to issuing public securities, and mandated that they had to receive approval from the government before selling any securities in the state (Mahoney 2003). Perhaps most importantly, the laws 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 securities pre-clearance prior to any public offerings and prohibited ex-post fraud. Maine was the lone exception, as its statute did not require securities pre-clearance; it only prohibited ex-post fraud (Mahoney 2003).

The motivation for passing the blue sky laws is largely attributed to rampant securities fraud at the turn of the 20th century (Seligman 1983, Reed and Washburn 1921,

Mulvey 1914). Scholars and contemporary press articles claim that a large number of firms and securities salesmen took advantage of "naïve" investors by selling them securities backed by little more than a promise of extraordinary returns (Nation's Business 1917, 1922). 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:

> 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 between 1912 and 1931.

A typical blue sky law would require a firm to submit information about its operations and financial characteristics to the state banking commission prior to issuing securities. The types of information collected by regulators would include a listing of officers and directors, historical financial information, state of incorporation, properties, and locations of 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).

The laws applied to the state of location where a firm sold securities, which was often coincident with the state of incorporation of the firm. During the early 1900's, external financing was quite localized, given technological constraints, information barriers, and the relatively high costs of communication across dispersed investors. For example, Wright (2002) finds that the vast majority of shareholders for companies

incorporated in Maine were also local residents of Maine. In the empirical estimation of this paper, since information on the location of shareholders involved in public issuance activity during the sample period is not publicly available, the state of incorporation is assumed to be the location for a non-trivial fraction of a given firm's shareholders.[2]

There is ample anecdotal 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 a number of 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,

[2] This assumption is further supported in three ways. First, most mining firms in the sample have common states of incorporation and operation, which is to be expected given that mining location decisions are typically based on the geographic proximity of ore deposits. Huberman (2001) and Coval and Moskowitz (1999) find that shareholders have a bias towards owning shares of local companies; this bias is likely to be even larger during the sample period. Second, the findings in Table 10 support the link between state of incorporation and blue sky law jurisdiction. Third, a violation of this assumption would suggest that regression estimates in this study understate the true impact of the laws on corporate policy, since the laws would not necessarily have a binding impact on a firm's investors.

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). Overall, 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 in the early 1900's.

Although most 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 that there were other motivations for 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. Macey and Miller (1991) argue that the laws were promoted by the private interests of small banks who wished to reduce competition with securities salesmen for depositors' funds. They claim that regulatory compliance raised the costs of security issuance. In response, firms would pass these increased costs onto investors, who would then 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.

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).[3] They argue that some of the differences in investor protection levels across countries can be attributed to the political influence of 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 reasoning might 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, this hypothesis is directly tested in Section 4.

[3] See Volpin and Pagano (2001, 2005) for similar models.

Section 3: Data

The dataset is constructed using several hand-collected sources of information. First, I identify all mining firms which appear in the monthly "Banking and General Quotation" section of the December issues of the Commercial and Financial Chronicle (CFC) from 1908 to 1917.[4] I choose 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 (Brown, Mulherin, and Weidenmier, 2008). Second, I examine the entire universe of firms for which data is available in order to mitigate the possibility of survivorship or attrition bias in the regression analysis. That is, I include all firms for which data is available prior to the passage of the first blue sky law in 1911(mitigating survivorship bias) and I also include all firms for which data is available after the passage of the first law (reducing attrition bias).[5] 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 and are therefore a natural sample for estimating the impact of investor protection laws on corporate policy (Mahoney 2003, Seligman 2003). For each firm, the CFC provides par values of stock, as well as bid and ask quotes for common shares outstanding.

For each firm which appears in the CFC mining stock quotations, I then collect background characteristics and financial statement information from the Poor's Manuals of Industrial Securities, volumes 1910 to 1918. Each volume typically contains several years of (overlapping) historical data for each firm. Where available, I obtain brief descriptions of the company, state and year of incorporation, names of officers and directors, names 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.

In addition to firms which appear in the CFC, the Poor's manuals also contain financial statement information for mining firms lacking published stock price data. I use

[4] The years 1908 to 1917 are primarily chosen because of data availability and to allow for approximately 5 years of stock price data before and after the passage of most sample laws.

[5] The results are unaffected by whether we restrict the sample to firms which appear only before or only after the passage of the first blue sky law in 1911.

the 1910 Poor's volume to identify such companies and I track their balance sheet and income statement characteristics over time using subsequent Poor's manuals.[6] I include these firms to provide a more comprehensive dataset for analysis, though it is worth noting that all the results hold even if we restrict attention to the reduced sample of firms for which stock prices are available.[7]

I also collect various types of product market data. Many companies in the mining industry publicly disclose "statements of operations" in the Poor's manuals. I use these statements to assemble data on the types of metal ores produced, the quantities sold each year, and the market prices of all goods. I then collect aggregate data on new firm incorporations and authorized capital stock in IPO's from 1900 to 1930 for 12 states (Evans, 1948). The observations are defined at the state-year level for firms across all industries. For each state, I am able to determine the number of new firms (small or large, based on median size indices in Evans (1948) and the amount of authorized equity for these firms at the IPO stage. Assembling all of this information yields a final, unbalanced panel dataset of firm balance sheet data, income statement information, stock prices, and product market characteristics.

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 (Nation's Business 1917). 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.

[6] I use the 1910 volume to identify firms which operate prior to the passage of the first blue sky law in 1911, to mitigate concerns of survivorship bias.

[7] Additionally, I restrict attention to firms in the U.S. If Canadian firms are added to the sample as a control group, the results are the same.

Second, in response to the passage of the blue sky laws, it is conceivable that some firms could relocate 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 better avoid securities registration costs and securities fraud liability. Such behavior, however, does not appear to be relevant for firms in our sample; almost all sample firms are incorporated well before 1913 and do not reincorporate during the sample period. It is likely that geographic proximity to mineral tracts figures most prominently in mining firm's location decisions, particularly during the sample time period. Thus, it is unlikely that regression estimates are biased by sample firm location decisions.[8]

Table 1 contains information on the timing of the blue sky laws and lists the number of firms in each state within the dataset. Overall, the dataset contains information on firms in 25 states, with the largest number of observations stemming from firms in states such as Maine and Michigan, which pass blue sky laws in 1913 and 1915, respectively, and firms in New Jersey, which passes its blue sky law in 1920. As the table illustrates, almost all states pass a blue sky law within a narrow window, from 1912 to 1923. The small window for the passage of the laws is helpful for the empirical strategy because it mitigates the potential concern that the measured response of firms to the blue sky laws is biased because the firms are responding in very different (i.e. incomparable) economic times.[9] [Set Table 1 about here.]

Table 2 contains descriptive statistics of sample firm characteristics. There are a total of 152 unique firms with balance sheet data for the years 1899 to 1918, yielding a dataset of 1215 firm-year observations.[10] As Panel A indicates, the average year of incorporation is 1898; almost all sample firms are incorporated several years before an investor protection law is passed. The average age of a firm in the sample is 15 years.

[8] 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 Delaware, less than 10% of sample firms are incorporated there. 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.

[9] Additionally, in tabulations not reported here, there are relatively similar numbers of observations for firms in the control vs. treatment groups in each year in the sample starting from 1913 onwards.

