

# MANDATED DISCLOSURE, STOCK RETURNS, AND THE 1964 SECURITIES ACTS AMENDMENTS\*

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## Abstract

We analyze the last major imposition of mandatory disclosure in US equity markets. The 1964 Securities Act Amendments required a group of firms traded over the counter (OTC) to periodically provide audited financial information, proxy information prior to shareholder meetings, and details on insider holdings and trades to their shareholders for the first time. This legislation left unchanged the disclosure requirements of all NYSE, all AMEX, and some OTC firms. When we use these unaffected groups as a counterfactual for the affected firms, we find that those firms that were newly required to make all types of disclosures required by the 1964 Act had a cumulative abnormal excess return of approximately 20% in the approximately year and a half between the initial calls for legislative action and the law's passage. In that same time period, firms for which proxy and insider information were the only new mandated forms of disclosure had a (marginally statistically significant) cumulative abnormal excess return of about 10%. In contrast, there is little evidence of a difference between the adjusted returns of affected and unaffected groups in a period when there is no new information about the law or which firms will comply with its requirements. Finally, event study analyses indicate that firms that initially register with the SEC after the 1964 Amendments experienced positive abnormal excess returns and modest reductions in bid/ask spreads.

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# 1 Introduction

Since the New Deal the federal government has actively regulated US equity markets and the centerpiece of these efforts has been the mandated disclosure of firm financial information. These interventions into the marketplace are controversial.<sup>1</sup> On the one hand, it is argued that such regulation is at best unnecessary, and at worst damaging. This argument is based on the notion that explicit contracts between involved parties and private litigation (and the threat of such) is a cost effective way to prevent misbehavior by insiders. Furthermore, reputation concerns of managers, underwriters and auditors reduce or eliminate the incentives for them to expropriate outside investors. In this case, mandated disclosure cannot have a positive effect on firms. On the other hand, the proponents of regulation argue that it may not be feasible to impose penalties (in litigation or in terms of lost reputation) that are high enough to insure honest disclosure. In addition, shareholders face free rider problems in monitoring. In principle either theory could be correct, so empirical evidence is necessary to help resolve this controversy.

This paper analyzes the consequences of the 1964 Securities Acts Amendments on the stock returns of affected companies. Notably, this legislation was the last major imposition of mandatory disclosure regulations in US equity markets but its consequences have gone unstudied. The 1964 Amendments required some firms traded on the Over the Counter (OTC) market that had been free of all mandatory disclosure requirements to comply with four different types of disclosure. Specifically, these firms had to: (1) register with the SEC; (2) provide quarterly updates on their financial position, such as audited balance sheets and income statements; (3) issue detailed proxy statements to shareholders, and (4) report on insider holdings and trades. Importantly, there were stiff statutory penalties associated with misstatements and the SEC was granted the power to investigate and issue fines. The Amendments also required a different set of OTC firms that had been complying with requirements (1) and (2) to begin issuing detailed proxy statements and reporting on insider holdings and trades.

The credibility of the analysis is aided by the availability of firms that were unaffected by this legislation. In particular, two separate sets of OTC firms and all firms traded on the New York and American Stock Exchanges were unaffected by the legislation. The benefit of this multitude of affected and unaffected groups is that it allows for multiple estimates of the effect of the mandatory reporting requirements.

One reason that an empirical analysis of the 1964 Amendments may not have been under-

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<sup>1</sup>The debate about the efficiency of mandated disclosure laws extends back to at least the vigorous exchange between Stigler (1964) Friend and Herman (1964).

taken previously is that the data to properly exploit the change in regulation were not available in machine-readable form. Using hard copies of *Barron's* newspapers from the 1960's and supplementing that with numerous other sources covering firms traded in the OTC market, we attempt to create the equivalent of the Center for Research in Security Prices (CRSP) data set that is commonly used in empirical studies of financial markets. The resulting data file contains information on share prices, dividends, stock splits, financial information, and SEC filing status for OTC companies from 1963 through 1966.

We exploit the structure of the 1964 Amendments and the timing of its passage to devise a test of its consequences. Specifically, we classify firms into affected and unaffected groups. These classifications are based on the firm characteristics two years before the legislation's passage in order to abstract from the possibility that firms endogenously choose membership in the affected or unaffected groups. We then compare the returns of the affected groups to those of the unaffected groups from the beginning of 1963 when efforts to extend mandatory disclosure were initiated until the legislation became law at the end of August of 1964. We argue that during this period, the probability of the extension of mandatory disclosure requirements to the affected groups increased steadily and eventually became one. Because equity markets are forward looking, we suspect the main effects of the law will be limited to this pre-law window where the expectation of passage was increasing.

Our basic finding is that firms that were required to begin mandatory disclosure due to the Amendments had positive abnormal returns in the period leading up to the passage of the law. This finding holds after differencing out the returns of unaffected firms and adjusting for the market return and the two Fama-French factors and a momentum factor. Specifically, the group of firms that were required to engage in all four forms of mandatory disclosure for the first time had a cumulative abnormal excess return of roughly 20% during the pre-law period. The second affected group, which was required to begin issuing detailed proxy statements and reporting on insider holdings and trades, had cumulative abnormal excess returns of approximately 10%. The effect on the first group would be judged strongly statistically significant by conventional criteria, while the findings for the second group are marginally significant.

We also study stock returns after the 1964 law was passed in order to further investigate the effects of the law. This also allows us to probe the validity of our research design and, more specifically, to evaluate our identifying assumption that the affected groups would not have had abnormal excess returns during 1963 and early 1964 in the absence of the law.

We find that from the time the law was passed through the initial disclosures of affected firms, the affected groups continued to outperform the unaffected groups. As we discuss in detail, the

interpretation of these results depends on one's beliefs about whether the market had the correct expectations about the information that was revealed in this period. While we argue that these results do not invalidate our findings about the pre-law period, we are also uncomfortable assigning these post-law returns to the law.

We also exploit the availability of a third period after the law has passed and after the vast majority of new filers have registered with the SEC. In this period there is no new information about the law or about which firms will comply with its requirements. For these reasons, we believe that this period provides the best opportunity to judge the validity of our approach. We find that we cannot reject the null hypothesis that either of the affected groups have zero abnormal excess returns in this period. This provides reassuring evidence on the validity of our research design and lends credibility to the possibility that the 1964 Amendments are the causal reason for the positive abnormal returns during the pre-law window

Finally, we present some preliminary evidence from event studies on the immediate consequences of registering with the SEC soon after the 1964 Amendments. First, we study returns around the date of initial registrations. We show that at least some of the post-law excess returns of affected firms were centered around the time of their first filing with the SEC under the 1964 Amendments. Second, we briefly explore to what degree increased liquidity was responsible for the effects of the law on stock prices. We show that there was a reduction in bid/ask spreads among affected firms upon registration with the SEC, but that the effect was fairly small. It appears unlikely that expectations of increased liquidity alone drove the equity market effects that we uncover.

Our results suggest that for shareholders' the benefits of the 1964 Amendments far outweighed the cost of complying with this law. The implication is that prior to 1964 managers of affected firms withheld information from shareholders, or were unable to disclose it in a credible manner, although shareholders valued this information. One interpretation of this evidence is that the Amendments allowed shareholders to discipline managers in ways either cheaper or more effective than were possible prior to the law. Even if this is the case, we cannot rule out the possibility that insiders lost an amount equal to or greater than that gained by shareholders.

There are a number of scenarios where the loss to shareholders from management's diversion of resources is greater than insiders' monetary gain. One example is "empire building" acquisitions or diversification into new lines of business, which may provide utility to company managers who enjoy being in charge of larger firm but have a substantial negative net present value to shareholders. Another example is when a positive net present value project is simply not undertaken because the fraction of profits shareholders expect to be paid is too low to cover the up-front investment cost. In this case both insiders and outsiders are hurt from the inability of insiders to commit to not

expropriating outsiders. A further example of a negative net benefit caused by insider 'misbehavior' is the reduced liquidity of stocks caused by suspected insider trading. In summary, however, the finding of substantial abnormal stock returns is a necessary, but not sufficient, condition for a positive welfare effect.

In the next section, we describe prior theoretical views on the value of mandatory disclosure and outline a model that helps frame our empirical analysis. In section 3, we provide some historical background on disclosure regulation in U.S. equity markets, explain in more detail why the 1964 Amendments provides a unique opportunity to study the effects of mandatory disclosure, and discuss related empirical studies. Sections 4 and 5 describe our data and our empirical methodology, respectively. The main empirical results are presented and interpreted in section 6. The results of event studies of the effect of registration on stock price and liquidity are in Section 7. Section 8 concludes and discusses future research.

## 2 Theoretical Background

One traditional view of economists studying securities market regulation, often attributed to Stigler (1964) and Coase (1960), is that government intervention in securities markets is at best ineffective and, at worst, damaging. Proponents of this view argue that private contracts combined with the possibility of litigation, between shareholders on the one side and managers, underwriters, auditors, and analysts on the other, is a cost effective way to ensure proper conduct by the parties involved in securities market transactions. Such private enforcement works particularly well when those in possession of superior information are concerned about their reputation due to repeated interactions with the market. In such a setting, the better informed will voluntarily and truthfully disclose all relevant information to the point where additional information is not socially beneficial.

These views imply that firms efficiently limit the information they disclose. Thus, a firm's failure to provide some information must reflect a high costs of provision, lack of value relevance, or valid concerns that competitors may benefit from its release. Thus, mandatory disclosure legislation must either be inconsequential or cause firms to release a suboptimal amount of information.<sup>2</sup>

An alternative view posits that incomplete contracts and the high costs of enforcing private contracts may create an environment where mandatory disclosure regulation is welfare enhancing. There are at least three examples of why this may be the case. First, the cost of lawsuits induces

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<sup>2</sup>See Verrecchia (2001) for a review of the theoretical financial disclosure literatures in accounting, economics, and finance. Disclosure is also widely studied by legal scholars. See Easterbrook and Fischel (1984) for a discussion of the theoretical and empirical evidence in favor of mandatory national disclosure rules versus other forms of regulation aimed at preventing securities fraud.

a free rider problem among shareholders, whereas a regulator does not face such coordination problems in deciding whether to bring suit. Second, Glaeser and Shleifer (2003) argue that while regulations that mandate the costly provision of information may not be preferable on cost grounds, they are less vulnerable to subversion than litigation. The additional information provided under the regulatory regime implies a higher probability of detection, which assures compliance with lower fines. This contrasts with more cost efficient private litigation where misbehavior is detected with a lower probability and the necessary fines are correspondingly higher. In such cases the insiders sued have much larger incentives to bribe or subvert the process in some other way. Third, regulators may be more specialized and thus better at detecting misbehavior than judges.

Our analysis seeks to shed light on the net benefits to shareholders of an important piece of U.S. mandatory disclosure law, the 1964 Securities Acts Amendments. In a recent paper, Shleifer and Wolfenzon (2002) model financial markets when government regulation reduces insiders ability to divert firm resources to themselves. Their starting point is that private contracts cannot set the penalty for expropriation high enough to effectively deter malfeasance. The model is useful for our evaluation problem, because it illustrates the benefits to shareholders of reduced diversion. Furthermore, the model's penalty to insiders for getting caught stealing can be thought of more generally as a distortionary effect of misbehavior which reduces the total pie available to shareholders and insiders (examples of such distortions were given in the introduction). Finally, the general equilibrium version of the model illustrates possible additional welfare effects of the law.

As Shleifer and Wolfenzon point out, their model combines the basic economics of crime models (see, for example, Becker (1968)) with the basic agency models (Jensen and Meckling (1976)). The model, while quite simple, is consistent with a large set of empirical relationships between investor protection and securities markets. We briefly sketch a simplified version of the Shleifer and Wolfenzon (2002) model and state the relevant results, as we interpret them, that apply to our empirical analysis.

Shleifer and Wolfenzon (2002) consider an economy with many risk-neutral entrepreneurs.<sup>3</sup> In period 1, an entrepreneur ( $E$ ) starts a company and contributes  $R_E \leq W$  of her own funds to the firm. In return for fraction  $x$  of future cash flow, she raises  $R_M$  from the market and invests  $I = R_M + R_E$  in the company. In period 2, the company generates  $(1 + g)I$  of gross cash flow. Investments in an alternative security return  $1 + i$ . The entrepreneur can attempt to divert some of this cash flow to herself before paying the rest out as dividends.

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<sup>3</sup>The welfare implications of the model focus on new businesses, so we concentrate the discussion on "entrepreneurs." However, the distribution effects of the model apply just as well to other insiders, such as entrenched managers or majority shareholders, that may be able to divert resources to themselves.

Critically, any entrepreneur that can divert funds retains control of the firm regardless of how much she invests relative to outsiders.<sup>4</sup> The entrepreneur diverts a fraction,  $d$ , of cash flow to herself. She is caught with probability  $k^{non}$  if she does not make financial disclosures and  $k^{dis} > k^{non}$  if she does. If caught, she returns diverted funds and pays a fine that is increasing in the amount diverted. Let  $d^*(x^*, k)$  be the optimal diversion function for the entrepreneur given how much of the firm she chooses to keep ( $1 - x^*$ ) and the expected state of disclosure rules ( $k$ ).

Consider first the case of *ex post mandatory disclosure* – that is, the effects on a firm that is established in a period where disclosure is not mandatory and it is not expected to become mandatory. The period 1 price of the firm’s stock is based on the common expectation of  $d^*(x_{non}^*, k^{non})$  and the stock-market value of the firm is  $\frac{(1+g)I}{1+i}[1 - (1 - k^{non})d^*(x_{non}^*, k^{non})]$ , where  $x_{non}^*$  represents the portion of the cash flow rights the entrepreneur will sell if she expects to not have to make any financial disclosures. Now suppose that after the firm is set up in period 1, but before both the paying of dividends and any diversion in period 2, the government requires all entrepreneurs to make substantial financial disclosures. As a result, the entrepreneur now chooses (and the market expects) diversion of  $d^*(x_{non}^*, k^{dis})$ .

Under reasonable assumptions, it can be shown that the expected value of diversion to the entrepreneur is decreasing in  $k$ . This is for two reasons. First, the entrepreneur diverts less because of the lower expected benefit and higher expected cost of attempted diversion (that is,  $\frac{\partial d^*(x, k)}{\partial k} < 0$ ). Second, the probability of getting caught goes up. Given that  $k^{dis} > k^{non}$ , this suggests that diversion will go down after mandatory increases in financial disclosure. As a result of the change in shareholder disclosure rules, the value of the stock receives a one-time increase of  $\frac{(1+g)I}{1+i}[(1 - k^{non})d_{non}^* - (1 - k^{dis})d_{dis}^*]$  where  $d_{non}^* = d^*(x_{non}^*, k^{non})$  and  $d_{dis}^* = d^*(x_{non}^*, k^{dis})$ . That is, value increases by the discounted value of reduced diversion and increased recovery of diverted funds. In percentage terms, the increase is  $\frac{(k^{dis} - k^{non})d_{non}^* + (1 - k^{dis})(d_{non}^* - d_{dis}^*)}{[1 - (1 - k^{non})d_{non}^*]}$ .