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

Panel B summarizes sample firm balance sheet and income statement characteristics in 1910, prior to the passage of the first blue sky law in 1911, separately for all firms, firms located in states which pass 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), this breakdown of firms does not reflect two disparate groups of control vs. treated firms in the regression framework. In the estimation, any firm in a state which has not yet passed a law is a 'control' firm, while any company in a state where a law has been passed is a 'treatment' firm. For example, all firms in Michigan are control firms until 1915.[11] Additionally, all results hold when the sample is restricted to firms in states which pass the laws during the sample period. [Set Table 2 about here.]

The average firm produces $3.57 million in sales in 1910; approximately 27% of sales are distributed as dividends to equity holders. The high payout percentage could be the result of greater investor demand for dividends during this time period (Nation's Business 1922, Miller 1977) or could reflect firms' desire to signal their quality in a market lacking alternative means of quality differentiation (Miller and Rock 1985, John and Williams 1985). The mean book value of assets is $15.5 million, with 69% of assets invested in Plant, Property, and Equipment (PPE). The relatively high fraction of assets in PPE is not surprising, given that the main assets of mining companies are the properties containing mineral tracts and the machines used for exploration and extraction. Sample firms have approximately 610,000 common shares outstanding, with a market capitalization of $13.2 million. Mean operating profitability, proxied by return on assets (ROA) is 14%, while market capitalization-to-free cash flow ratios are approximately 7.77 for all sample companies. T-tests indicate that firm characteristics between the two groups depicted in Panel B are statistically indistinguishable, though it is worth reemphasizing that any potential differences are muted by the fact that all sample firms are control (treatment) firms prior to (after) the passage of their respective states' blue

[11] See Bertrand and Mullainathan (2003) for a similar explanation.

sky laws.[12] Overall, the sample of firms represents a broad swath of companies in the mining industry that appear to be remarkably similar across a number of observable dimensions.

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:

$$Dependent\ Var. = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \beta_4(Controls_{it}) + \varepsilon_{it}, \qquad (1)$$

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 that 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*. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. $Controls_{it}$ are various controls which may vary by firm and year.[13]

The coefficient for $IPLaw_{it}$ is effectively a difference-in-difference estimate of the average impact of investor protection laws on the dependent variable of interest. In this framework, any firm incorporated in a state which has not yet passed a law is considered a control firm, while any firm in a state where a law has been passed is 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).[14] Firm fixed effects are included to ensure that static

[12] This issue is further addressed in the Analysis section. It is also worth noting that the regression results are not driven by outliers in the sample; all regressions results are the same, if not stronger, for samples winsorized at the 1% and 5% tails.

[13] These controls include measures of sales growth and (log) firm age. Sales growth is sometimes used as a proxy for investment opportunities (LLSV 2000), but is likely to be an endogenous outcome variable that should be excluded as a regressor. The coefficient for Ln(firm age) is primarily estimated off the concavity of the log function, and is difficult to interpret in the presence of year fixed effects. For these reasons, I report results for specifications excluding these controls, though it is worth noting that the estimated treatment effects are similar when these controls are included in the regression analysis.

[14] For a similar formulation, see Bertrand and Mullainathan (2003).

differences across companies do not account for observed patterns in investment and financing. Year fixed effects control for time period-specific changes in investment and financing. Standard errors are clustered by state in order to control for residual correlations of the error terms across firms within a given state (Bertrand, Duflo, Mullainathan 2004).[15]

To shed light on the dynamics of the average treatment effect before and after the passage of a law, *IPLaw* in specification (1) is replaced by variables *IPLaw_Before* and *IPLaw_After*. The resulting equation is the following OLS regression (time trends) model:

$$\textit{Dependent Var.} = \alpha + \beta_1(IPLaw\_Before_{it}(-2)) + \beta_2(IPLaw\_Before_{it}(-1)) + \beta_3(IPLaw\_After_{it}(0)) + \beta_4(IPLaw\_After_{it}(1)) + \beta_5(IPLaw\_After_{it}(2+)) + \beta_6(Firm_i) + \beta_7(Year_t) + \beta_8(Controls_{it}) + \varepsilon_{it}, \quad (2)$$

$IPLaw_{it}\_Before_{it}(-2, -1)$ 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 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 firm *i*.

The coefficients on these terms provide estimates of the average changes in the dependent variable of interest in the immediate years preceding and following the passage of a blue sky law. The estimates are useful for evaluating the possibility that the investor protection statutes were passed either in response to or in conjunction with gradual, unobservable changes in investment opportunities in states that passed laws during the sample period. Examples of such changes could consist of growth in product demand, improvements in excavation technologies, new discoveries of ore deposits, etc. If the laws were passed at the same time that investment opportunities improved in sample treatment states, then the coefficient estimates of *IPLaw* in specification (1) would overstate the impact of the blue sky laws on corporate policy and performance.

Both regression specifications are estimated for various subsamples. The full sample is used for estimating the average (dynamic) impact of investor protection statutes

[15] The results are robust to clustering standard errors at the firm-level.

for all firms across time. To explore the possibility that the blue sky laws were passed simultaneously with immediate changes in investment opportunities, I also estimate the time trends model for a subsample of treated firms in Michigan and West Virginia and control firms in states which do not pass blue sky laws during the sample period. 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 time 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. Estimates from the time trends model for this subsample of data can be used to test this hypothesis.

I also evaluate whether the impact of the blue sky laws is driven by the mechanism of ex-post fraud reduction or by the mechanism of securities pre-clearance. As described in Section 2, 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, most laws also required that securities be registered with the government prior to any public offering. One exception to the securities pre-clearance feature of the blue sky laws was promulgated by Maine in 1913; while Maine's blue sky prohibited ex-post fraud, it did not require firms to register their securities with the government prior to public issuance (Mahoney 2003). I estimate the time trends model for a subsample of treated firms in Maine and control firms in states which do not pass blue sky laws during the sample period. The results of this analysis can be used to test whether the blue sky laws primarily affect firms through the ex-post reduction of expropriation risk after issuance or through the reduction of adverse selection at the pre-clearance stage.

4.1 Financing Decisions

4.1.1 Payout Policy

The impact of investor protection law on firm payout policy is estimated using specifications (1) and (2) with $Dividends_{it} / Sales_{it}$ as the dependent variable. $Dividends_{it} /$

$Sales_{it}$ is the ratio of common stock dividends to sales for firm *i* in year *t*. The regression results are presented in Table 3. Overall, the results indicate the passage of the blue sky laws is associated with an economically large and statistically significant increase in dividend payouts. Column 1 of Table 3 depicts an estimate of 0.062 for the treatment effect; when controlling for year fixed effects, the estimate increases to 0.131. The increase in the size of the estimates is not surprising, as there are likely time effects in sales and dividend payments across firms. These figures imply that the blue sky laws are associated with a 6.2% to 13% average increase in dividends to sales. [Set Table 3 about here.]

These results are consistent with various anecdotes concerning the nature of investment and securities fraud during the early 1900's. As discussed in Section 2, much of the fraud which took place in the mining industry during this period stemmed from investors being falsely promised extraordinary dividends in return for stock investments. Many investors, hoping to receive dividends on the capital they contributed during public securities offerings, were expropriated under the guise of legitimate mineral excavation and exploration. The blue sky laws appear to have given investors a legal fulcrum to exert pressure on firms to disgorge cash in the form of dividend payments. This mechanism is precisely the "outcome" model of LLSV (2000), which predicts firms increase dividend payouts when investor protection laws are stronger in order to mitigate the increased (expected) costs of alleged fraud. Interestingly, the findings reject the empirical importance of an alternative theory relating dividends to investor protection laws: a "substitute" theory of dividends would suggest dividend payments should decrease when investor protection laws are stronger because firms no longer need to pay high dividends in order to signal their quality in a weak investor protection regime. Empirically, the results illustrate the "outcome" model appears to be more salient than the "substitute" model of dividends and investor protection law.

Columns 3–5 provide evidence further supporting the causal interpretation of the treatment effect point estimates in columns 1 and 2. It is possible that the laws were coincident with gradual, unobserved changes in investment opportunities or other economic factors specific to each state, thereby causing regression estimates to overstate the impact of the blue sky laws on dividends. To evaluate this alternative hypothesis,

column 3 presents estimates of average changes in dividend payments around the passage of the blue sky laws. If dividends are already changing in response to changing, unobserved investment opportunities around the passage of the laws, then one should observe significant changes in dividend payments in the immediate years preceding the passage of the blue sky laws. In contrast to this hypothesis, however, column 3 shows there are no significant changes in dividend payouts in the two years preceding the passage of the laws (coefficients for *IPLaw_Before(-2)* and *IPLaw_Before(-1)* are statistically indistinguishable from zero), while significant changes in dividend payouts are observed in the immediate years following the passage of the laws (coefficients for *IPLaw_After* range from 9.3% to 13.6%). Figure 1 plots the treatment effect coefficients and graphically illustrates the observed time pattern in dividend payments around the introduction of the blue sky laws. [Set Figure 1 about here.]

To verify that the measured changes in dividends are not simply companies' response to immediate, unobserved changes in investment opportunities or other factors which affect payout policy, column 4 presents estimates from the time trends model for a subsample of firms in Michigan and West Virginia and firms in states which do not pass blue sky laws during the sample period. This subsample analysis is informative because Michigan and West Virginia initially passed blue sky laws in 1913 that were quickly deemed unconstitutional by federal courts; the states then passed revised laws in 1915. If the blue sky laws were passed simultaneously with immediate changes in unobserved factors, then one should see statistically significant increases in dividend payouts in 1913 and 1914—the immediate years following the passage of the first law in each state. In contrast to this hypothesis, however, the coefficient estimates in column 4 indicate firms in Michigan and West Virginia only increase dividends in the immediate years following the passage of the constitutionally upheld blue sky laws in 1915, not in the laws passed in 1913; the coefficients for *IPLaw_Before(-2)* and *IPLaw_Before(-1)* are statistically indistinguishable from zero. This evidence further solidifies the causal interpretation of the estimated treatment effect.

Finally, to test whether the impact of the blue sky laws is primarily driven by the mechanism of ex-post fraud reductions vs. securities pre-clearance, column 5 presents estimates for a subsample of treated firms in Maine and control firms in states which do

not pass a blue sky law during the sample period. Maine's blue sky law prohibited securities fraud after issuance but did not require securities registration with the government. If securities pre-clearance is the main channel through which the blue sky laws affect corporate policy, then the measured treatment effect of the Maine law on payout policy should be zero. In contrast to this hypothesis, however, dividends for Maine firms increase significantly following the passage of the Main blue sky law. The estimated effects of the blue sky law in Maine are very similar both in magnitude and in timing to the effects estimated for firms in the full sample; dividends to sales increase by 9.1% to 13.6% for companies in Maine. Overall, the evidence illustrates the impact of the blue sky laws is not simply a function of securities registration, but rather, reflects the empirical importance of ex-post fraud reduction in affecting firm payout policy, consistent with the theoretical predictions of LLSV (2000).

4.1.2 Equity Issuance

The impact of investor protection statutes on firm common stock outstanding is estimated using specifications (1) and (2) with $Ln(Shares_{it})$ as the dependent variable, which is defined as the log of common shares outstanding of firm i in year t.[16] The results are presented in Table 4. Collectively, the results indicate firms issue more equity once the blue sky laws are passed. Columns 1 and 2 show the estimated treatment effect ranges between 0.095 and 0.128, which means that firms issue approximately 10% to 13.7% more equity once the investor protection statutes are in place. [Set Table 4 about here.]

The increase in equity outstanding is likely driven by outside investors rather than insiders, thereby implying that firms are able to raise more external capital from financial markets once the blue sky laws are in place. There are two reasons for this interpretation. First, the blue sky laws generally did not apply to issuances of securities to insiders such as managers or pre-existing stock holders; if equity issuances were solely made to these groups, there should be no observed changes in equity outstanding around the blue sky laws (Reed and Washburn 1921). Second, Holderness, Kroszner, and Sheehan (1999)

[16] Stock splits in the sample are rare; only one treatment firm appears to have reduced shares outstanding by half after the law is passed.

find that managerial stock ownership during the early part of the twentieth century was quite limited (at least relative to current times), suggesting that increases in common stock outstanding were unlikely to be realized by insiders exclusively.

The findings are consistent with historical accounts of the immediate, increased availability of capital once investors were able to more securely invest in public offerings (Nation's Business, 1917). Because mining firms were primarily the types of companies engaged in securities fraud, it perhaps not surprising that these firms were able to raise more capital once investors had legal recourse for expropriation. The findings are strongly supportive of theories that predict greater investor protections lead to increased participation in equity markets because investors become more willing to partake in capital investment (LLSV 1997, Shleifer and Wolfenzon 2002).

To verify that the changes in equity issuance are not driven by unobservable changes in investment opportunities or other factors related to external financing, columns 3 and 4 illustrate that the observed changes in equity outstanding only occur in the years following the passage of the blue sky laws. For the full sample, there are no statistically significant changes in shares outstanding in the immediate two years prior to the passage of the blue sky laws. In contrast, outstanding equity increases by 6.4% to 14.4% in the years following the passage of the blue sky laws. A similar pattern can be found for the Michigan/West Virginia treatment subsample; the observed changes in equity outstanding occur only when the blue sky laws are passed and upheld after 1915 rather than in the years 1913 and 1914. Additionally, column 5 illustrates increased stock issuance is common for firms in Maine and mirrors the results for the full sample, further supporting the mechanism of ex-post fraud reduction as the key feature of the blue sky laws that impacts corporate financing decisions.

4.2 Firm Size

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.[17] Table 5 depicts regression results for specifications (1) and (2) using $Ln(PPE_{it})$ as the dependent variable. The findings indicate firms invest and grow

[17] The findings are similar if market capitalization is used as an additional measure of firm size.

in size once the blue sky laws are in place. The coefficient of *IPLaw* measures the approximate elasticity of physical capital to the passage of a blue sky law. Columns 1 and 2 show the elasticity of physical capital to the passage of investor protection laws is approximately between 29% and 39% (the point estimates of *IPLaw* range between 0.257 and 0.327). Columns 3 through 5 illustrate the measured changes in PPE appear to occur only once the laws are passed; there are no significant changes in PPE in the immediate years prior to the passage of the blue sky laws. [Set Table 5 about here.] As with the findings on payout policy and external financing, the results suggest the observed changes in physical capital stock are not driven by unobserved changes in investment opportunities around the passage of the laws. Furthermore, the evidence in column 5 shows the impact of the blue sky laws in Maine resembles the average effects of the blue sky laws for other sample states, supporting the notion that the legal prohibition of investor expropriation is the key channel through which the blue sky laws impact corporate investment.