This example of *ex post mandatory disclosure*, with an exogenous and unexpected change in disclosure policy, suggests the imposition of disclosure may generate positive abnormal excess returns for firms that were set up without expectation of mandatory disclosure and that were still operating when the law was enacted. While the existence of abnormal excess returns when mandated disclosure is imposed would certainly document that information provided is valuable, such a finding would not, by itself, document that increased disclosure improved welfare. Excess

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<sup>4</sup>Therefore, an efficient market for corporate control would take away the opportunity for diversion. Alternatively, we could assume the entrepreneur will always invest enough relative to outsiders that she always remains in control. In this case, in addition to the standard large shareholder/small shareholder conflict (see, for example, Burkart, Gromb and Panunzi (1997)) we would be allowing the large shareholder to attempt to engage in fund diversion that the government declares to be illegal.

returns could simply reflect a transfer of a fixed set of resources from one party (the entrepreneur) to another (the outside investor). However, it is also possible that reduced diversion by existing firms could generate welfare improvements. This would occur if the act of diversion is costly (e.g., entrepreneurs may only be able to keep a fraction of each diverted dollar) or if fines are deadweight losses.

Within the Shleifer-Wolfenzon framework, welfare gains could also be generated through the effect of *ex ante mandatory disclosure* on new investment projects that rely on financing from equity markets. Rather than welfare improvements coming from a reduction in wasteful diversion, they would stem from the fact that reduced opportunities for diversion can lead to more efficient investment in productive resources. Increased disclosure tilts entrepreneurs' incentives toward less diversion and increases their demand for outside funds. If the supply of funds is less than perfectly elastic, the increase in demand pushes up the equilibrium interest rate. This induces entrepreneurs with marginally profitable projects in the absence of disclosure regulation to not start their projects and instead invest their funds in higher quality projects of other entrepreneurs. As a result, projects with low  $g$  will not be funded and the average project that receives funding will be of higher quality (that is, will have higher  $g$ ). Because the average project is of a higher quality, investors gain. In the Shleifer and Wolfenzon (2002) model this welfare gain relies on imperfect capital mobility across countries.

Finally, it is important to bear in mind that expected diversion is reflected in stock prices under any of these regimes. So, while stock values, amount owned by insiders, and number of firms that go public are all affected by diversion, stock *returns* are not affected as long as the disclosure regime is constant. We do not expect any difference in returns between firms subject to mandatory disclosure and those not subject to disclosure either before or after the change in disclosure rules. Rather, we would expect the shift to mandatory disclosure to lead to a one-time gain in the price of firms that shift from not disclosing to disclosing.

### **3 Background on Federal Disclosure Regulations and a New Research Design**

Our empirical analysis examines the impact of the 1964 Securities Acts Amendments on the stock returns of affected firms. In the context of the previous section's model, we interpret this law as increasing the probability that diversion by insiders will be detected, or specifically as an increase from  $k^{non}$  to  $k^{dis}$ . This section provides a brief history of securities laws before the 1964 Amendments and how the structure of the legislation and the timing of its passage and implementation

provide a compelling research design to test for its effects. We also provide background on related empirical studies.

### **3.1 The 1933 Securities Act and 1934 Securities Exchange Act**

Prior to 1933, the Federal Government was essentially not involved in securities regulation.<sup>5</sup> Some states regulated brokerages, but there were no rules governing firms that issue securities. The only regulation of publicly traded firms came from rules imposed by securities exchanges. For example, the New York Stock Exchange (NYSE) required firms to show proper practices before they could list on the exchange and once listed they had to make important and regular disclosures to shareholders. However, the NYSE rules were enforced inconsistently. Further, there was essentially no regulation of firms traded Over the Counter (OTC). Although there were many examples of securities frauds both on and off exchanges prior to the Depression, proposals to add more Federal oversight were never seriously considered.

The 1933 Securities Act and 1934 Securities Exchange Act marked the first substantive federal regulation of securities markets. These Acts specified four forms of mandated disclosure that some publicly traded companies were required to comply with. As we detail below, the Acts' requirements did not apply equally to all categories of firms. The first type of mandated disclosure requires any firm making a new or secondary offering to file a registration statement and send a prospectus to purchasers. The registration and prospectus statements include a "Schedule A" with detailed financial information (including at least three years of balance sheet and income statement data) and descriptions of the firm's business, officers, costs of issuing the security, and intended use of any capital.

The second type of required disclosure is that firms listed on the NYSE and the American Stock Exchange (AMEX) must file annual (Form 10-K) and semi-annual (9-K) reports with the SEC, as well as report whenever a material event occurred (8-K). A 1936 law extended these periodic disclosure requirements to companies that issued securities through an initial or secondary offering after May 1936, regardless of whether they were traded on an exchange.<sup>6</sup> Consequently, periodic reporting was required of all listed firms and of those large OTC firms that had issued securities after May 1936.

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<sup>5</sup>This section draws heavily from Seligman (1995), as well as *New York Times* and *Wall Street Journal* articles from the 1960's.

<sup>6</sup>The specific requirement was that any company making a primary or secondary offering that increased the total market capitalization of all classes of securities issued by the company to above \$2 million had to file a registration statement and periodic reports with the SEC. Such a firm could cease filing these reports if and while the value of the newly issued security class issued dropped below \$1 million as of the last issue of the security.

The third type of mandated disclosure requires firms to provide proxy statements in advance of shareholder meetings or votes and sets minimum standards for information provided in these statements. In particular, these statements are required to report the qualifications of directors and nominees for directors, executive compensation, and transactions between the company and its officers or directors. The fourth type obliges firms to list the identities of officers, directors, and large shareholders. Further, they must report these individuals' holdings of the company's equities and provide monthly statements of any changes. The Act also allows companies to recover any profits that an insider realizes from the purchase and sale of the company's stock in any period of less than six months. The third and fourth types of mandated disclosure only applied to firms on exchanges, leaving all OTC firms uncovered.<sup>7</sup>

The 1933 and 1934 Acts were revolutionary forays into the regulation of securities markets, but their limitation was that all firms were not regulated equally. In particular, mandated disclosure requirements varied with firms' industry, the date they issued securities and whether they were listed on the NYSE or American Stock Exchange (AMEX). Harvard Law Professor Louis Loss (1983) wrote<sup>8</sup>:

For thirty years after enactment of the Exchange Act there was a double standard of investor protection – a standard that resulted, more by accident than by design, from the piecemeal adoption of the SEC statutes but that nevertheless glowed with an incandescent illogic: if an investor happened to be a stockholder of a listed company, or a public utility holding company or a subsidiary of such a company or an investment company, he had the protections afforded by the reporting requirements as well as (with some exceptions) the proxy rules and the insider-trading provisions. If, on the other hand, he happened to hold a security that did not fall within any of these categories but that had been offered to the public and registered under the 1933 Act since 1936, he was likely to have current information...[due] to the reporting requirements of the Exchange Act but not to the proxy or insider-trading provisions. The third investor, who held an unlisted security in an industrial corporation that had not had any public

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<sup>7</sup>The insurance and banking industries received preferential treatment under the 1933 and 1934 Acts. In the case of insurance companies, the legislation imposed slightly less burdensome periodic reporting requirements. The Acts “temporarily” exempted banks until appropriate disclosure rules could be determined. The rules were never written, however, and the temporary exemption lasted until 1964. Banks and insurance companies that chose to list on exchanges were not exempt from rules imposed by exchanges. It is noteworthy that most of these firms remained on the OTC market.

<sup>8</sup>Loss was President Kennedy's first choice to head the SEC. When Loss declined, Kennedy appointed William Cary.

financing since 1936, was still further from the fire; so far as he was concerned, the whole series of SEC statutes might just as well not have existed except for a few fraud provisions, no matter how large the corporation or how widely distributed its securities. (pp. 462-463)

Approximately three decades later the passage of the 1964 Securities Acts Amendments aimed to rectify the seemingly arbitrary inequities in regulatory intensity across categories of firms. It is these efforts that form the basis of this paper's examination of the consequences of mandated disclosure laws. Before we describe the exact form of these Amendments, the next subsection describes some of the consequences of the system of differential reporting requirements that prevailed for three decades.

### **3.2 The Impacts of Differential Reporting Requirements**

In response to complaints of fraud and manipulation on securities markets, Congress commissioned a study in 1961 of the operations of securities markets in the US. This study took over two years to complete and became known as the *Special Study of the Securities Markets*. It found that the existing system of differential reporting requirements had at least two undesirable effects on US equity markets.

First, the management of companies traded on the OTC market were not forthcoming with important details about firms' operations. For example, the *Special Study* said that roughly half of the complaint letters that the SEC received were from investors who either could not obtain information about a company in which they had invested or felt that the information sent to them was inadequate.

In a 1962 survey, the SEC randomly sampled one fifth of the OTC securities in which trades had been made during the last quarter of 1961. Of 1,965 OTC firms approached, 1,618 replied. 1227 had assets above \$1 million, of which about half had at least 500 shareholders. The survey found that:

1. More than a quarter of the firms did not provide any reports on the firms' financial position or results in that year.
2. In 73 percent of proxy solicitations for voting on the Board of Directors, the shareholders were not told the names of the nominees. The proxy solicitations listed the directors' qualifications in only 11 percent of the proxy solicitations. Thus, in the vast majority of cases, shareholders were asked to vote blindly for current management or their nominees.

3. In 95 percent of proxy solicitations, management compensation was not reported.
4. 29 percent of the firms did not solicit proxies before shareholder meetings.

These findings applied to all OTC firms. It seems reasonable to assume that the frequency of poor communication between management and shareholders were disproportionately concentrated among the firms that were completely free of SEC disclosure requirements.

Second, the legislative differences in mandated disclosure requirements appeared to distort firms' decisions on where to list their shares. The *Special Study* concluded that the system of differential regulation affected firms' decisions on where to list (*Special Study*, Part III, p. 16). Loss (1983) also argued that regulation affected listing decisions when he said, "For, just as surely as water flows downhill, business will move from a regulated to an unregulated market" (p. 464.) Prominent examples of firms that chose not to list on exchanges prior to the 1960's, but joined exchanges shortly before or after the Act took effect, included Time Inc., G.D. Searle, and Weyerhaeuser. Also, virtually all major banks remained unlisted until after the law was passed.

By all accounts, many companies that met the listing requirements of the more liquid New York and American Stock Exchanges chose to keep their shares on the OTC market, although this may not have been in shareholders' best interests. Despite repeated efforts, we were unable to determine the precise listing requirements of the NYSE and AMEX in this period. However, market capitalization has always been an important factor in determining eligibility for listing on both exchanges. In the subsequent analysis, there are 228 firms that were completely free of disclosure requirements before the 1964 Amendments. We were able to ascertain the market capitalization of 217 of these firms in the beginning of 1963. 184 of these firms, or roughly 85%, had a market capitalization that exceeded the 25th percentile market capitalization of firms on the AMEX and NYSE measured at the same time.

### **3.3 Negotiation and Passage of the 1964 Amendments**

We now trace out the time line of important events that led to the passage of the Securities Acts Amendments in August, 1964. In the subsequent analysis, the primary outcome variable is equity returns. Because equity markets are forward looking, any effect of the law should be concentrated in the period preceding its passage. Table 1 summarizes the key dates that are described in this subsection.

Before 1963, politicians and the public expressed little interest in revising the 1933 and 1934 Acts. On a fairly regular basis, SEC staff, a few legislators, or some other prominent public figure would call for an extension of the mandated reporting requirements to all OTC firms. However,

these recommendations never seemed to gain any traction in Congress (see Loss (1983), page 464.) For example, Senator Frear of Delaware introduced a bill to extend mandatory disclosure in 1949, but no action was taken because attention turned to higher priorities such as the Korean War (*Special Study*, part 3, page 7.) In 1961, the SEC's budget was increased and William Cary, the new head of the SEC, called for the agency to be granted new powers, including the extension of disclosure rules to all OTC securities. However, there was little evidence that the Kennedy Administration was willing to push for substantive legislative changes in its first year in power. In our examination of newspapers, we failed to find any evidence that market participants thought that Cary's calls foreshadowed substantive legal changes.

Also in 1961, there was a scandal on the American Stock Exchange. It was discovered that the company Re and Re offered special options grants to the individuals that determined whether the company could list on the AMEX. This scandal crystallized feelings that US securities markets were not fair and in September 1961, the Commission was authorized by Congress to "make a study and investigation of the adequacy, for the protection of investors, of the rules of . . . exchanges and national securities associations" (press release on SEC website). The mandate to the SEC was to examine the functioning of US equity markets generally – not necessarily with a focus on OTC firms. Based on our reading of newspapers from this period, it was far from certain whether the study was meant as more than a means to defuse public concerns about the operation of equity markets.

The first part of the *Special Study* was released in April, 1963 and it appeared to change the political climate. The *Special Study* was accompanied by a letter from William Cary stating that the SEC would make several legislative recommendations, including expanding the disclosure requirements for OTC securities. The Senate quickly reacted to this event. On July 9, 1963, a subcommittee unanimously approved a bill extending disclosure rules to all OTC firms. The release of the second installment of the *Special Study* on July 17, 1963 recommended a major overhaul of the OTC markets. Less than 2 weeks later on July 30, 1963, the full Senate passed the Securities Act Amendment which held OTC firms with at least \$1 million of assets and 500 shareholders to the same disclosure rules as the 1934 Act imposes on securities traded on the NYSE and AMEX.<sup>9</sup>

The bill moved on to the House, where it stalled. The size requirements were contentious. For example, some argued that there should not be an asset minimum and others thought that the shareholder floor should be lowered to 300. Further, insurance companies argued that they were

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<sup>9</sup>The staff that put together the *Special Study* argued that all OTC firms with at least 300 shareholders should be forced to comply with the mandated disclosure requirements imposed on firms on listed exchanges and that there should be no asset test. This would have forced approximately 5,500 firms of the roughly 25,000 OTC firms to comply. The cut-offs in the bill passed by the Senate were endorsed by the SEC commissioners.

already regulated by state insurance commissions so they should be exempted from the legislation. Banks contended that their supervision by the Comptroller of the Currency was sufficient, although the Comptroller had no mandated disclosure rules. While these issues proved difficult to resolve, the bill's general principle of increased disclosure was never seriously contested.<sup>10</sup>

The solution to the slowdown in the House came in late January and early February of 1964 when President Johnson made two public endorsements of the legislation before the House. Johnson applied his famous skills of persuasion and the controversy over the particular provisions began to evaporate.<sup>11</sup> A House subcommittee passed the bill on March 19, 1964. It was soon evident that it would become law and in May Cary announced that he would resign soon, noting that his work was complete. On August 5 and 6, 1964 the full House and Senate passed identical versions of the bill, thereby sending it on to Johnson who signed it into law on August 20, 1964.