The data indicate mining firms are able to raise capital and invest the proceeds into property and equipment used for mineral exploration and excavation. The results are consistent with various models that posit firms grow in size when investors are better protected from securities fraud (LLSV 1998, Shleifer and Wolfenzon 2002, LLS 2008). PPE is the primary asset of mining firms during this period; Table 2 indicates approximately 69% of assets are comprised of PPE for the typical mining firm. Casual observation of the data further suggests the most valuable item within PPE is the property owned by a company.[18] The high percentage of investment in property is attributed to the primary function of these firms: to extract minerals and metal ores from the land and quarries they owned. Historical accounts of securities fraud during this period indicate that mining firms would often justify the use of external capital for the purposes of drilling and excavation. Thus, it is perhaps not surprising that firm investment into physical capital increases once companies are more able to access capital markets for external financing. The nature of PPE in the mining industry also helps explain the large magnitude of the treatment estimate. Property is a unique asset in that it can be quickly

[18] This assessment if based on a limited number of companies which separate PPE into its individual components in the Poor's manuals.

acquired in large quantities upon an immediate influx of capital (in contrast to machinery or other types of assets which could take more time to accumulate).

The impact of investor protection statutes on firm assets, measured by $Ln(Assets_{it})$, is estimated using specifications (1) and (2). The results are presented in Table 6. Overall, the findings indicate firms increase the size of their assets once the blue sky laws are passed. [Set Table 6 about here.] Columns 1 and 2 show that the treatment effect ranges between 0.176 and 0.315, which implies an elasticity of assets to blue sky laws of 19.2% to 37%.[19] Columns 3 and 4 indicate the impact of the laws appears to manifest primarily in the years following the passage of the laws, rather than in the years prior to the laws being passed. This evidence suggests the changes in book assets of firms are not simply driven by unobservable changes in investment opportunities or other factors related to firm growth. Column 5 shows the impact on assets is similar for Maine firms as it is for the whole sample, further reinforcing the importance of ex-post fraud reduction as a key feature of the blue sky laws. Collectively, the results strongly support theories that predict that investor protection laws promote growth in firm size (LLSV 1998, Shleifer and Wolfenzon 2002, LLS 2008).

4.3 Operating Performance and Valuation

The estimated effects of the blue sky laws on financing and investment decisions indicate firms respond to the laws by paying out greater dividends, raising equity from capital markets, and growing in firm size. The findings are consistent with various theories regarding the importance of investor protections on corporate policy. However, the evidence per se does not show whether the increased activity by firms is profitable. To investigate the performance implications of the blue sky laws, I estimate the effects of the blue sky laws on operating profitability and market valuations.

4.3.1 Operating Profits

[19] The treatment effect estimate is only statistically significant at the 10% level in column 2, but this is likely due to the presence of non-PPE related items in firm assets that have little relation to investor protection statutes; these items are not expected to change around the passage of the blue sky laws and therefore they mitigate the total impact of the laws on book assets.

The impact of investor protection statutes on operating performance is estimated using specifications (1) and (2) with ROA_{it} as the dependent variable, 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 from firm income statements either by using data on Sales and Total/Operating Expenses or by using company reported figures for net profits. The data on operating income is admittedly noisy, as the reporting of operating income appears to be less consistent across firms than balance sheet data. For example, operating income is straightforward to calculate as the difference between sales and operating expenses for many companies; for other firms, however, operating income is simply proxied by a single item such as "net profits". Additionally, interest expenses, deprecation, and taxes are either explicitly reported or not disclosed at all (and are thus potentially components of "Total/Operating Expenses" listed in the income statements). While ROA is potentially mismeasured, however, it is unclear whether the data is systematically biased in any particular way. It is likely that any measurement error increases the size of regression standard errors, causing coefficient estimates of *IPLaw* to understate the impact of investor protection laws on operating performance (in terms of statistical significance).

Regression estimates are presented in Table 7. Columns 1-2 indicate the coefficient for *IPLaw* is positive and statistically significant, ranging between 0.058 and 0.071. Intuitively, these results imply the blue sky laws are associated with an average increase in ROA of 6% to 7.4%. The estimates are only weakly significant at the 10% level. While the weak statistical significance of the difference-in-difference estimates could be explained by measurement error, column 3 illustrates the results are more likely to be explained by the dynamics of ROA after the passage of the blue sky laws. Column 3 shows the positive, statistically significant changes in ROA occur only two years after the passage of the typical blue sky law. [Set Table 7 about here.] These results are in line with the characteristics of the mining industry. Mining firms are able to raise capital and increase the size of their mining operations once a blue sky law is passed, but profits which result from these decisions take time to be realized. The relative lag time in profitability likely reflects the fact that mineral exploration and excavation are limited by

technological constraints (particularly during the 1900's), thereby leading to observed changes in profitability only two years after a blue sky law gets passed.

Column 4 shows statistically significant changes in profitability occur two years after blue sky laws are passed in Michigan and West Virginia, illustrating the laws do not appear to be coincident with unobserved changes in investment opportunities. Column 5 shows that changes in operating profitability are also observed for firms in Maine, suggesting that ex-post fraud reduction is the salient feature of the blue sky laws.[20] Overall, the results strongly support the hypothesis that improvements in investor protection law lead to positive NPV-driven changes in corporate financing and investment decisions, consistent with the theoretical models highlighted above.

4.3.2 Market Valuations

The impact of investor protection statutes on market valuation-to-cash flow, $MarketValue_{it} / CashFlow_{it}$, following LLSV (2002), is estimated using specifications (1) and (2). $MarketValue_{it}$ 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 sales minus the sum of operating expenses, taxes, and net capital expenditures for firm i in year t.[21] The dependent variable captures the market's valuation of a firm's outstanding equity given its cash flows. As discussed in LLSV (2002), this measure is a useful way to examine the market's valuation of cash flows and expropriation risk; the higher is the risk that insiders may steal free cash flows, the lower is the conditional market value of equity. Regression estimates are presented in Table 8. Overall, the data indicate that price-to-cash flow ratios increase in response to the blue sky laws.[22] [Set Table 8 about here.]

[20] The findings for Maine also rule out the possibility that the improvements in ROA are simply the result of improved financial transparency due to the blue sky laws, since Maine's law did not require securities pre-clearance and impact financial reporting.

[21] 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 lack 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.

[22] The results are similar, if not stronger, if we instead examine price-to-earnings multiples. Price-to-cash flow ratios are a more relevant valuation metric for the purposes of this study, mainly because price-to-earnings may increase simply because of an increase in positive NPV capital expenditures, rather than a reduction in free cash flow expropriation risk.

Columns 1 and 2 depict an estimated treatment effect of 1.158 to 3.197. Assuming that the average firm has a market value-to-cash flow ratio of 8.28 (based on 1910 figures from Table 2), these numbers suggest that price-to-cash flow multiples increase by 14.0% to 38.6%. Investors in the market appear to strongly support the passage of the blue sky laws, and equity prices reflect the market's anticipation of subsequent years of increased dividends resulting from the laws.

Column 3 shows that there do not appear to be any pre-period time trends in price-to-cash flow ratios, supporting the notion that observed changes in policy and performance are not driven by underlying trends in unobservable investment opportunities. Interestingly, column 4 indicates that market value-to-cash flow ratios appear to increase in 1913 and 1914, rather than increase solely in 1915 and thereafter (in contrast to previously examined corporate policies). This finding could reflect the market's belief that Michigan and West Virginia would eventually pass blue sky laws that were deemed constitutional even after its initial laws were rejected by federal courts.

Column 5 illustrates that Maine firms increase in value significantly around the passage of the Maine blue sky law. The valuation improvements observed in the immediate years following the law's passage show that the market reacts favorably to the legal prohibition of ex-post fraud. Valuations of Maine companies also appear to increase in the year preceding the passage of the law; however, this observation is likely related to the fact that Maine's law was adopted on April 9, 1913, and was probably discussed prior to the start of 1913 (Di Trolio, 2003-2004). Assuming the market was able to correctly foresee the law's eventual passage, it is perhaps not surprising that the stock price would adjust accordingly, even before the law was formally enforced.

The evidence strongly supports theories that predict greater investor protections lead to improved market valuations (LLSV 2002, Shleifer and Wolfenzon 2002). The reduction of insider expropriation risk engendered by the blue sky laws provided investors with greater incentive to invest in mining securities. The resulting increase in capital allowed mining firms to accumulate mineral tracts and properties, increase operating profits, and transfer larger dividends to investors. Collectively, the results indicate that the blue sky laws were met with a favorable response by market participants.

4.4 Political Economy Hypotheses

The evidence thus far illustrates the effects of the blue sky laws on corporate financial policy, investment decisions, and value. The analysis of time trend models for all firms and for subsamples of firms in Michigan and Virginia illustrate the observed changes in corporate behavior do not appear to be caused by unobservable changes in economic conditions. Additionally, the analysis of companies in Maine indicates the protection of investors from ex-post fraud is the key mechanism by which the blue sky laws affect firm activity.

Most academic research and anecdotal evidence point to the public interests of securities fraud prevention as the primary motivation for the passage of the blue sky laws (Seligman 1983, Reed and Washburn 1921, Mulvey 1914, Macey and Miller 1991). The findings thus paint an accurate portrayal of the impact of an exogenous change in investor protection law on corporate policy and performance. Nevertheless, this section explores two additional, alternative explanations for the passage of the blue sky laws, both of which speculate on the prevailing political environment of the early 1900's. The first hypothesis relates to state banking interests. Macey and Miller (1991) argue the blue sky laws were passed at the behest of banks that competed with mining firms and other corporate entities for investor capital. They theorize the laws would cause security issuance costs to rise, and that firms would pass these costs onto investors; in turn, investors would respond by redirecting their capital into banks rather than equity markets.

This hypothesis is unlikely to have to significant explanatory power. If this supposition were true, then one would expect to find deleterious effects of the blue sky laws on corporate policy and performance. Firms should exhibit reduced capital market access, decrease in size, and have lower valuations. In contrast, the findings illustrate investors appear to have benefited greatly from the blue sky laws and responded to their passage by channeling greater amounts of capital into mining firms and increasing stock valuations.[23]

A second alternative explanation for the findings is that the blue sky laws were a manifestation of political lobbying efforts by incumbent mining firms. While this

[23] At the very least, the Macey and Miller (1991) hypothesis would suggest the evidence documented in this paper understates the positive impact of the blue sky laws on corporate decisions and performance.

hypothesis has not been mentioned in the extant literature on the blue sky laws, it is theoretically possible the laws were simply the result of incumbent industrial firms lobbying to reduce competition from potential market entrants by raising the costs of regulatory compliance. If this hypothesis were true, then regression estimates of the treatment effects would overstate the impact of the blue sky laws on corporate policy and performance. Specifically, by limiting entry by competitors, incumbent firms could find it easier to issue equity, grow in size, and increase profits due to their improved standing in product markets.

I evaluate this hypothesis by testing three of its implications. The first implication is that if the blue sky laws improved incumbent firms' product market power (implicitly, mining product markets are imperfectly competitive), then one might expect to observe higher prices and lower quantities of output produced by mining firms. These changes could result from an inward shift in the supply curve due to reduced competition. Alternatively, holding aggregate supply constant, these changes could stem from the increased ability of incumbent firms to determine market prices and output.

To test whether mining firms appear to have increased market power as a result of the blue sky laws, I estimate the changes in the prices and quantities of metal ores sold by mining firms. As described in Section 3, 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. I estimate specification (1) with product prices and quantities as dependent variables; specifically, I define $Ln(Quantity_{it})$ as the log of ore quantities (tons or ounces) produced for firm *i* in year *t*, and $Ln(Price_{it})$ as the log of ore prices (per ton or ounce) sold by firm *i* in year *t*.

The results are presented in Table 9. 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 effects of the laws on the prices of Copper, Silver, and Gold ores. The results for ore quantities indicate the blue sky laws are associated with increases in quantities of ores produced. [Set Table 9 about here.] The coefficient estimates of 0.436 for *IPLaw* in column 2 and 0.661 for *IPLaw* in column 3 are economically large. The results imply 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 suggests copper production does not decrease significantly in response to the blue sky laws. Columns 4-6 illustrate estimates of the laws' effects on ore prices are statistically insignificant. The measured coefficients on copper and gold prices are economically trivial, while the price effect for silver is qualitatively negative.[24]

Two additional implications of the industrial lobbying hypothesis are that the blue laws led to a reduction in the creation of new firms and also led to a reduction in the amount of capital raised by new firms. To evaluate these implications, I collect data on new incorporations and their authorized capital stock in IPO's (by state) and test whether the number of new companies and the amount of authorized capital stock decreased around the passage of the blue sky laws. The data includes firms aggregated across all industries; I am unable to separately identify mining firms. However, given that companies in non-mining industries were considered less fraudulent than mining firms, it is likely that the regression estimates of aggregate capital formation provide a lower bound for the impact of the blue sky laws on mining firms.

The impact of investor protection statutes on new capital formation is estimated using OLS specification (3):

$$Dependent\ Variable = \alpha + \beta_1(IPLaw_{it}) + \beta_2(State_i) + \beta_3(Year_t) + \varepsilon_{it} \qquad (3)$$

where subscripts *it* uniquely identify individual observations for state *i* in year *t*. The dependent variables I examine are: $Incorporations_{it}$, the number of new incorporations in state *i* in year *t*; $Ln(Stock_{it})$, the log of newly authorized capital stock in state *i* in year *t*; and $Ln(Stock_{it}/Incorporations_{it})$, the log of capital stock authorized by the average new firm.

Table 10 presents the results for regressions estimated separately for new firms of different sizes, based on median size indices calculated by Evans (1948). Columns 1 and 2 indicate total new capital stock weakly increases around the passage of the blue sky

[24] As Table 8 illustrates, there are many more observations for the Quantity regressions than for the Price regressions; this disparity is simply due to data availability in the Poor's manuals. The results for the quantity regressions are similar if the sample is limited to those firms with non-missing price data.

laws, with smaller firms showing a slight, statistically significant increase. [Set Table 10 about here.] The increase in capital stock ranges between 14.7% and 34.9%; the large magnitudes for these broader measures of capital formation can be attributed to the same reasons underlying disaggregated estimates presented in Tables 4-6. Columns 3 and 4 illustrate that the number of new firms created appears to increase (though estimates are imprecise), while columns 5 and 6 show that the amount of capital authorized by the average new firm increases significantly (by a percentage ranging between 6.6% and 36.2%).

Although the treatment effects are sometimes statistically insignificant, across all specifications, the positive coefficient estimates do not support the hypothesis that the blue sky laws lead to a decrease in capital formation either through a reduction in the amount of capital raised by new firms or through a reduction in the number of new firms created.[25] The results from Tables 9 and 10 indicate product markets do not appear to become less competitive after investors are better protected from securities fraud. The findings thus suggest the blue sky laws did not result from lobbying efforts by incumbent mining firms. If anything, the results actually illustrate that the blue sky laws are effective at promoting greater entry by new firms and facilitating capital allocation to newly formed companies.[26] The findings could even be interpreted as support for some of the broader implications of investor protection theories, which posit stronger legal institutions lead to greater economic growth (LLSV 1998, LLS 2008).