The most important provision of the law was that it extended all four forms of mandated disclosure to new categories of firms. It specifically required that any firm that had at least 750 shareholders and \$1 million of assets as of the last day of its first fiscal year to end after July 1, 1964 (or any year after that) must register with the SEC within 120 days of the end of the fiscal year. In order to give companies more time to prepare their initial statements and to give themselves a chance to prepare for the onslaught of new filings, the SEC extended the deadline to April 30, 1965 for firms whose first fiscal year end was before the end of 1964. The first wave of new filings with the SEC took place in April, 1965, but first information disclosures were made as late as October 31, 1965. The compliance date for firms that met the asset test and had between 500 and 750 shareholders was the last day of its first fiscal year to end after July 1, 1966.<sup>12</sup> Firms with fewer than 500 shareholders and/or \$1 million in assets were unaffected by the 1964 Amendments.

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<sup>10</sup>We have found no public arguments against the Act on the grounds that disclosure was a bad idea. It was probably difficult to make such a case because, according to Seligman (1995), the available evidence seemed to indicate that the "vast majority of securities fraud occurred among firms not subject to the SEC's periodic reporting requirements" (p. 313-4). There was some resistance by business groups such as the US Chamber of Commerce and National Association of Manufacturers on the grounds that compliance would be costly and that business should be free of regulatory burdens. However, Edwin Etherington, the president of the American Stock Exchange, estimated the annual compliance costs at approximately \$1500 to \$3000 for most OTC companies that were to be covered by the bill and Congress seemed to find this estimate compelling.

<sup>11</sup>Largely due to the actions of House Commerce Committee Chairman Oren Harris, the securities bill was not seriously considered in the House until the Spring of 1964. Seligman (1995) suggests that Harris eventually helped enact the legislation in return for Johnson appointing Harris to be a Federal district judge (which Johnson did in 1965.)

<sup>12</sup>The *Special Study* survey of OTC firms indicates that, as of 1961, approximately 32% of OTC firms had enough assets and shareholders that they would be bound by the new disclosure rules initially with another 8% becoming bound two years later. These firms account for a substantial majority of all OTC firm assets.

### 3.4 The 1964 Amendments as a New Research Design

The structure and timing of the passage and implementation of the 1964 Amendments provide the framework for a compelling research design to evaluate the impacts of the 1964 Amendments. This subsection describes how the legislation affected different categories of firms and created three different periods in which we can test for the law’s effect and perform validation exercises of our approach.

*Firm Variation.* An appealing feature of the 1964 Amendments from an evaluation perspective is the availability of cross-sectional variation in which firms were affected by the change in mandated disclosure laws. We use the structure of the 1964 Amendments to divide firms into groups that were affected or unaffected by the law, based on their pre-legislation characteristics and whether they were already covered by the 1933 and 1934 Acts. We then impose a central assumption. Specifically, our analysis takes as a given that after adjustment for standard covariates (i.e., the overall market return and 2 Fama-French factors and a momentum factor), the unaffected groups are a valid counterfactual for the stock returns of the affected firms in the absence of the law.

The 1964 Amendments caused two groups of firms traded over-the-counter to become subject to the same four types of disclosure requirements as firms on the NYSE and AMEX. The first group consists of firms that switched from being free of all SEC reporting requirements to having to comply with *all four* types of disclosure. We refer to this as the “fully affected” or 0-4 group.<sup>13</sup> It is comprised of firms that only issued shares before 1936, or issued shares after 1936 but without bringing the total market value of the share class above \$2 million, had at least 500 shareholders, and had at least \$1 million in assets. The second affected group is labeled “partially affected” or 2-4. These firms were required to comply with the SEC registration and periodic reporting requirements before 1964 by virtue of having had a primary or secondary offering of shares since 1936 bringing the total market value of a share class above \$2 million, had at least 500 shareholders, and had at least \$1 million in assets. The 1964 Amendments compelled them to also comply with the proxy and insider trading types of disclosure. This group can be compared to the fully affected group to estimate the joint effect of the registration and periodic reporting requirements.

There are two groups of firms that were unaffected by the 1964 legislation. The first is the “unlisted unaffected” group. This group, which consists of firms that traded OTC at the time of the 1964 Amendments *and* that were not subject to any new mandatory disclosure as a result of the law, is actually comprised of two subgroups. The first subgroup is those firms that were free of

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<sup>13</sup>The number before the dash refers to the number of types of disclosure that the firm was required to comply with before the 1964 Amendments, while the number after the dash indicates the number of forms of disclosure that they were subject to after the new legislation was in force.

reporting requirements before and after the 1964 Amendments and are denoted as 0-0. These firms only issued shares before 1936, or issued shares after 1936 but without bringing the total market value of the share class above \$2 million, and fell below the 500 shareholders and/or \$1 million in assets floor. The second subgroup of the unlisted unaffected group is firms that were subject to the registration and periodic reporting requirements before *and* after the 1964 Amendments. This subgroup is labeled 2-2. These firms were below either the shareholders floor or assets floor of the 1964 Amendments.

The second unaffected group is comprised of the firms that trade on the NYSE and AMEX. This “listed unaffected” group was subject to all 4 forms of disclosure before and after 1964. These firms generally had higher market values than OTC firms. We therefore create two subsamples of the “listed unaffected” group that are intended to serve as unaffected comparisons for the fully affected and partially affected groups, respectively. Each of the subsamples are chosen to match one of the affected group’s median market capitalization. We describe the selection process further below. Table 2 summarizes the different affected and unaffected groups, as well as their disclosure requirements before and after the 1964 Amendments.<sup>14</sup>

Two details about our method for assigning individual firms to these groups bear mentioning. First, we use 1962 data on assets, shareholders, and filing status to assign firms to the various groups, which predates even the first serious discussions about passing the legislation that would become the 1964 Amendments. This is preferable to relying on 1963 or 1964 data or using firms’ post Amendments filing status, because we want the estimated impact of the law to be purged of any bias associated with firms endogenously choosing membership in the affected or unaffected groups.

Second by defining our affected group in advance of the filing date we have not conditioned on actual filing status. In light of the finding that many OTC firms appeared to have chosen to avoid the mandatory disclosure requirements by failing to move to a listed exchange, it is reasonable to suspect that some of these firms tried to avoid the Amendment’s requirements either legally (e.g., by reducing the number of shareholders below 500 or going private) or illegally (e.g., by refusing to comply).<sup>15</sup> Thus, our affected groups are comprised of firms that do and do not subsequently comply with the law. This is an advantage of our approach, because the policy parameter of interest for a country that is considering implementing mandatory disclosure laws must account for

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<sup>14</sup>Banks were treated like other firms in the 1964 Act, while insurance companies were exempted from mandatory disclosure. We currently exclude both groups because the rules imposed on them by other regulators make it difficult to determine how much information they disclosed before and after the the 1964 Act.

<sup>15</sup>It is also possible that some of the firms in our affected group fell below the asset or shareholder limits by the compliance date due to factors that are unrelated to the legislation.

the possibility that some firms will choose (and be able to) avoid the requirements. Put another way, an analysis that estimates the effect on firms that choose to comply would likely provide a biased estimate of the effect of a law that applied to a class of firms.

*Time Variation.* In the ideal event study, the event is a surprise to the affected parties. This is especially important when financial markets are used to study the impact of an event, because these markets are forward looking and quickly capitalize changes in expectations about the future. As the previous subsection documented, the 1964 Amendments were the result of a long process that can be traced back to at least the origins of the *Special Study* and perhaps longer. During this period, the probability that the disclosure requirements would be extended to uncovered OTC firms increased from some unknown level to something approaching one.

We define the event window as lasting from January 1, 1963 through August 24, 1964. Although the Special Study was released on April 3, 1963, we suspect that its basic findings may have begun to permeate the investment community earlier. How much earlier is unknown, so there is an element of arbitrariness in the choice of January 1. The choice of August 24, 1964 as an end date seems relatively uncontroversial. It is two weeks after Johnson signed the bill in order to allow full dissemination of the news in the population of investors.

We attempted to identify sub-periods when there were unexpected increases or decreases in the probability of the legislation's passage. The availability of such periods would allow for tighter hypotheses that could more easily be refuted by the data. Ultimately, we were unable to precisely identify true surprises so we cannot divide the event window into short periods of time where the probability of passage was increasing or decreasing.<sup>16</sup>

Thus, we test the null hypothesis that the fully affected and partially affected groups had zero abnormal excess returns in the period January 1, 1963 through August 24, 1964. A failure to reject this null hypothesis is consistent with the notion that on average the disclosure requirements were not expected to produce information that was valued by outside shareholders (after accounting for compliance costs), either because this information was already being transmitted without government interference or firms would evade compliance. A finding of positive abnormal excess returns among our affected groups would be consistent with the notion that the disclosure requirements

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<sup>16</sup>As an example, we initially suspected that Johnson's announcement that he supported the bill on January 22, 1964 represented a sharp increase in the probability of passage. That same day, Representative Harris made statements indicating the House would soon act on the bill. Upon closer inspection, however, we found that the *New York Times* had given relatively limited coverage to Johnson's announcement. The relevant article was printed on page 50 and highlighted the fact that Johnson had not allocated any new funds to help enforce the proposed legislation. By contrast, when Johnson more aggressively pushed for the bill as part of a February 5 "Special Message on Consumer Interests", the *Times* reported the endorsement prominently the next day and followed up with a February 9 story declaring that the endorsement had made the prospects for the bill "suddenly bright." Thus, it is unclear which actions surprised the markets so we cannot form a precise hypothesis to test.

increased the value of affected firms. In the context of the model in Section 2, this would be due to reduced scope for diversion by management.

We divide the period August 25, 1964 through December 31, 1966 into two periods, which are referred to hereafter as Periods 2 and 3, respectively. Period 3 runs from November 2, 1965 through the end of 1966 when our data file ends. We believe that this period provides the best opportunity to judge the validity of our approach. This is because there was no new information about the probability that the Amendments would be passed or enacted or about the financial position of the potentially affected firms in this period. Recall that firms' exact compliance date varied with their fiscal year end but all firms with more than 750 shareholders and \$1 million in assets were required to comply by the end of October 1965. Consequently, under the standard efficient markets hypothesis, a failure to reject the null of zero abnormal excess returns will provide support for our research design and the interpretation of the differential period 1 returns as being due to the 1964 Amendments.

Period 2 runs from August 25, 1964 through November 1, 1965. In this period, firms were required to begin to comply with the Amendment's requirements. As with Period 1, our focus is on a test of whether the affected groups have abnormal excess returns relative to the unaffected groups after adjustment for the standard factors. A rejection of the null of equal returns could have at least three interpretations. First if the market had incorrectly expected that the amendments would not be enforced (or that enforcement would be delayed, as it briefly was for a subset of firms), then prices may have been affected by the increasing likelihood that the law would be enforced as planned. If mandatory disclosure is valuable to shareholders, this scenario would imply a positive abnormal excess return. Second it is possible that as of the end of August 1964, the market had an incorrect estimate of the fraction of firms that would file or the mean financial strength of affected firms revealed in the registration statements. In this case, the affected groups could have a positive or negative abnormal excess return, depending on the nature of the revelations relative to expectations.

The third possibility is that the market correctly assumed that the law would be enacted and also had correct expectations about the fraction of firms in the affected group that would comply with the law. Here, we would expect zero abnormal excess returns. Under this assumption, a rejection of the null hypothesis would suggest that our approach is susceptible to falsely finding abnormal excess returns and this would invalidate our research design. Overall, the interpretation of the results for this period depends on one's initial assumptions.

### 3.5 Related Empirical Research

This is far from the first paper to study the effects of changes in Federal (and other) mandatory disclosure regulations in financial markets. Numerous previous researchers have studied the effects of the increased disclosure requirements of the Securities and Exchange Acts of 1933 and 1934.<sup>17</sup> Some of the better known work in this area was designed to influence the securities regulation policy debates of the early 1960's. Important examples include Stigler's (1964) attack on the need for securities market regulation. Stigler (1964) looked at new issues in the pre-SEC and post-SEC periods. Analyzing the average price and the variance in prices across issues, he concluded that the laws had no effect on stock prices and lowered variance only by discouraging high variance issues from coming to market. Friend and Herman (1964) and Robbins and Werner (1964) defended the SEC and argued that Stigler (1964) could not effectively separate effects of the 1933 and 1934 Acts from changes in the market overall.

Later, Benston (1973) took advantage of the fact that the 1934 law required NYSE firms to disclose their sales figures and that, prior to the law, some firms already disclosed this information while others did not. While Benston (1973) did have a comparison group, disclosure of this one piece of information might not be expected to have a large effect on stock performance. Possibly due to this weakness in his experiment, Benston (1973) found no effect of the 1934 law on volatility. Simon (1989) provided the strongest evidence that the early SEC Acts had a positive effect on stock markets. Using data from 1926-1940, she showed that issue-specific risk was significantly lower in the post-1933 period. She also compared IPO's in bull markets before 1933 to those after 1933 and exploited differences in pre-1933 disclosure rules for NYSE and other exchanges. Simon (1989) focused on risk for new issues and did not measure changes to price and volatility of existing issues. Because of data limitations, no study of the 1930's legislation has used the same empirical approach that we use to study the impact differential impact on exchange-listed firms (the affected group of the 1930's legislation) and OTC firms (the unaffected group at that time.) There may have been relatively little impact, given that exchanges already had some disclosure requirements. However, there is no reason such an undertaking could not be conducted by researchers who can gather appropriate OTC data from that time.

There is a large literature studying the effects of other changes in financial disclosure.<sup>18</sup> As

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<sup>17</sup>Table 3 provides an overview of the samples, measures, and findings of several relevant papers studying the 1930's Securities Acts and other changes in disclosure.

<sup>18</sup>See Healy and Palepu (2001) for a review of the empirical financial disclosure literature. There is also a substantial body of work on the effects of disclosure in non-financial markets. A recent example is Jin and Leslie (2003) who study the effect of mandatory disclosure of restaurant health inspection scores using methodology similar to ours.

noted by Leuz and Verrecchia (2000), the empirical evidence on the effects of disclosure is not strong or entirely consistent. In motivating their study of disclosure in Germany, they argue that disclosure changes outside the United States provide better opportunities to study large changes. We take a different strategy in finding an important change in disclosure regulation – rather than go outside the U.S., we go back in time.<sup>19</sup> Our analysis also differs from Leuz and Verrecchia (2000), as well as many other papers in the disclosure literature, in that we focus primarily on a case of mandatory disclosure.