Conclusion

The findings in this paper address recent debate concerning the theoretical and empirical relevance of investor protection law. While various studies argue that safeguarding investors from insider expropriation is critical for a well-functioning system of corporate governance, others conclude that investor protection laws have little value. This paper improves upon the techniques utilized in existing empirical research

[25] This evidence also mitigates the possibility that the results in Table 9 could be explained by monopolistic incumbent firms reducing entry, maintaining product pricing power, and simply lowering their own costs of production through additional securities issuance.

[26] These findings also support the notion that a non-trivial fraction of a firm's initial shareholders are located within the firm's state of incorporation.

comprised of cross-country studies that exploit variation in legal origin and within-country studies that rely on time-series variation in regulatory reform.

Holding legal origin fixed, and exploiting both cross-sectional and time series exogenous variation in the passage of U.S. state blue sky laws, I find firms respond to the introduction of investor protection statutes by increasing dividends, raising equity, and growing in size. The laws are also associated with improvements in accounting performance and market valuations. The evidence is strongly supportive of theories that predict investor protection law has a significant impact on corporate financing and investment policy.

The results in this paper point to several avenues for additional research. It would be interesting to explore the broader implications of investor protection law on the functioning of securities markets. Recent work by Albuquerque and Wang (2008) has begun to explore this line of inquiry. Another fruitful topic would be to explore the extent to which investor protection laws are compliments or substitutes for other dimensions of capital markets reform. While this paper highlights the importance of investor protection law on corporate policy, there is still a need to understand why many countries do not have such legal institutions in place currently.

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Figure 1
Impact of Investor Protection Laws on Payout Policy

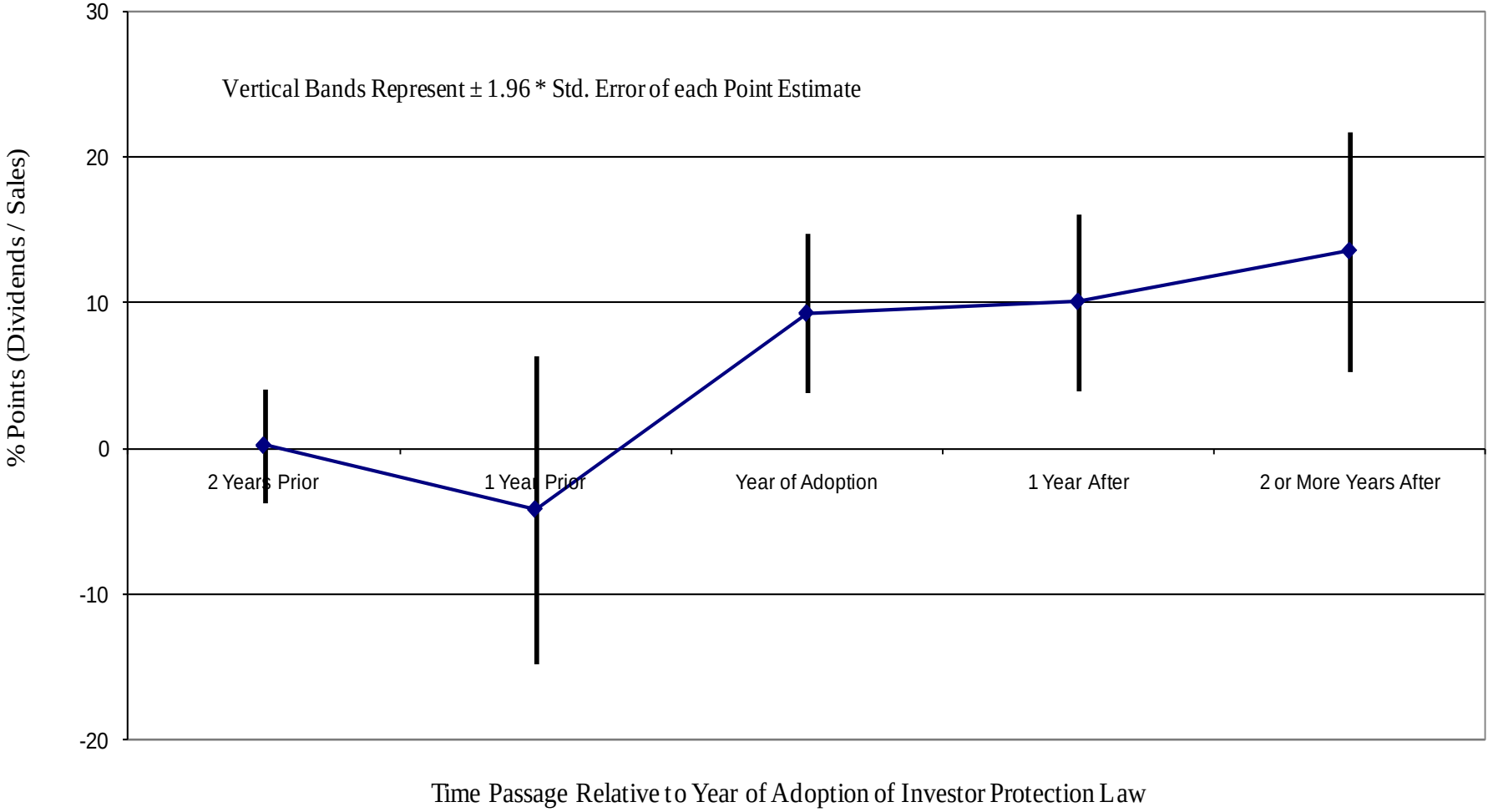

Table 1

Blue Sky Law Legislation and Sample States

This table lists sample states and the years in which they pass blue sky laws, along with the number of sample mining firms from each state.

State	Year of Law Passage	Number of Firms
Alabama	1919	1
Arizona	1912	7
California	1913	2
Colorado	1923	8
Delaware	1931	13
Georgia	1913	1
Kentucky	1920	1
Maryland	1920	1
Maine	1913	24
Michigan	1915	29
Minnesota	1917	6
Missouri	1913	2
Montana	1913	2
Nebraska	1913	1
New Jersey	1920	21
Nevada	----	1
New York	1921	8
Pennsylvania	1923	5
South Dakota	1913	1
Texas	1913	1
Utah	1919	1
Virginia	1916	2
Washington	1923	1
West Virginia	1915	11
Wyoming	1919	4

Table 2

Sample Descriptive Statistics

This table presents descriptive statistics of sample firm characteristics. Panel A indicates the sample contains 152 unique firms in mining industries with observations taken across sample years 1899-1918 for a total of 1,215 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 1910 (prior to the passage of all blue sky laws), separated by all firms, firms in states which pass laws during the sample period (<=1918), and firms in states which pass the laws after the sample period (>1918). *% PPE* is the fraction of (Book) *Assets* invested in Plant, Property, and Equipment. *Market Capitalization* is number of shares outstanding (*Common Shares*) times share price as of end of December, 1910. *Dividend/Sales* is the ratio of dividends on common stock to *Sales*. *ROA* is defined as operating earnings divided by book value of assets. *Market Cap/Cash Flow* is the ratio of *Market Capitalization* to *Free Cash flow*. All values are in nominal 1910 dollars.