Bushee and Leuz (2002) take an approach similar to ours by analyzing a noteworthy change in SEC disclosure regulation and by using a control group of unaffected securities. They study the OTC Bulletin Board (OTCBB), where rules similar to the 1934 Securities Act were enforced beginning in 1999. Prior to the rule change, approximately two-thirds of OTCBB firms were too small to be subject to SEC disclosure rules. That is, they were part of the “unlisted unaffected” group after the 1964 Amendments. Bushee and Leuz (2002) find that increased disclosure led to increased liquidity (as proxied by share turnover, trading volume, number of days where trades took place, and number of market makers) and decreased cost of capital. There are two important differences between our study and Bushee and Leuz (2002). First, OTCBB firms had more discretion over whether or not to adopt the new rules relative to most of the firms we study in 1964 because they could choose to leave the OTCBB for less liquid markets. In fact, approximately three-quarters of OTCBB firms who were newly required to file with the SEC chose to delist from the OTCBB. One important finding of Bushee and Leuz (2002), therefore, is that many firms have a strong preference for avoiding SEC disclosure requirements. The option to delist limits Bushee and Leuz’s (2002) ability to study price and volatility effects of changes in disclosure because not that many firms actually changed their level of disclosure and because any market reaction is probably as much a reaction to the firms’ decisions regarding disclosure as to disclosure itself. Secondly, the OTCBB is a trivial portion of the U.S. equity market. The average and median *nominal* market capitalization of affected firms in the Bushee and Leuz (2002) sample is smaller than these same statistics for affected groups in our sample, despite forty years difference.<sup>20</sup>

To our knowledge, Ferrell (2003) is the only other empirical analysis of the 1964 Securities Acts Amendments. Ferrell (2003) also studies the effect of the 1964 law on OTC companies. Ferrell (2003) focuses on volatility and correlation between individual firms and the overall market

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<sup>19</sup>See Butler, Kraft and Weiss (2003) for another paper that uses SEC changes in the mid-1900’s to examine the effects of disclosure. They analyze SEC rule changes in 1955 and 1970 that forced exchange-listed firms to provide more frequent financial reports.

<sup>20</sup>The Dow Jones Industrial Average increased fourteen-fold in the time between the beginning of our sample and the beginning of Bushee and Leuz’s (2002).

(synchronicity). He finds that OTC volatility is substantially lower in 1965-1967 than 1962-1964, but that synchronicity was unaffected. He also argues that the 1964 Amendments had little effect on stock returns. Our results are quite different, which appears to be a result of our more precise assignment of OTC firms to different groups and to our focus on a shorter event window. Ferrell focuses on firms from Barrons' primary list of OTC firms, leaving out the (on average smaller firms) from Barrons' supplemental section and uses all listed firms as a comparison group. We use firms from both Barrons' lists and Ferrell's sample is thus a subsample of ours. Ferrell argues that most of the firms on the primary list newly had to comply with all four disclosure requirements of the 1964 Amendments. However, detailed firm lists in annual SEC publications show that about 73 percent of firms on the primary list already complied with the initial registration requirement and periodic reporting of financials as of 1963.<sup>21</sup>

Lo (2003) also takes an approach similar to ours in his analysis of the SEC's 1992 extension of executive compensation information that had to be disclosed. He studies how market values changed in the eight months from the SEC's initial announcement that they were considering an extension until the detailed rules became known. He forms relatively affected and unaffected groups by using the amount of lobbying firms did to influence the SEC. Lo (2003) finds that increased disclosure created shareholder value, which is consistent with a reduction in diversion.

Disclosure rules are not the only means by which regulators attempt to make markets more efficient. There is a literature that compares securities laws and institutions across countries to determine what factors are most important in making a country's markets efficient. A recent example is Porta, de Silanes and Shleifer (2002), who study securities laws and markets in 49 countries. They argue that more developed and efficient stock markets are associated with mandatory disclosure and with holding firms that issue securities, as well as their investment advisors and accountants, liable for misleading information. They find no evidence that criminal penalties for financial misdeeds affect market development. Glaeser, Johnson and Shleifer (2001) draw similar conclusions from a comparison of development of the Czech and Polish stock markets. These studies provide useful general rules for what factors are associated with well developed markets. However, they generally cannot evaluate specific disclosure policy choices because they do not distinguish clearly between the effects of various types of disclosure, they have very coarse measures of market efficiency, and they do not look at any within-country changes.

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<sup>21</sup>Furthermore, about 13 percent of firms on the primary list were too small to have data for assets and shareholders in Moody's or other publicly available sources and were thus less likely to become affected by the 1964 Amendments.

## 4 Data

### 4.1 Data Sources

To implement our analysis, we created the first electronic data set containing information on securities traded Over the Counter (OTC) from 1963-1966. This database contains information on bid and ask share prices, dividends, stock splits, whether the firms disclosed information to the SEC, and financial information for the subset of firms for which this was available. The forthcoming Data Appendix will contain more details. We also use similar information for equities traded on the American and New York Stock Exchanges (from CRSP and Compustat).

The basis of the analysis is a comparison of weekly returns across different categories of firms from 1963-66. The Center for Research in Security Prices (CRSP<sup>®</sup>) database, which is the standard source for historical equity returns data, only begins coverage of OTC firms in December 1972. Therefore, we created our own database of OTC firms' returns.

The resulting database is based upon the hand entry of data from 8 separate sources on OTC firms and the CRSP electronic data file. We collected weekly bid and ask prices for OTC companies from 1963-66 weekly publications of *Barron's*. The relevant pages of this publication were photocopied and these copies were then scanned. The image files were sent to Mascon Computer Services (P)Ltd. of India. They hand entered the security name, the bid price, and the ask price from each issue of *Barron's*.

To create a panel data file of prices, we matched firms across issues of *Barron's* by using their reported names. Elaborate checking was performed to minimize spurious exit of firms in cases where name abbreviations differed slightly between Barrons issues.<sup>22</sup>

We hand entered all dividend and stock split information from *Standard and Poor's Annual Dividend Record*. In particular, we started with a list of all the names of the firms in *Barron's* for each year and entered the amounts and dates of every cash, property, and liquidating dividend and stock split. The S&P publication is an ideal data source for this information, because it aims to be a "record of dividend payments on virtually every American and Canadian preferred and common stock."<sup>23</sup> Approximately 80% of the firms in our sample of OTC firms were in the S&P book. We

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<sup>22</sup>To avoid incorrect matches, we also assessed the quality of the match by checking for implausibly large changes in the bid and ask prices between *Barron's* issues. The roughly 500 largest weekly changes in price were investigated by research assistants in the United States. These investigations included rechecking the Barron's entries and the stock split information, as well as verifying the match between firm names across issues of *Barron's*. Any documented errors were corrected. Otherwise, the observation was unchanged.

<sup>23</sup>The average number of firms covered in this publication during the 1961-68 period was 10,000. Notably, there were roughly 2,000 firms on the American and New York Stock Exchanges in these years, so approximately 80% of the entries were for OTC (and Canadian) firms.

also used the *National Stock Summary* and the *Directory of Obsolete Securities* to verify dividends, especially in cases where firms ceased operations.

These price and dividend data were then used to calculate the weekly returns for each company. The first step in this process was to adjust the stock prices for all stock splits. We adjusted the dividends analogously. The returns were then calculated as the change in the mean of the bid and ask price between consecutive weeks plus any cash dividends divided by the mean of the bid and ask price in the first of those weeks in 1963-66.

Securities that disappeared from *Barron's* posed a particular challenge. In these cases, the first step was to ascertain the reason that the firm's share price was no longer reported in *Barron's*. We relied on a myriad of sources for this task, including the *Standard and Poor's Annual Dividend Record*, *National Stock Summary* and the *Directory of Obsolete Securities*. For securities that exited *Barron's* due to mergers, name changes, liquidations, or bankruptcies, we used the *National Stock Summary* and the *Directory of Obsolete Securities* to generate "delisting" returns similar to procedures used by CRSP when a firm leaves their sample. For firms that moved to the NYSE or AMEX, we used *CRSP* to continue the time series of prices. The forthcoming Data Appendix will provide further details on how returns were calculated for firms that disappeared from *Barron's*.

An essential component of the analysis is the accurate identification of the firms that disclose financial information through official SEC channels. The *Directory of Companies Filing Annual Reports with the SEC Under the Securities Exchange Act of 1934* reveal the identities of each company that complies with mandated disclosure requirements. Each company's filing status was collected annually and matched to our sample of *Barron's* firms. The *US SEC News Digest* reveals when a company files a new type of information with the SEC for the first time. That information is used in the event study part of our analysis.

We gathered accounting information on the firms in the *Barron's* sample from *Moody's Industrial Manual*, *Moody's Public Utility Manual*, *Moody's Bank and Financial Manual*, and *Moody's Transportation Manual*. These manuals were published annually. For most companies, the manuals contain fairly detailed income statement and balance sheet data, information on directors and managers, and other details. Some companies did not disclose this information, however, so there is a substantial fraction of the firms for which some or all of this information was not available.

## 4.2 Sample Details

Our primary OTC sample is comprised of the 1,251 OTC firms that appeared in the January 7, 1963 issue of *Barron's*.<sup>24</sup> We form a panel data file of these companies that runs through the end of 1966. These firms are then divided into the affected and unaffected groups to form equally weighted portfolios. We do not add new firms to the portfolios because our focus is on the effect of changing the mandated disclosure requirements on an existing set of firms.<sup>25</sup> Here, we provide some summary statistics on this panel and describe how the firms are divided into the affected and unaffected groups.

Table 3 details the attrition from the *Barron's* sample, starting at the beginning of 1963. The weekly return for each portfolio is based on the firms for which we were able to ascertain a price that week. In cases where the firm disappears from *Barron's* and we cannot continue the series, it is assigned a delisting return in the first week that a price cannot be located. For all subsequent weeks, it does not enter the calculation of the portfolio's return. This is equivalent to assuming that the missing firms' return in the subsequent weeks are equal to the mean return of the remaining firms.

The first row of the table reports the sample size for the week of 1963, 1964, 1965, and 1966, as well as the final week of the panel (week 52 of 1966). Rows (a) through (f) report the 6 different reasons that firms leave *Barron's* and the numbers that leave for each of these reasons between each of the dates. For each exit reason, the table presents the number of firms for which we assigned a delisting return upon the exit from *Barron's* and whether we were able to continue the series. The largest single source of attrition from the sample is firms that move to an exchange. Over the course of four years, roughly 21% of the firms move to the NYSE or the AMEX. The movement to exchanges is especially high in 1963 and 1964, slows somewhat in 1965, and slows even more in 1966. This foreshadows our results in that as mandatory disclosure began to look more likely, the value of staying unlisted declined. The other forms of attrition were smaller and of a roughly equal number across years.

## 4.3 Composition of the Affected and Unaffected Groups

To conduct the analysis, each of the 1,251 *Barron's* OTC firms was assigned to either the fully affected, partially affected, unlisted unaffected, or listed affected group. Because it is likely that

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<sup>24</sup>We drop listings for B shares and preferred shares to have a uniform set of stocks, along with listings for banks and insurance companies.

<sup>25</sup>Another interesting question is whether the 1964 Act changed private firms' decisions on where to have its shares trade upon going public. This is beyond the scope of the current paper.

some firms attempted to evade the mandated disclosure rules either by going private or altering their assets or the number of shareholders, an analysis of the firms that comply with the disclosure requirements would suffer from classic selection bias.<sup>26</sup> Consequently, we assign firms to three groups based on characteristics in 1962, which predates the event window. This assignment rule means that the affected group will include firms that complied with the 1964 Amendments and some that evaded their requirements. However, our estimated effect of the law is the policy parameter of interest because it allows for the inevitable efforts to evade the disclosure requirements.

In assigning the *Barron's* firms to the groups we used 1962 information to mimic the statutory requirements of the 1964 Amendments. This was complicated by the absence of information on the number of shareholders and/or assets for a number of firms. The fully affected group is comprised of firms that did not file with the SEC in 1963 and had measured assets in 1962 exceeding \$1 million and more than 500 shareholders or had measured assets exceeding \$5 million but no shareholder data.<sup>27</sup> The partially affected group includes firms that filed with the SEC in 1963 and had measured assets in 1962 exceeding \$1 million and more than 500 shareholders or had measured assets exceeding \$5 million but no shareholder data. Firms that did not file in 1963 and fell below the asset or shareholder floors (including those with both assets and shareholder data missing) were assigned to the unlisted unaffected group, as were firms that filed with the SEC in 1963 and fell below the asset or shareholder floor or were missing data on both variables.

We created two other unaffected groups from the AMEX and NYSE firms, both of which had no change in filing status. The first is labeled “listed unaffected with market capitalization  $\leq$  \$35 million.” It is comprised of all AMEX and NYSE firms with market values less than \$35 million at the beginning of 1963. This cut-off was chosen because the median market capitalization of this group is \$9 million, which is the same median market capitalization of the fully affected group. Thus, this listed unaffected group is used as a counterfactual for the fully affected group. Analogously, the “unlisted unaffected with market capitalization  $\leq$  \$45 million” group is chosen to match the \$10 million median market capitalization of the partially affected group and is used as a counterfactual for this group. These two unaffected groups are a small fraction of the overall listed markets. The “unlisted unaffected with market capitalization  $\leq$  \$36 million” group comprised 3.6% of CRSP market capitalization at the beginning of 1963, while the “ $\leq$  \$45 million” group

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<sup>26</sup> Another negative consequence of defining the groups based on post-1964 observed filing status is that this would induce a mechanical correlation between filing status and returns in the 1963-64 period. This is because firms with high returns in 1963-4 will have to file simply because their high returns are likely to push them above the asset and shareholder thresholds.

<sup>27</sup> There are no cases where shareholder information is available but assets are not available. Results are not sensitive to changing the asset cutoff used for firms with no shareholder information.

comprised 4.8% of CRSP market value.

#### 4.4 Attrition and Filing Status by Groups

Panel A of Table 4 displays the sample sizes of each of the five groups (there are two listed unaffected groups, one to match each affected group) of firms at the start of our analysis, as well as showing the attrition from each group over the analyzed period. The fully affected group has 228 firms in the beginning of 1963. This group is considerably smaller than the partially affected group because many firms that were trading in the OTC market as of 1963 had raised equity in the previous twenty-seven years. Sample attrition is similar among these two groups, with approximately 80% of each group still trading as a stand-alone firm at the end of 1966. Sample attrition is much higher in the unaffected OTC group.