Panel A: Sample Characteristics

Total Number of Firms	152
Sample Years	1899-1918
Total Number of Firm-Year Observations	1,215
Mean year of Incorporation	1898
Mean age (years)	15.33

Panel B: Sample Firm Characteristics

Year = 1910	All Firms	Law Passed > 1918	Law Passed <= 1918
Age	13.54	12.66	14.18
	(14.10)	(14.06)	(14.21)
Sales ($MM)	3.57	4.72	2.63
	(6.25)	(7.53)	(4.88)
Dividends/Sales	0.27	0.27	0.26
	(0.31)	(0.23)	(0.36)
Assets ($MM)	15.50	23.80	9.17
	(28.30)	(36.10)	(18.50)
% PPE	0.69	0.73	0.66
	(0.23)	(0.20)	(0.25)
Common Shares (MM)	0.61	0.74	0.51
	(1.03)	(1.06)	(1.00)
Market Capitalization ($MM)	13.20	13.90	12.90
	(29.20)	(27.80)	(30.40)
ROA	0.14	0.13	0.15
	(0.42)	(0.16)	(0.54)
Market Cap/Cash Flow	8.28	9.57	7.69
	(17.15)	(11.69)	(19.35)

Table 3

Estimated Effects of Investor Protection Laws on Payout Policy

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

$$Dividends_{it} / Sales_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it},$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Dividends_{it} / Sales_{it}$ is the ratio of common stock dividends to sales for firm *i* in year *t*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an IP Law by year *t*. In columns 3-5, $IPLaw_{it}$ is replaced by $IPLaw\_Before_{it}(-2, -1)$ and $IPLaw\_After_{it}(0, 1, 2+)$, which are indicators of whether the observation for firm *i* in year *t* takes place (2 years before, 1 year before) and (0 years after, 1 year after, 2 or more years after), respectively, 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. *Sample* describes the firms analyzed in each column: *Full* refers to all firms, *MI/WV* refers to firms in Michigan, West Virginia, and all states where an IP law is not passed during the sample period, *ME* refers to firms in Maine and all states where an IP law is not passed during the sample period. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

Dependent Variable: Dividends/Sales					
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.062***	0.131***			
	(0.011)	(0.025)			
IPLaw_Before(-2)			0.002	0.024	-0.071***
			(0.020)	(0.022)	(0.022)
IPLaw_Before(-1)			-0.042	-0.140	0.003
			(0.054)	(0.087)	(0.035)
IPLaw_After(0)			0.093***	0.073**	0.136***
			(0.028)	(0.031)	(0.020)
IPLaw_After(1)			0.101***	0.130***	0.091**
			(0.031)	(0.032)	(0.036)
IPLaw_After(2+)			0.136***	0.262**	0.120***
			(0.042)	(0.095)	(0.032)
Constant	0.263***	0.360**	0.358**	0.430*	0.429***
	(0.002)	(0.137)	(0.139)	(0.205)	(0.020)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.014	0.053	0.051	0.075	0.134
Sample	Full	Full	Full	MI/WV	ME
No. of firms	105	105	105	73	63
No. of obs.	669	669	669	474	372

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

Table 4

Estimated Effects of Investor Protection Laws on Equity Issuance

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

$$Ln(Shares_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it},$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(Shares)_{it}$ 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 IP Law by year *t*. In columns 3-5, $IPLaw_{it}$ is replaced by $IPLaw\_Before_{it}(-2, -1)$ and $IPLaw\_After_{it}(0, 1, 2+)$, which are indicators of whether the observation for firm *i* in year *t* takes place (2 years before, 1 year before) and (0 years after, 1 year after, 2 or more years after), respectively, 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. *Sample* describes the firms analyzed in each column: *Full* refers to all firms, *MI/WV* refers to firms in Michigan, West Virginia, and all states where an IP law is not passed during the sample period, *ME* refers to firms in Maine and all states where an IP law is not passed during the sample period. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

Dependent Variable: : Ln(Shares Outstanding)					
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.128***	0.095***			
	(0.026)	(0.032)			
IPLaw_Before(-2)			-0.010	-0.033	-0.020
			(0.024)	(0.020)	(0.015)
IPLaw_Before(-1)			0.004	-0.035	-0.051*
			(0.025)	(0.021)	(0.028)
IPLaw_After(0)			0.064*	0.021	0.057**
			(0.034)	(0.029)	(0.021)
IPLaw_After(1)			0.106**	0.114**	0.032
			(0.048)	(0.042)	(0.029)
IPLaw_After(2+)			0.144**	0.095	0.069
			(0.062)	(0.064)	(0.040)
Constant	12.256***	12.263***	12.237***	12.144***	12.686***
	(0.006)	(0.017)	(0.045)	(0.021)	(0.029)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.041	0.049	0.054	0.029	0.087
Sample	Full	Full	Full	MI/WV	ME
No. of firms	150	150	150	102	85
No. of obs.	1175	1171	1171	814	653

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

Table 5

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

This table presents estimates of the effects of state investor protection (blue sky) laws on firm plant, property, and equipment. The baseline specifications are OLS linear models:

$$Ln(PPE_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it},$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(PPE)_{it}$ 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 IP Law by year *t*. In columns 3-5, $IPLaw_{it}$ is replaced by $IPLaw\_Before_{it}(-2, -1)$ and $IPLaw\_After_{it}(0, 1, 2+)$, which are indicators of whether the observation for firm *i* in year *t* takes place (2 years before, 1 year before) and (0 years after, 1 year after, 2 or more years after), respectively, 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. *Sample* describes the firms analyzed in each column: *Full* refers to all firms, *MI/WV* refers to firms in Michigan, West Virginia, and all states where an IP law is not passed during the sample period, *ME* refers to firms in Maine and all states where an IP law is not passed during the sample period. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

Dependent Variable: Ln(PPE)					
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.327***	0.257**			
	(0.088)	(0.099)			
IPLaw_Before(-2)			-0.016	-0.130	0.013
			(0.055)	(0.076)	(0.133)
IPLaw_Before(-1)			0.030	-0.051	0.179**
			(0.073)	(0.055)	(0.080)
IPLaw_After(0)			0.190*	0.165	0.262***
			(0.106)	(0.123)	(0.044)
IPLaw_After(1)			0.289**	0.585**	0.217***
			(0.128)	(0.261)	(0.061)
IPLaw_After(2+)			0.352**	0.342	0.302
			(0.170)	(0.588)	(0.177)
Constant	14.893***	14.796***	14.790***	14.617***	15.274***
	(0.021)	(0.076)	(0.082)	(0.054)	(0.106)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.059	0.081	0.084	0.066	0.047
Sample	Full	Full	Full	MI/WV	ME
No. of firms	134	134	134	90	79
No. of obs.	871	871	871	572	531

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

Table 6

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

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

$$Ln(Assets_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it},$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $Ln(Assets)_{it}$ 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 IP Law by year *t*. In columns 3-5, $IPLaw_{it}$ is replaced by $IPLaw\_Before_{it}(-2, -1)$ and $IPLaw\_After_{it}(0, 1, 2+)$, which are indicators of whether the observation for firm *i* in year *t* takes place (2 years before, 1 year before) and (0 years after, 1 year after, 2 or more years after), respectively, 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. *Sample* describes the firms analyzed in each column: *Full* refers to all firms, *MI/WV* refers to firms in Michigan, West Virginia, and all states where an IP law is not passed during the sample period, *ME* refers to firms in Maine and all states where an IP law is not passed during the sample period. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