Panel B of Table 4 shows the annual SEC filing status of firms in the initial *Barron's* sample. It is evident that the law affected the filing status of the fully affected group. In particular, the proportion of these firms that filed in 1963 increased from 0% to 76.8% in 1966. Because our assignment rule is based on 1962 information and actual filing status is based on 1964 information, it is to be expected that some of our affected groups do not file after the Amendments become law and that some firms in the unaffected group do file. The table demonstrates that this is in fact the case. However, there is a sharp difference between the proportion of “fully affected” firms that actually file by 1966 (76.8%) and that portion of the “unlisted unaffected” firms that did not file before the law (32.0%). It is evident that our classifications into these two groups is highly correlated with eventual disclosure status.

#### 4.5 1962 Characteristics of Groups.

Table 5 reports the means of a number of important variables at the end of 1962, by group. It also reports the number of firms with nonmissing observations for each variable by group. The first panel reports the mean and median stock market capitalization and mean share price for each of the groups. The entries suggest that the market capitalization of the fully affected and partially affected groups is roughly an order of magnitude greater than the market cap for the unaffected group. The reliability of this finding is limited by the small number of firms with nonmissing market capitalization data in the unaffected groups. However, it has been noted elsewhere that the price per share is correlated with market capitalization and if this holds true here then the affected firms are indeed larger.

The rest of the table reports on a series of firm-level variables also collected from Moody's annual handbooks. Many of these variables are missing for a substantial fraction of OTC firms. This

highlights the difficulty that current and prospective shareholders of OTC firms faced in obtaining relatively basic information about these companies before the 1964 Amendments. Further, it underscores that this legislation was likely to substantially increase the level of information available about these firms. As with the market capitalization variable, the affected firms are much larger than the unaffected firms by all measures.

## 5 Empirical Methodology

Our analysis compares the returns to shareholders of firms affected and unaffected by the 1964 Amendments' disclosure requirements. Here, we discuss the econometric models used to estimate whether abnormal excess returns accrued to shareholders of affected firms.

We begin by constructing a time series of the equal weighted portfolio returns for each of our groups. This is denoted as:

$$R_{gt} = \frac{1}{N} \sum_{i=1}^N R_{igt} = \frac{1}{N} \sum_{i=1}^N (P_{igt} - P_{igt-1} + D_{igt}) / P_{igt-1} \quad (1)$$

where  $R_{gt}$  is the return for holding the group  $g$  portfolio from the end of week  $t - 1$  to the end of week  $t$ . The return for each firm in the portfolio,  $R_{igt}$ , is calculated as the change in the price per share at the ends of week  $t$  and  $t - 1$  (i.e.,  $P_{igt} - P_{igt-1}$ ) plus any dividends paid between the price observations (i.e.,  $D_{igt}$ ) all divided by the price at the end of week  $t - 1$ .  $N$  is the number of firms in each group in that week. Because the market capitalization for a number of the firms in the sample is missing, we cannot calculate the value weighted return.

It is standard to model the returns of any of the groups using a four factor model of the form:

$$R_{gt} - R_{ft} = \alpha_g + \beta_{1g}(R_{mt} - R_{ft}) + \beta_{2g}SMB_t + \beta_{3g}HML_t + \beta_{4g}MOM_t + \varepsilon_{gt}, \quad \varepsilon_{gt} = \lambda_t + \nu_{gt} \quad (2)$$

where  $R_{ft}$  is the return on a risk-free asset, which is measured as the Treasury bill rate. This equation controls for the difference between market and risk-free returns, two factors based on Fama and French (1993), and a momentum factor based on Carhart (1997). The market return is measured as the value weighted CRSP return that uses both NYSE and AMEX firms. The three factors are measured as the difference in the returns of portfolios of small and large stocks (SMB), the difference in returns of portfolios of value and growth stocks (HML), and the difference in returns of portfolios of stocks with high and low returns over the period from two to twelve

months prior to the current date.<sup>28</sup> These three factors are obtained with NYSE and AMEX data so the absence of market capitalization and accounting data for all the OTC firms does not pose a problem for estimation.<sup>29</sup> The  $g$  subscript on the parameters underscores that this equation can be estimated separately for each of our groups and that the effect of the factors can vary across groups.  $\varepsilon_{gt}$  is the unobserved determinant of group  $g$ 's return and is composed of a time-specific factor (i.e.,  $\lambda_t$ ) and an idiosyncratic factor (i.e.,  $\nu_{gt}$ ).

In this setting,  $\alpha_g$  measures the abnormal excess returns, specific to group  $g$ . The appeal of this measure of abnormal returns is that it has been purged of any covariance with the overall market and with the three factors. This is important in our context because our groups may have high or low returns in the examined period simply because of their riskiness relative to the market or because they are disproportionately comprised of small, value, and/or high momentum firms. In general, it is rare to find instances where groups of stocks or portfolios have statistically significant abnormal excess returns based on the above model and it this provides a useful performance benchmark.

An important limitation of equation (2) is that it is impossible to separately identify  $\alpha_g$  and  $\lambda_t$ , so the estimated  $\alpha_g$  may measure the  $g$ 's abnormal excess return *and* time varying factors that are common to multiple groups. For instance, suppose there was a positive shock to the returns of all OTC firms during one of the periods we study. Then the estimated  $\alpha_{fa}$ , where “ $fa$ ” indicates fully affected, would capture the effect of the 1964 Amendments *and* the shock. In this case, it would be invalid to interpret the estimated  $\alpha_{fa}$  as a causal estimate of the abnormal excess returns for the fully affected group. Because of these limitations, we present our estimates of Equation (2) in Appendix Table 1. We focus our discussion and our primary tables on analyses that control for time shocks across portfolios.

In light of this possibility, a compelling feature of our research design is that the unaffected groups can be used to purge the estimated  $\alpha_g$ 's of these common time factors. This is accomplished by differencing equation (2) for an affected and unaffected groups. An example of special interest is the difference between the fully affected group and the unlisted unaffected (“uu”) group, which becomes:

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<sup>28</sup>Kenneth French generously provided us with the SMB and HML daily series and extended them back an extra year so we can perform analysis through all of 1963. Using daily return data, we calculated the weekly momentum series based on the formulas on French's web page ([http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data\\_library.html](http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html).)

<sup>29</sup>Another advantage of using SMB, HML, and MOM series calculated with NYSE and AMEX data is that all of these firms are listed and unaffected. So, these portfolios do not reflect the effect of the 1964 Amendments.

$$\begin{aligned}
R_{fa,t} - R_{uu,t} = & (\alpha_{fa} - \alpha_{uu}) + (\beta_{1,fa} - \beta_{1,uu})(R_{mt} - R_{ft}) + (\beta_{2,fa} - \beta_{2,uu})SMB_t \\
& + (\beta_{3,fa} - \beta_{3,uu})HML_t + (\beta_{4,fa} - \beta_{4,uu})MOM_t + (\nu_{fa,t} - \nu_{uu,t}) \quad (3)
\end{aligned}$$

Note that  $\lambda_t$  has been differenced out of equation (3), so time varying factors common to the two groups cannot bias the estimated parameters. The estimated  $\beta$ 's are the difference in the loadings between the fully affected and unlisted unaffected groups. The parameter of interest is  $\alpha_{fa} - \alpha_{uu}$ , which is interpreted as the difference in the abnormal excess returns between the fully affected and unlisted unaffected groups. The null hypothesis is that this difference is equal to zero. An important feature of our research design is that it divides the 1963-1966 period into three periods. We fit equation (3) separately in each of these periods.

A few other features of our approach merit highlighting. First, we exploit the availability of the two unaffected groups (listed/unlisted) to present two different estimates of the effect of the law on each of the affected groups. It is not evident which of these groups is the more appropriate control group, so the presentation of both estimates allows for an examination of the sensitivity of the results to alternative choices.

Second, to the extent that there are differences in the industry composition of the affected and unaffected groups, the estimated abnormal excess return from the fitting of equation (3) may be biased by industry-specific shocks that are unrelated to the law. To probe this possibility, we also present results where the weekly returns for the unaffected groups are calculated with the industry weights of the relevant affected groups. Third, the estimation of equation (3) with group by week data, rather than firm by week data, is a conservative solution to the likely correlation in unobserved returns across firms within the same week.

Fourth, our primary estimates are average abnormal excess returns so they are equivalent to a portfolio that is rebalanced every week to ensure an equal weighting across all firms in the portfolio. The shortcoming of this approach is that it does not mimic the buy and hold strategy that many investors employ. Its advantages are that it is possible to control for the factors without accounting information on OTC firms and that normality assumptions are closer to being valid for average abnormal excess returns than for buy and hold returns (see Fama (1998)). Nevertheless, we also present results when the weights on a given stock are allowed to evolve based on its returns since the construction of the group or portfolio.

Fifth, equation (3) relies on estimated  $\beta$ 's that are obtained from data during each of the three periods. This is a valid approach if the law does not affect the  $\beta$ 's. It would be inappropriate,

however, if the policy changed investors' expectations of the  $\beta$ 's in the affected group, because we would be using endogenously determined  $\beta$ 's to estimate the abnormal excess return. In the next draft we plan to also report results from Periods 1 and 2 when the  $\beta$ 's are estimated from Period 3 and to calculate policy effects under different assumptions about whether the changes in  $\beta$ 's is or is not due to the 1964 Amendments.

## 6 Results

### 6.1 Unadjusted Returns

We begin with a graphical analysis of the unadjusted returns by group. Figure 1 depicts the unadjusted cumulative returns for the fully affected group, the unlisted unaffected group, and the matched listed unaffected group. Thus, each data point represents the current week's return for each group added to the sum of the return in all previous weeks. The vertical lines denote divisions between each of the three periods. During period 1, the fully affected and listed unaffected groups each had cumulative returns of approximately 20% (a bit lower for the listed unaffected group). In contrast, the cumulative return was approximately negative five percent for the unlisted unaffected group. The figure also shows that unadjusted returns are more similar for each of these three groups in periods 2 and 3.

Figure 2 performs a similar analysis for the partially affected group. The figure shows that the partially affected group outperformed the unlisted unaffected group during period 1, but that the partially affected and unlisted unaffected groups had comparable returns. The results for periods 2 and 3 are also similar to those in Figure 1. Specifically, the three groups seem to follow more similar patterns in their returns throughout 1965 and 1966.

Overall, these figures provide an initial examination of the effects of the 1964 Amendments. In period 1, the plots imply that the passage of the 1964 Amendments led to higher returns when the affected groups are compared to the unlisted unaffected group, but there is only small effects when the comparisons are to the matched listed unaffected groups. The next subsection will explore the robustness of these preliminary findings to adjustment for the standard determinants of stock returns. Notably, the excess returns of affected firms relative to those of listed unaffected firms become larger once we control for the four factors.

### 6.2 Abnormal Excess Returns

We now turn to estimates of the adjusted, or abnormal excess returns for each of the three periods. Table 6A presents the results from testing for abnormal excess returns for the fully affected group.

In the first and second panels, the unaffected groups are the listed unaffected and unlisted unaffected groups, respectively. In the third panel, the dependent variable is the difference between the returns of the fully affected and partially affected groups. In principle, the results in this last panel identify the separate effect of the registration and periodic reporting requirements, while the first two panels identify the effect of all four disclosure requirements. For this interpretation to be valid, it is necessary to assume that the effect of these two forms of disclosure are additive and separable.

Each column reports the abnormal excess return (i.e., the parameter on the constant) from the estimation of alternative versions of equation (3). The specification details are reported in the bottom rows of the table. In the first column, the only adjustment is for the overall market return. The second column adds the three other factors to the specification. In the third column, we account for the differences in the industry composition of the affected and unaffected groups by recalculating the unaffected group returns with the week-specific industry weights of the fully affected group. In the first three columns, we have implicitly assumed that investors in our constructed portfolios, or groups, rebalance the equal-weighted portfolios every week. Since it is unlikely that investors would follow such an investment strategy, the fourth column reports results from a “buy and hold” strategy. Specifically, we start by putting an equal dollar value in each stock of the portfolio. In calculating the portfolio return from date  $t$  to  $t + 1$ , we assume that the share invested in stock  $i$  equals (value of investment in stock  $i$  at  $t$ )/(total value of portfolio at  $t$ ) where the investors is now assumed to make no trades between the initial portfolio formation and date  $t$ .

The first two panels suggest that the fully affected group had significant abnormal excess returns in Period 1. When the listed unaffected group is used to difference out the time effect, the estimated abnormal excess weekly return is similar across all four specifications and ranges from 0.17% to 0.22%. Notably, all of these estimates would be judged to be statistically significant by conventional criteria. Because there are 85 weeks in this period, these estimates indicate that by the end of the period the fully affected group’s cumulative abnormal excess return was approximately 14-18%. In the second panel, where the unlisted unaffected group is the comparison group, the cumulative abnormal excess return ranges from 21% to 26%. The similarity of the estimates across specifications and between the two unaffected groups is reassuring. In the context of the Shleifer-Wolfenzon model, these results suggest that shareholders expect the present discounted value of future dividends to increase by 14-26% due to reduced insider diversion of profits or lack of effort.

In the third panel, the point estimates range from 0.08% to 0.15% and are generally marginally significant when judged by conventional criteria. Under the additive separability assumption, the findings suggest that the registration and periodic reporting requirements are responsible for a cumulative abnormal excess return of 7% to 13% during period 1.

The fit of the regressions measured by the R-squared statistic in all three panels is lower than is generally associated with the four factor model. There are two explanation for this poor fit. First, the regression is for excess returns rather than returns. Second, there is a weaker covariance between the returns of OTC firms and the standard four factors.<sup>30</sup>

Panel B of Table 6 repeats this analysis for the partially affected group. These results provide direct evidence on the effect of compliance with the proxy and insider trading types of mandated disclosure. All of the estimates of  $\alpha$  are positive. They range between 0.080% and 0.23% and 3 of 8 would be judged statistically significant by conventional criteria. The estimates are smaller when the listed unaffected group is used for comparison. Overall, these estimates indicate that the 1964 Amendments are associated with a cumulative abnormal excess return of 7% to 20% for the partially affected group in period 1.

Table 7 is identical to Table 6, except that it reports the estimated  $\alpha$ 's from fitting equation (2) in period 3. Recall, period 3 is after the law has passed and the vast majority of new filers have registered with the SEC. Thus, there is no new information about the law or about who will comply with its requirements. Consequently, we expect that the groups, which were constructed based on whether they would be required to comply with the law, should not predict returns. Specifically, we expect that neither of our affected groups will have abnormal excess returns in this period. If this null hypothesis is rejected by the data, it raises the possibility that our research design is invalid, which would undermine the credibility of the results from period 1.

The entries in panels A and B of Table 7 provide little evidence of abnormal excess returns. Across the different combinations of affected and unaffected groups and specifications, the point estimates are almost all small in magnitude. Further, none of them are statistically significant by standard criteria. Overall, the findings from this sixty week period support the validity of our research design and lend credibility to the hypothesis that the estimated effects in period 1 are due to the 1964 Amendments.

Panels A and B of Table 8 repeat the same analysis for period 2. Here, the fully affected group has positive abnormal excess returns that would generally be judged statistically significant. The magnitude of the fully affected group's excess abnormal returns is similar to its performance in period 1, though the fact that period 3 is shorter than period 1 (62 weeks instead of 85 weeks) means that the cumulative abnormal excess return is smaller, 7-16%. The estimates for the partially

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<sup>30</sup> Appendix Table 1 reports the results from the separate estimation of equation (2) for each group. The R-squared statistics in Period 1 range between 0.15 and 0.46 for the OTC groups and 0.89 to 0.91 for the listed groups. As expected, the listed groups' returns have a higher covariance with the overall market. The estimated constants from these regressions provide an opportunity to better understand the source of the results in Table 6. The second and third columns report the period 2 and 3 results.

affected group are also generally positive but are much smaller in magnitude. Only one of the eight is statistically significant. These findings for the partially affected group are consistent with the possibility that it was easier for the market to form correct expectations about whether these firms would comply with the law because they were already filing with the SEC and on average were further above the asset and shareholder cutoffs for compliance.

As we discussed above, the interpretation of these results depends on one's beliefs about whether the market had the correct expectations about the information that was revealed in this period. Under the assumption that the market had formed the correct expectations about the enforcement of the Amendments, the fraction of firms that would register with the SEC, and the revealed financial position of filers, then the finding of positive abnormal excess returns invalidates our research design and the standard four factor model in this setting. Alternatively, if markets updated their beliefs about any of the above during period 2, then these abnormal excess returns can be interpreted as a consequence of the law and the period 1 effects underestimate the law's total impact. The finding of zero abnormal excess returns in period 3 supports this possibility. Furthermore, the event study of the exact first filing dates below suggests that resolution of compliance uncertainty is indeed important, especially so for the fully affected group.

Ultimately, we do not think the period 2 results invalidate the period 1 findings. But we are also uncomfortable assigning the period 2 effects to the law. Because the underlying assumptions are inherently untestable, we recognize that others may reach alternative conclusions about period 2.

Figures 3 and 4 summarize the abnormal excess returns graphically. First, consider Figure 3 which plots the cumulative abnormal excess returns for the fully affected group across all three periods. The data points to the left of the first vertical line are obtained from the period 1 regressions that were reported in column (2) of Table 6A. Recall, this specification adjusts for all four factors. We measure the abnormal excess return in each week as the sum of the estimated  $\alpha$  and the residual. These abnormal excess returns are then summed cumulatively and plotted. Thus, the value at the first vertical line is equal to the cumulative abnormal excess return in period 1. The period 2 and 3 regression results from Tables 8A and 7A are used to extend the line between the two vertical lines and to the right of the second vertical line, respectively. Figure 4 plots the cumulative abnormal excess returns for the partially affected group and is produced identically.

The figures show that the period 1 abnormal excess returns for the affected groups occur throughout that period. This is consistent with our impression that the probability of the passage of the legislation increased consistently throughout the event window. In other words, these graphs do not reveal a sharp increase (or decrease) in abnormal returns consistent with an un-

expected jump upward (or downward) in the probability that mandated disclosure requirements would be extended to the fully and partially affected groups. The period 2 abnormal excess returns occur throughout that period. Finally, there is little evidence of abnormal excess returns in any subperiod of period 3.

## 7 Event Study Evidence on the Consequences of Registration

In this section, we present an event study of the consequences of complying with the disclosure requirements of the 1964 Amendments soon after its passage. In the first subsection, we examine the effect on returns. The second subsection explores the effect on bid-ask spreads, which we interpret as a measure of liquidity. These results provide further evidence that the market placed a value on the information firms were mandated to disclose as a result of the 1964 law.

### 7.1 Price Effects of Registration

Here, we test whether the market rewarded firms that registered with the SEC in the period immediately surrounding the SEC's receipt of their registration materials. To implement this analysis, we obtained the exact date that the SEC received registration materials from new filers from all issues of the *SEC News Digest* from 1965 and 1966. We combined this information with the stock returns data from periods 2 and 3.

These data were used to implement an event study of the effects of registration with the SEC. Specifically, we estimate firm-level equations for firm's weekly return minus the riskless rate on a sample of all firms that registered in this period. The sample includes all filers, regardless of whether they are assigned to one of our affected or unaffected groups in the prior analysis. These equations include separate intercepts for period 2 and 3 and control for the overall market return and the three other factors. The effects of the four factors are allowed to vary across the affected and unaffected groups (as we have classified them based on the 1962 information) but are constrained to be equal across the periods within group. We fit this model and then calculate the estimated residuals for each of the 30 weeks prior to the registration week and the 30 weeks subsequent to it.

These residuals are considered the average abnormal return in a week, where the precise week is denoted by its distance from the registration date. Figure 5 plots the cumulative average abnormal returns against the week, again denoted by its distance from the registration date, for firms that filed on or before their due date. This is done separately for firms that had never previously filed with the SEC (i.e., firms assigned to the "0-0" subgroup of the unlisted unaffected group and firms in the fully affected group) and firms that were already complying with the periodic reporting

requirements (i.e., firms assigned to the “2-2” subgroup of the unlisted unaffected group and firm in the partially affected group) but began to comply with the proxy and insider trading requirements for the first time in this period. The vertical lines are placed at 10 weeks before and after the registration week.

For the “0-0” subgroup and fully affected firms, there is a large increase in returns in the 5 weeks preceding the week of registration. Specifically, the filers in this group had cumulative abnormal returns of approximately 8.2% in the 10 weeks preceding registration. This is economically meaningful and statistically significant. There is little evidence of positive or negative abnormal returns in the weeks prior or subsequent to this 10 week period.

There are at least two reasons that it is not surprising that this effect is evident in advance of the recorded registration date. First, firms may have announced the impending filing prior to sending it to the SEC. Second, we found evidence that there were modest delays between the SEC’s receipt of the registration materials and the announcement in the *SEC News Digest*.

In the case of the “2-2” subgroup and partially affected firms, cumulative abnormal returns increase in the 10 weeks preceding the registration week, but this increase appears to be part of a trend throughout the entire sixty week period. Consequently, we suspect that the increase in returns in the 10 week window is not related to the act of registration. The revelation that these firms would comply with the proxy and insider requirements may not have been very surprising because they were already filing quarterly and annual accounting reports with the SEC. Consequently, it seems reasonable to suppose that these registrations did not surprise the market.

Figure 6 displays the analogous results from an examination of the effect of registration for firms that register after the date that they are required to do so based on their fiscal year end date. Here, the results are based on all OTC firms as a group. The line is flat throughout the entire period, indicating that there is no evidence of abnormal excess returns in any subperiod of the sixty weeks. We found evidence that firms were frequently granted extensions by the SEC that allowed them to file after their required registration date. Thus, it is possible that these firms had previously communicated their plans to file to market participants in a credible manner. In this case, the failure to find abnormal returns in the period immediately preceding the registration week is not surprising.

We plan to probe the robustness of these results in a number of ways. First, we plan to do an event study for firms that failed to file, around the potential due date for filing. This date can be determined from their fiscal year ends. As long as the market placed a positive probability on these firms’ filing, we expect that the failure to file would lead to a decline in these firms’ stock prices. Second, we will estimate richer econometric models that include week fixed effects and allow for

the loadings on the four factors to vary at the firm level, rather than the group-level

## 7.2 Registrations' Effect on Bid-Ask Spreads

Our focus to this point has been on price effects of the 1964 Amendments and the subsequent information disclosures that the Act generated. One way that disclosure can affect stock prices is by making a stock more liquid, so we now consider to what extent this was part of the effect of the law. We look at the effect of disclosures on bid/ask spreads. To do this, we essentially run another event study, though creating an appropriate counterfactual is much simpler in this context. We run regressions of the form

$$S_{it} = \alpha_i + \delta_t + r_{it} + c_{it} + \varepsilon_{it} \quad (4)$$

where  $S_{it}$  is the bid/ask spread of firm  $i$  in week  $t$  (defined as the *Barron's* ask minus the bid divided by the average of the bid and ask),  $\alpha$  is a firm fixed effect,  $\delta$  is a week fixed effect,  $r_{it}$  is an indicator that takes the value one if firm  $i$  has filed a registration statement with the SEC as of week  $t$ , and  $c_{it}$  is an indicator that takes the value one if the firm has not registered with the SEC and if week  $t$  is past the due date for SEC registration based on the firm's fiscal year end and the SEC registration rules. The two variables of particular interest are  $r_{it}$ , which indicates that a firm has disclosed the significant amount of information in the registration statement and committed to future disclosures of the three other forms, and  $c_{it}$ , which indicates that the firm has revealed that has *not* registered with the SEC by the stated deadline.

*Barron's* changed the way it calculated bid and ask prices early in 1965 for the firms in its primary OTC table. Because it did not make this change to its supplemental OTC table and because the unlisted unaffected firms are more likely to be on the supplemental list, this analysis could lead to finding a mechanical relationship between filing status and changes in bid/ask spreads during 1965. We deal with this problem by, in some cases, restricting our analysis to firms on the supplemental list or to weeks after the change in *Barron's* and by including separate week fixed effects for the primary and supplemental OTC tables in all specifications. Also, if the equilibrium price to brokers for making a market is similar regardless of share price (for example, if the costs of making trades are constant on a per share basis), then we would expect higher priced stocks to have smaller bid/ask spreads. We therefore control for stock price in some specifications (though this never affects our conclusions.)

Table 9 contains our results. In Panel A, we perform the bid/ask analysis for the fully affected group and then for the partially affected group. We limit the sample to firms that register at some point, so we are not measuring  $c_{it}$ . There is no reason to limit the sample to these affected groups,

however. Given that those groups were determined based on characteristics in 1962, many firms in the other groups will actually register and some in the affected group will not. Therefore, in the top portion of Panel B, we look at all OTC firms that register with the SEC. In the bottom panel, we look at all OTC firms, whether they register or not, which allows us to measure  $c_{it}$ .<sup>31</sup>

The table shows that there appears to be a small association between registration and the reduction of bid/ask spreads. In every specification, the post-registration coefficient is negative. Most of these coefficients are not statistically significant at conventional levels. The estimates using the largest sample vary from marginally significant to significant at the 1% level. Depending on the exact specification, the reduction varies from about 0.1% to about 0.5%. Given a mean bid/ask spread of just over 8%, these point estimates suggest a small but meaningful effect.

Figure 7 displays the exact timing of the change in bid/ask spreads in the weeks immediately leading up to and following initial SEC registration. The graph is based on a regression very similar to the one reported in column (2) of the top part of Figure 9, Panel A. We now include indicators for each of twenty weeks before and after initial registration. The graph displays the coefficients on each of these indicator variables, with any week more than twenty-one weeks prior to registration as the excluded variable. As the graph shows, the estimates of any given weekly coefficient is quite noisy. However, there is a distinct drop down in spreads at the time of the first registration. Similar graphs on other samples show a similar break at the time of registration. The graph shows that there is a drop of approximately 0.5% in the bid/ask spread and that this drop is typically exactly at the time of the firm's registration. One interpretation of these findings is that the likely reduction of insider trading due to the 1964 Amendments had an immediate effect on liquidity.

## 8 Conclusion

We studied the last major change in mandatory disclosure in American financial markets – the Securities Acts Amendments of 1964. The Act imposed new disclosure standards on some OTC firms. The Act did not affect the disclosure requirements of firms listed on exchanges because they were already quite strict and it also did not affect small OTC firms. Therefore, we used the Act to compare the effects of disclosure changes on a group of affected firms while using various groups of unaffected firms to create a counterfactual.

We found that firms in the most affected group (that is, those firms that the Act forced to make periodic financial disclosures, to release proxy information before shareholder meetings, and

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<sup>31</sup>We had to drop firms that moved to an exchange before registering, however, because bid/ask spreads are not available for these firms.

to provide details on holdings and trades by insiders) had a cumulative abnormal excess return of about 20% from the time of the first serious calls for changes in disclosure rules through the time the Act passed. During this same period, those firms that were affected by the imposition of proxy and insider mandates had a cumulative abnormal excess return of approximately 10%.

Our interpretation of these results is that shareholders expected dividends to increase by up to 20% due to reduced insider diversion of resources or increased insider effort after the 1964 Securities Acts Amendments. These estimates might seem quite high in that it is hard to imagine insiders were diverting (or not creating by lack of effort) one-fifth of firm resources before disclosure. However, other studies have found large effects of “improved behavior by insiders”. For example, takeover premia are typically substantial and the benefits of trading on inside information has been shown to be quite large (see, for example Seyhun (1986) and Meulbroek and Hart (1997)).

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Table 1  
Key Dates Associated with the Securities Acts Amendment of 1964

1. November 8, 1960: John F. Kennedy elected President.
2. September 1, 1961: Congress appropriated \$750,000 to the SEC to conduct a study of securities markets.
3. November 28, 1961: Chairman Cary said the SEC would advocate new powers for the agency including subjecting OTC securities to the same rules as applied to those listed on exchanges.
4. April 3, 1963: The SEC released the first part of the Special Study. It recommended imposing exchange disclosure rules on OTC securities.
5. July 9, 1963: A Senate subcommittee unanimously approved a bill extending disclosure rules to OTC firms.
6. July 17, 1963: The SEC released second part of Special Study, which recommended major overhaul of OTC market. The Wall Street Journal gave the study significant coverage and argued that the proposals in the report were dramatic.
7. July 30, 1963: The Senate passed the bill extending disclosure rules to OTC firms.
8. February 6, 1964: President Johnson focused on the SEC's proposed legislation in a "Special Message."
9. March 19, 1964: A House subcommittee passed the bill. But, it appeared banks and insurance companies would not be as affected as other firms in Group 0-4.
10. May 7, 1964: A House committee passed the bill.
11. August 5-6, 1964: The full House and Senate passed the bill.
12. April 30, 1965: Deadline for filing a registration statement with SEC for newly covered firms with at least \$1 million of assets and 750 shareholders if they had a fiscal year end between July 1 and December 31 of 1964. (Deadline was 120 days after fiscal year end for firms with a fiscal year ending between January 1, 1965 and June 30, 1965 and it was 120 days after first fiscal year end after July 1, 1966 for firms with at least \$1 million of assets and 500 shareholders.)