Dependent Variable: Ln(Assets)					
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.315***	0.176*			
	(0.100)	(0.095)			
IPLaw_Before(-2)			-0.061	-0.241**	0.033
			(0.070)	(0.090)	(0.073)
IPLaw_Before(-1)			0.024	-0.089*	0.078**
			(0.040)	(0.046)	(0.034)
IPLaw_After(0)			0.135*	0.163**	0.085**
			(0.071)	(0.071)	(0.032)
IPLaw_After(1)			0.169*	0.382***	0.066
			(0.093)	(0.097)	(0.038)
IPLaw_After(2+)			0.164*	0.065	0.147**
			(0.081)	(0.205)	(0.049)
Constant	15.253***	15.367***	15.369***	15.090***	15.948***
	(0.024)	(0.177)	(0.180)	(0.039)	(0.040)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.133	0.215	0.216	0.224	0.197
Sample	Full	Full	Full	MI/WV	ME
No. of firms	141	141	141	94	79
No. of obs.	963	963	963	654	545

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

Table 7

Estimated Effects of Investor Protection Laws on Operating Performance

This table presents estimates of the effects of state investor protection (blue sky) laws on firm operating performance (ROA). The baseline specification is an OLS linear model:

$$ROA_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it},$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. 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*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an IP Law by year *t*. In columns 3-5, $IPLaw_{it}$ is replaced by $IPLaw\_Before_{it}(-2, -1)$ and $IPLaw\_After_{it}(0, 1, 2+)$, which are indicators of whether the observation for firm *i* in year *t* takes place (2 years before, 1 year before) and (0 years after, 1 year after, 2 or more years after), respectively, 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. *Sample* describes the firms analyzed in each column: *Full* refers to all firms, *MI/WV* refers to firms in Michigan, West Virginia, and all states where an IP law is not passed during the sample period, *ME* refers to firms in Maine and all states where an IP law is not passed during the sample period. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

Dependent Variable: ROA					
	(1)	(2)	(3)	(4)	(5)
IPLaw	0.071*	0.058*			
	(0.038)	(0.033)			
IPLaw_Before(-2)			-0.017	-0.025	-0.012*
			(0.012)	(0.014)	(0.006)
IPLaw_Before(-1)			-0.010	-0.035**	0.019
			(0.010)	(0.012)	(0.011)
IPLaw_After(0)			0.038	0.032	0.024**
			(0.025)	(0.029)	(0.010)
IPLaw_After(1)			0.061	0.139*	0.028**
			(0.041)	(0.064)	(0.010)
IPLaw_After(2+)			0.032**	0.092***	0.035**
			(0.016)	(0.014)	(0.013)
Constant	0.128***	0.155***	0.158***	0.162***	0.090***
	(0.011)	(0.010)	(0.007)	(0.006)	(0.021)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.048	0.125	0.127	0.127	0.086
Sample	Full	Full	Full	MI/WV	ME
No. of firms	115	115	115	73	64
No. of obs.	619	619	619	410	329

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

Table 8

Estimated Effects of Investor Protection Laws on Market Value

This table presents estimates of the effects of state investor protection (blue sky) laws on firm market capitalization to free cash flow. The baseline specification is an OLS linear model:

$$MarketCapitalization_{it} / CashFlow_{it} = \alpha + \beta_1(IPLaw_{it}) + \beta_2(Firm_i) + \beta_3(Year_t) + \varepsilon_{it},$$

where subscripts *it* uniquely identify individual observations for firm *i* in year *t*. $MarketCapitalization_{it} / CashFlow_{it}$ is the ratio of market capitalization to free cash flow of firm *i* in year *t*. $IPLaw_{it}$ is an indicator of whether the state of incorporation of firm *i* has passed an IP Law by year *t*. In columns 3-5, $IPLaw_{it}$ is replaced by $IPLaw\_Before_{it}(-2, -1)$ and $IPLaw\_After_{it}(0, 1, 2+)$, which are indicators of whether the observation for firm *i* in year *t* takes place (2 years before, 1 year before) and (0 years after, 1 year after, 2 or more years after), respectively, 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. *Sample* describes the firms analyzed in each column: *Full* refers to all firms, *MI/WV* refers to firms in Michigan, West Virginia, and all states where an IP law is not passed during the sample period, *ME* refers to firms in Maine and all states where an IP law is not passed during the sample period. Standard errors, reported in parentheses, are heteroskedasticity-robust and clustered by state.

Dependent Variable: Market Capitalization to Free Cash Flow					
	(1)	(2)	(3)	(4)	(5)
IPLaw	1.158***	3.197**			
	(0.370)	(1.227)			
IPLaw_Before(-2)			0.325	6.834*	1.431
			(1.792)	(3.052)	(3.454)
IPLaw_Before(-1)			1.360	12.364**	13.469***
			(2.373)	(4.481)	(2.468)
IPLaw_After(0)			3.359	1.570	9.569**
			(2.840)	(3.976)	(3.115)
IPLaw_After(1)			4.608**	13.481***	7.345*
			(1.862)	(2.998)	(3.078)
IPLaw_After(2+)			4.207	13.228***	2.309
			(2.451)	(1.891)	(3.154)
Constant	5.732***	0.582	0.423	-3.838**	10.242*
	(0.121)	(4.200)	(4.106)	(1.175)	(4.427)
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	No	Yes	Yes	Yes	Yes
R^2	0.004	0.032	0.035	0.107	0.189
Sample	Full	Full	Full	MI/WV	ME
No. of firms	79	79	79	52	44
No. of obs.	402	402	402	282	191

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

Table 9

Estimated Effects of Investor Protection Laws on Product Markets

This table presents estimates of the effects of state investor protection (blue sky) 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 10:

Estimated Effects of Investor Protection Laws on Firm Creation

This table presents estimates of the effects of state investor protection (blue sky) laws on new firm incorporations and total authorized capital stock. The baseline specification is an OLS linear model:

$$Ln(Stock_{it}) \text{ [or] } Ln(Incorporations_{it}) \text{ [or] } Ln(Stock_{it}/Incorporations_{it}) = \alpha + \beta_1(IPLaw_{it}) + \beta_2(State_i) + \beta_3(Year_t) + \varepsilon_{it}$$

where subscripts *it* uniquely identify individual observations for state *i* in year *t*. $Ln(Stock_{it})$ is the log of newly authorized capital stock in state *i* in year *t*. $Incorporations_{it}$ is the number of new incorporations in state *i* in year *t*. $IPLaw_{it}$ is an indicator of whether state *i* has passed an Investor Protection (IP) Law by year *t*. $Firm_i$ and $Year_t$ denote firm and year fixed effects, respectively. *Sample* refers to the sample of firms (all or small), based on median size indices in Evans (1948). Robust standard errors are reported in parentheses.

	Ln(Authorized Capital Stock)		Ln(New Incorporations)		Ln(Authorized Stock / New Incorporations)	
	(1)	(2)	(3)	(4)	(5)	(6)
IPLaw	0.300	0.138*	0.074	0.074	0.309**	0.064
	(0.183)	(0.083)	(0.054)	(0.052)	(0.149)	(0.049)
Constant	11.830***	9.046***	7.771***	7.710***	5.139***	1.400***
	(0.486)	(0.220)	(0.142)	(0.136)	(0.395)	(0.130)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.647	0.639	0.717	0.720	0.619	0.584
Sample	Full	Small	Full	Small	Full	Small
No. of obs.	230	227	227	227	227	227
No. of states	12	12	12	12	12	12

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