Table 2  
Effects of Securities Acts Amendment of 1964 on Various Types of Securities

Type of Disclosure Period <sup>1</sup>	Registration		Periodic Reporting		Proxy		Insider Trades	
	Pre	Post	Pre	Post <sup>2</sup>	Pre	Post	Pre	Post
Fully Affected Group	No	Yes	No	Yes	No	Yes	No	Yes
Partially Affected Group	Yes	Yes	Yes	Yes	No	Yes	No	Yes
Unlisted Unaffected Group	Varied	Unchanged	Varied	Unchanged	No	No	No	No
Listed Unaffected Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

“Fully Affected Group” = Unlisted securities issued before 1936 with at least 500 shareholders and at least \$1million in firm assets

“Partially Affected Group” = Unlisted securities issued (original or secondary) after 1936

“Unlisted Unaffected Group” = Unlisted securities with fewer than 500 shareholders and/or less than \$1million in firm assets

“Listed Unaffected Group” = Securities listed on major exchanges

<sup>1</sup> “Pre” refers to before the Securities Acts Amendments of 1964 and “Post” refers to after the law.

<sup>2</sup> If the number of shareholders for any given class of securities falls below 300 as of the end of a fiscal year, the reporting requirement is suspended for the subsequent fiscal year.

Table 3  
OTC Sample Firm Counts, 1963-1966

	Jan 63	Jan 64	Jan 65	Jan 66	Dec 66
Sample size	1251	1127	1050	980	943
(a) Move to NYSE/AMEX	85	80	62	38	
Assigned DR (continued/not cont.)	85 (85/0)	80 (80/0)	62 (62/0)	38 (38/0)	
Not assigned DR (continued/not cont.)	0 (0/0)	0 (0/0)	0 (0/0)	0 (0/0)	
(b) Name changes	13	14	13	10	
Assigned DR (continued/not cont.)	8 (8/0)	10 (8/2)	11 (6/5)	7 (7/0)	
Not assigned DR (continued/not cont.)	5 (2/3)	4 (0/4)	2 (0/2)	3 (0/3)	
(c) Mergers	24	25	31	21	
Assigned DR (continued/not cont.)	17 (12/5)	19 (18/1)	24 (14/10)	17 (9/8)	
Not assigned DR (continued/not cont.)	7 (2/5)	6 (5/1)	7 (3/4)	4 (1/3)	
(d) Liquidations	11	10	4	8	
Assigned DR (continued/not cont.)	11 (0/11)	10 (0/10)	4 (0/4)	7 (0/7)	
Not assigned DR (continued/not cont.)	0 (0/0)	0 (0/0)	0 (0/0)	1 (0/1)	
(e) Exit from Barrons, last price > \$2	37	19	14	15	
Assigned DR (continued/not cont.)	32 (0/32)	14 (0/14)	14 (0/14)	11 (0/11)	
Not assigned DR (continued/not cont.)	5 (0/5)	5 (0/5)	0 (0/0)	4 (0/4)	
(f) Exit from Barrons, last price ≤ \$2	66	25	15	15	
Assigned DR (continued/not cont.)	61 (0/61)	22 (0/22)	15 (0/15)	13 (0/13)	
Not assigned DR (continued/not cont.)	5 (0/5)	3 (0/3)	0 (0/0)	3 (0/3)	
(g) Discont. due to gap into next calendar year	8	1	0	2	
Assigned DR (continued/not cont.)	6 (0/6)	0 (0/0)	0 (0/0)	0 (0/0)	
Not assigned DR (continued/not cont.)	2 (0/2)	1 (0/1)	0 (0/0)	0 (0/2)	
Total exits/changes	244	174	139	109	
Total series continued	109	111	85	55	

Note: “DR” = delisting return. The number of observations at the beginning of the following year does not exactly equal the number of observations at the beginning of the current year, minus the number of exits/changes, plus the number of series that are continued. Further details are available on request.

Table 4

OTC Sample Firm Counts By Groups Defined Based on 1963 Filing Status and Size, 1963-1966

Panel A. Sample sizes

Group	Number of firms					% in sample at end of 1966
	63 week 1	64 week 1	65 week 1	66 week 1	66 week 52	
Fully Affected	228	209	193	183	177	77.6
Partially Affected	740	698	667	623	606	81.9
Unlisted Unaffected	283	220	190	174	160	56.5
Listed Unaffected with mkt. cap. <=\$35M (CRSP)	1106	1028	985	925	883	79.8
Listed Unaffected with mkt. cap.<=\$45M (CRSP)	1219	1134	1087	1023	977	80.1

Note: All OTC and CRSP (NYSE/AMEX) firms tabulated are present in their respective samples as of week 1 of 1963. CRSP firms exclude firms that enter from the Barrons sample during the 63-66 period. We exclude banks and insurance companies (2-digit SIC codes 60, 61, 63, and 64). The total number of firms on CRSP (excluding Barrons entrants) on the five dates above are 2014, 1943, 1919, 1900, 1889. Firms in the listed unaffected group with market capitalization<=\$35M constitute 3.6 percent of CRSP market capitalization in 1963, week 1. Firms in the listed unaffected group with market capitalization<=\$45M constitute 4.8 percent of CRSP market capitalization in week 1 of 1963.

Table 4, continued

## OTC Sample Firm Counts By Groups Defined Based on 1963 Filing Status and Size, 1963-1966

## Panel B. Percent of firms actually filing with the SEC

Group	Percent of firms present in OTC sample in week 1 of 1963 who file with the SEC as of July of:				
	63	64	65	66	67
Fully Affected	5.3 (0)	11.0 (4.8)	77.2 (71.9)	76.8 (69.3)	72.8 (65.4)
Partially Affected	100 (100)	98.4 (96.9)	95.0 (91.2)	89.9 (84.6)	81.9 (76.2)
Unlisted Unaffected					
“0-0” subgroup	4.7 (0)	8.3 (2.4)	27.2 (20.7)	32.0 (25.4)	37.9 (31.4)
“2-2” subgroup	100 (100)	97.4 (94.7)	93.0 (89.5)	90.4 (85.1)	84.2 (79.0)

Group	Percent of firms in OTC sample in week indicated who file with the SEC as of the following July				
	63, week 1	64, week 1	65, week 1	66, week 1	66, week 52
Fully Affected	5.3 (0)	10.3 (4.8)	82.4 (78.2)	82.0 (77.1)	78.5 (72.9)
Partially Affected	100 (100)	98.9 (97.3)	96.7 (93.0)	93.6 (88.6)	85.8 (80.7)
Unlisted Unaffected					
“0-0” subgroup	4.7 (0)	10.5 (3.2)	36.7 (28.4)	46.5 (37.4)	57.8 (47.8)
“2-2” subgroup	100 (100)	99.0 (95.8)	96.3 (91.4)	97.3 (89.3)	94.3 (85.7)

Note: Numbers in parenthesis exclude firms that file because they merged with a firm that is filing.

Table 5  
Firm Characteristics by Mandatory Disclosure Groups, 1962

	Fully Affected		Partially Affected		Unlisted Unaffected	
	Sample	Mean	Sample	Mean	Sample	Mean
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
# Firms	228	–	740	–	283	–
Share Price	228	\$19.43	740	\$17.04	283	\$7.13
Dividends per Share	228	\$0.79	740	\$0.54	283	\$0.11
Market Cap.	217	\$59.6	688	\$99.2	29	\$2.3
Median	217	\$24.6	688	\$29.9	29	\$2.2
# Shareholders	217	2,773	688	4,848	29	491
Assets	228	\$28.6	736	\$33.3	220	\$3.4
Book Value of Assets	155	\$13.9	736	\$15.5	38	\$2.7
Net Income	225	\$1.7	730	\$1.6	218	\$0.1
Sales	190	\$33.0	665	\$32.6	208	\$4.1
Director Names Published	228	97.8%	736	98.9%	221	33.5%
Stock Options Info	228	42.5%	736	61.0%	221	14.9%
Year of Incorporation	227	1928	734	1937	215	1950

All dollar figures are in millions, except per share data. All financial data are for 1962. “Mean” is the mean of each variable for those observations where the variable is available. All information, except share price and dividends are from *Moody’s* publications.

Table 6  
Average Abnormal Excess Returns  
Period 1: January 1963 - August 24, 1964

Panel A: Fully Affected Group				
	(1)	(2)	(3)	(4)
Fully Affected Group ("0-4") - Listed Unaffected Group ("4-4")				
$\alpha_{\text{period 1}}$ (%)	0.166 (0.076)	0.177 (0.076)	0.177 (0.077)	0.217 (0.073)
R-squared	0.313	0.410	0.377	0.489
Observations	85	85	85	85
Fully Affected Group ("0-4") - Unlisted Unaffected Group ("0-0,2-2")				
$\alpha_{\text{period 1}}$ (%)	0.283 (0.093)	0.310 (0.104)	0.251 (0.105)	0.282 (0.093)
R-squared	0.005	0.022	0.030	0.013
Observations	85	85	85	85
Fully Affected Group ("0-4") - Partially Affected Group ("2-4")				
$\alpha_{\text{period 1}}$ (%)	0.089 (0.046)	0.080 (0.059)	0.151 (0.068)	0.091 (0.059)
R-squared	0.098	0.120	0.142	0.154
Observations	85	85	85	85
Factor Model	Market only	4 factors	4 factors	4 factors
Industry Matched	No	No	Yes	No
Buy and Hold	No	No	No	Yes

Table 6, continued  
Average Abnormal Excess Returns  
Period 1: January 1963 - August 24, 1964

Panel B: Partially Affected Group

	(1)	(2)	(3)	(4)
Partially Affected Group (“2-4”) - Listed Unaffected Group (“4-4”)				
$\alpha_{\text{period 1}}$ (%)	0.080 (0.062)	0.105 (0.069)	0.124 (0.068)	0.131 (0.068)
R-squared	0.255	0.341	0.264	0.393
Observations	85	85	85	85
Partially Affected Group (“2-4”) - Unlisted Unaffected Group (“0-0,2-2”)				
$\alpha_{\text{period 1}}$ (%)	0.194 (0.085)	0.230 (0.096)	0.181 (0.106)	0.191 (0.086)
R-squared	0.061	0.081	0.051	0.087
Observations	85	85	85	85
Factor Model	Market only	4 factors	4 factors	4 factors
Industry Matched	No	No	Yes	No
Buy and Hold	No	No	No	Yes

Table 7  
Average Abnormal Excess Returns  
Period 3: November 2, 1965 through end of 1966

Panel A: Fully Affected Group				
	(1)	(2)	(3)	(4)
Fully Affected Group ("0-4") - Listed Unaffected Group ("4-4")				
$\alpha_{\text{period 3}}$ (%)	-0.155 (0.095)	-0.004 (0.080)	-0.011 (0.090)	-0.003 (0.105)
R-squared	0.364	0.680	0.461	0.598
Observations	60	60	60	60
Fully Affected Group ("0-4") - Unlisted Unaffected Group ("0-0,2-2")				
$\alpha_{\text{period 3}}$ (%)	-0.103 (0.146)	-0.021 (0.132)	-0.047 (0.151)	-0.122 (0.170)
R-squared	0.010	0.173	0.096	0.099
Observations	60	60	60	60
Fully Affected Group ("0-4") - Partially Affected Group ("2-4")				
$\alpha_{\text{period 3}}$ (%)	-0.019 (0.057)	-0.011 (0.063)	0.080 (0.068)	-0.002 (0.083)
R-squared	0.009	0.019	0.006	0.006
Observations	60	60	60	60
Factor Model	Market only	4 factors	4 factors	4 factors
Industry Matched	No	No	Yes	No
Buy and Hold	No	No	No	Yes

Table 7, continued  
Average Abnormal Excess Returns  
Period 3: November 2, 1965 through end of 1966

Panel B: Partially Affected Group				
	(1)	(2)	(3)	(4)
Partially Affected Group (“2-4”) - Listed Unaffected Group (“4-4”)				
$\alpha_{\text{period 3}}$ (%)	-0.133 (0.086)	-0.003 (0.071)	-0.026 (0.069)	-0.003 (0.076)
R-squared	0.376	0.642	0.442	0.705
Observations	60	60	60	60
Partially Affected Group (“2-4”) - Unlisted Unaffected Group (“0-0,2-2”)				
$\alpha_{\text{period 3}}$ (%)	-0.084 (0.142)	-0.010 (0.129)	0.052 (0.119)	-0.120 (0.159)
R-squared	0.020	0.158	0.105	0.090
Observations	60	60	60	60
Factor Model	Market only	4 factors	4 factors	4 factors
Industry Matched	No	No	Yes	No
Buy and Hold	No	No	No	Yes

Table 8  
Average Abnormal Excess Returns  
Period 2: August 25, 1964 - November 1, 1965

Panel A: Fully Affected Group				
	(1)	(2)	(3)	(4)
Fully Affected Group ("0-4") - Listed Unaffected Group ("4-4")				
$\alpha_{\text{period 2}}$ (%)	0.095 (0.081)	0.159 (0.077)	0.161 (0.079)	0.215 (0.082)
R-squared	0.052	0.293	0.222	0.272
Observations	62	62	62	62
Fully Affected Group ("0-4") - Unlisted Unaffected Group ("0-0,2-2")				
$\alpha_{\text{period 2}}$ (%)	0.197 (0.088)	0.228 (0.092)	0.262 (0.116)	0.242 (0.127)
R-squared	0.007	0.112	0.103	0.188
Observations	62	62	62	62
Fully Affected Group ("0-4") - Partially Affected Group ("2-4")				
$\alpha_{\text{period 2}}$ (%)	0.109 (0.048)	0.121 (0.051)	0.194 (0.053)	0.148 (0.060)
R-squared	0.007	0.085	0.107	0.074
Observations	62	62	62	62
Factor Model	Market only	4 factors	4 factors	4 factors
Industry Matched	No	No	Yes	No
Buy and Hold	No	No	No	Yes

Table 8, continued  
Average Abnormal Excess Returns  
Period 2: August 25, 1964 - November 1, 1965

Panel B: Partially Affected Group				
	(1)	(2)	(3)	(4)
Partially Affected Group ("2-4") - Listed Unaffected Group ("4-4")				
$\alpha_{\text{period 2}}$ (%)	-0.014 (0.064)	0.038 (0.055)	0.047 (0.049)	0.067 (0.061)
R-squared	0.060	0.237	0.152	0.233
Observations	62	62	62	62
Partially Affected Group ("2-4") - Unlisted Unaffected Group ("0-0,2-2")				
$\alpha_{\text{period 2}}$ (%)	0.088 (0.076)	0.107 (0.083)	0.196 (0.091)	0.094 (0.119)
R-squared	0.002	0.046	0.046	0.132
Observations	62	62	62	62
Factor Model	Market only	4 factors	4 factors	4 factors
Industry Matched	No	No	Yes	No
Buy and Hold	No	No	No	Yes

Table 9  
Mandatory Disclosure Regulation and Bid/Ask Spreads

Panel A: Affected Groups Only

	(1)	(2)	(3)	(4)	(5)
Fully Affected Group					
Post-registration	-0.0045 (0.0047)	-0.0061 (0.0031)	-0.0057 (0.0032)	-0.0026 (0.0021)	-0.0015 (0.0073)
R-squared	0.706	0.715	0.717	0.758	0.630
Observations	25,632	11,531	11,531	11,531	8,235
Partially Affected Group					
Post-registration	-0.0037 (0.0021)	-0.0022 (0.0018)	-0.0016 (0.0017)	-0.0025 (0.0011)	-0.0009 (0.0047)
R-squared	0.741	0.786	0.790	0.831	0.705
Observations	75,107	33,091	33,091	33,091	16,138
1963-1966	Yes	No	No	No	Yes
March '65 - 1966 only	No	Yes	Yes	Yes	No
Control for price	No	No	Yes	Yes	No
Firm-specific trends	No	No	No	Yes	No
Supplemental list only	No	No	No	No	Yes

Table 9, continued  
Mandatory Disclosure Regulation and Bid/Ask Spreads

Panel B: Larger Samples

	(1)	(2)	(3)	(4)	(5)
All registering firms					
Post-registration	-0.0027 (0.0023)	-0.0007 (0.0016)	-0.0002 (0.0016)	-0.0014 (0.0010)	-0.0013 (0.0045)
R-squared	0.729	0.776	0.779	0.824	0.654
Observations	113,059	50,254	50,254	50,254	29,655
All OTC firms					
Post-registration	-0.0081 (0.0028)	-0.0024 (0.0016)	-0.0019 (0.0016)	-0.0020 (0.0010)	-0.0088 (0.0057)
Post-expected regist.	0.0028 (0.0039)	-0.0004 (0.0024)	-0.0001 (0.0024)	-0.0002 (0.0020)	0.0034 (0.0070)
R-squared	0.734	0.813	0.815	0.850	0.665
Observations	180,214	67,773	67,773	67,773	60,653
1963-1966	Yes	No	No	No	Yes
March '65 - 1966 only	No	Yes	Yes	Yes	No
Control for price	No	No	Yes	Yes	No
Firm-specific trends	No	No	No	Yes	No
Supplemental list only	No	No	No	No	Yes

Notes: All regressions include indicators for each firm and week indicators (each of which is interacted with an indicator for being on the Barron's "Supplemental" list). "Post-registration" indicates that the observation is from after the firm files a registration statement with the SEC. "Post-expected registration" indicates that the firm did not file with the SEC by the end of 1966 but, had it filed 18 weeks after the end of its first fiscal year ending after July 30, 1964, the observation would have been after such a filing. Robust standard errors allow for correlation across observations within each firm.

Appendix Table 1A  
Four Factor Estimates by Portfolio by Period – Affected Groups

	Period 1	Period 2	Period 3
<b>Fully Affected Group</b>			
$\alpha$	0.205 (0.088)	0.191 (0.077)	0.018 (0.087)
Market Factor Beta	0.380 (0.139)	0.776 (0.097)	0.806 (0.063)
SMB Factor	0.462 (0.136)	0.522 (0.138)	0.843 (0.140)
HML Factor	-0.022 (0.114)	0.222 (0.138)	0.235 (0.063)
Momentum Factor	-0.135 (0.170)	-0.024 (0.089)	-0.185 (0.086)
R-squared	0.302	0.702	0.890
Observations	85	62	60
<b>Partially Affected Group</b>			
$\alpha$	0.124 (0.088)	0.070 (0.056)	0.0029 (0.068)
Market Factor Beta	0.503 (0.124)	0.834 (0.080)	0.826 (0.054)
SMB Factor	0.581 (0.120)	0.669 (0.141)	0.877 (0.095)
HML Factor	-0.018 (0.117)	0.281 (0.106)	0.260 (0.066)
Momentum Factor	-0.115 (0.152)	-0.127 (0.086)	-0.183 (0.054)
R-squared	0.456	0.792	0.938
Observations	85	62	60

Notes: Listed unaffected group size matched to fully affected (partially affected) group includes AMEX firms with market capitalization under \$36 million (\$63 million) as of January 1, 1963. “SMB” Factor is Fama-French small minus big factor and “HML” is Fama-French high minus low factor. See Fama and French (1993) for details. “Momentum Factor” is calculated based on daily returns so as to be equivalent to the “UMB” factor available monthly from Kenneth French’s web page ([http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data\\_library.html](http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html)). Robust standard errors in parentheses.

Appendix Table 1B  
Four Factor Estimates by Portfolio by Period – Unaffected Groups

	Period 1	Period 2	Period 3
<b>Unlisted Unaffected Group</b>			
$\alpha$	-0.105 (0.141)	-0.036 (0.098)	0.018 (0.087)
Market Factor Beta	0.325 (0.168)	0.861 (0.115)	0.806 (0.063)
SMB Factor	0.516 (0.180)	0.853 (0.189)	0.843 (0.140)
HML Factor	0.170 (0.164)	0.257 (0.160)	0.235 (0.063)
Momentum Factor	-0.222 (0.213)	-0.192 (0.116)	-0.185 (0.087)
R-squared	0.148	0.654	0.740
Observations	85	62	60
<b>Listed Unaffected Group (size matched to Fully Affected)</b>			
$\alpha$	0.028 (0.045)	0.033 (0.048)	0.061 (0.061)
Market Factor Beta	0.799 (0.039)	0.955 (0.070)	1.000 (0.052)
SMB Factor	0.913 (0.083)	1.023 (0.097)	1.233 (0.098)
HML Factor	0.205 (0.069)	0.629 (0.088)	0.313 (0.068)
Momentum Factor	-0.099 (0.068)	-0.219 (0.131)	-0.122 (0.072)
R-squared	0.889	0.905	0.964
Observations	85	62	60
<b>Listed Unaffected Group (size matched to Partially Affected)</b>			
$\alpha$	0.019 (0.042)	0.032 (0.044)	0.062 (0.055)
Market Factor Beta	0.810 (0.037)	0.961 (0.065)	1.004 (0.047)
SMB Factor	0.902 (0.076)	1.015 (0.088)	1.179 (0.086)
HML Factor	0.220 (0.064)	0.522 (0.079)	0.319 (0.062)
Momentum Factor	-0.096 (0.063)	-0.205 (0.121)	-0.106 (0.062)
R-squared	0.906	0.922	0.971
Observations	85	62	60

Notes: See notes to Appendix Table 1A.

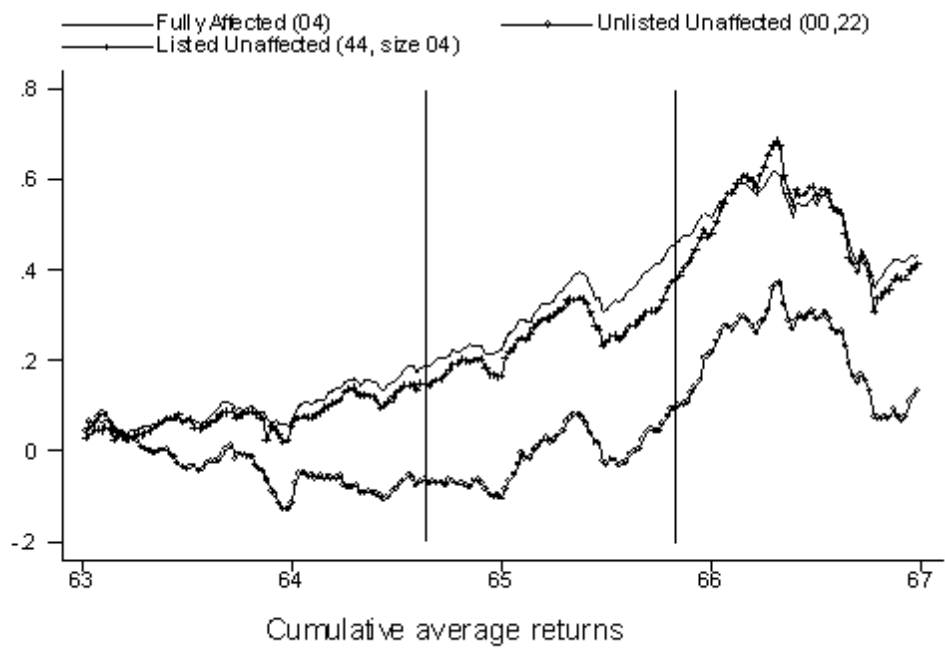


Figure 1: Returns by group, 1963-1966

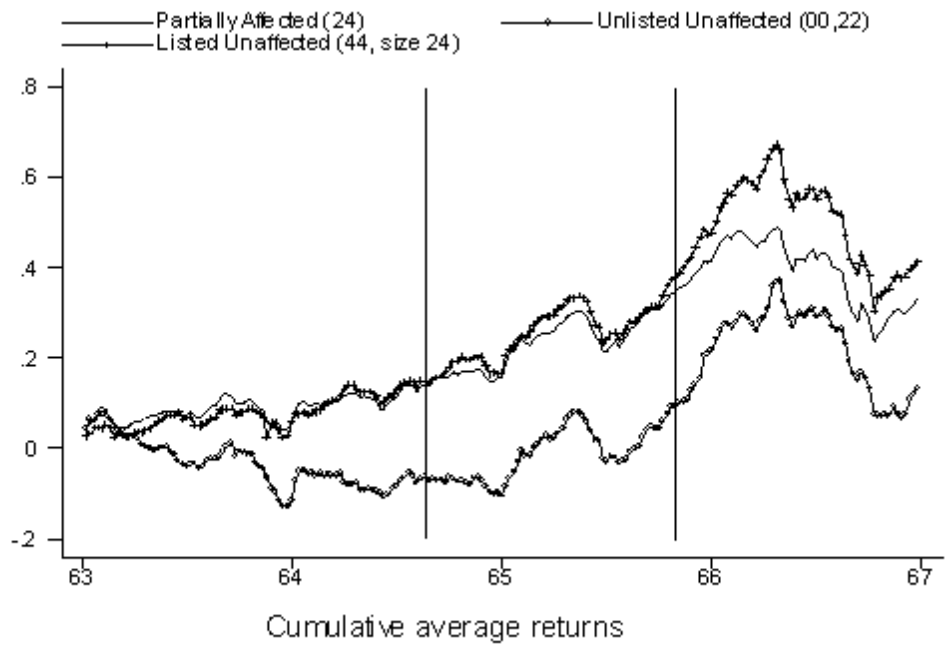


Figure 2: Returns by group, 1963-1966

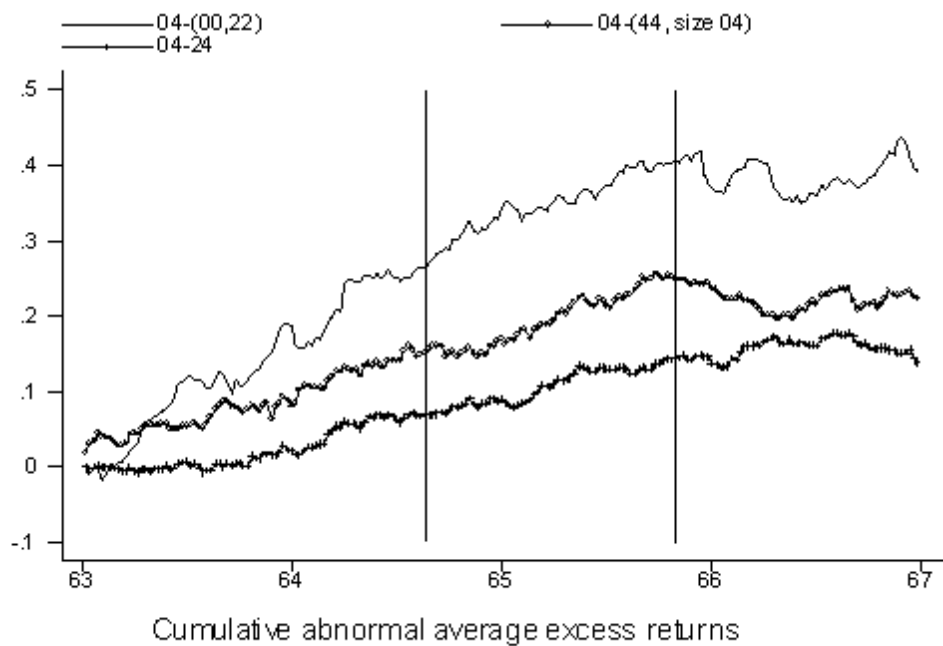


Figure 3: Abnormal excess returns of fully affected group

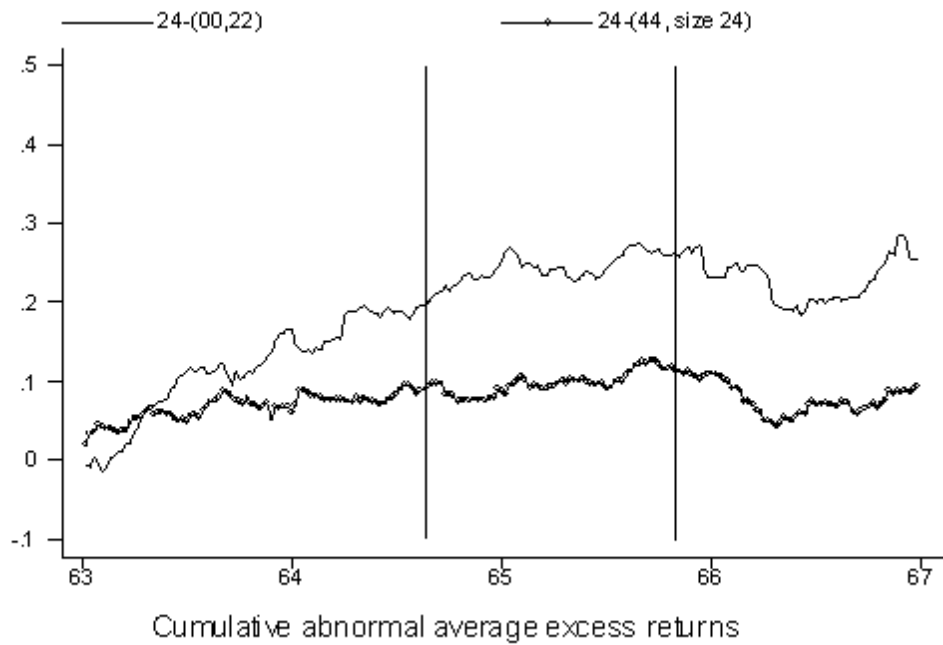


Figure 4: Abnormal excess returns of partially affected group

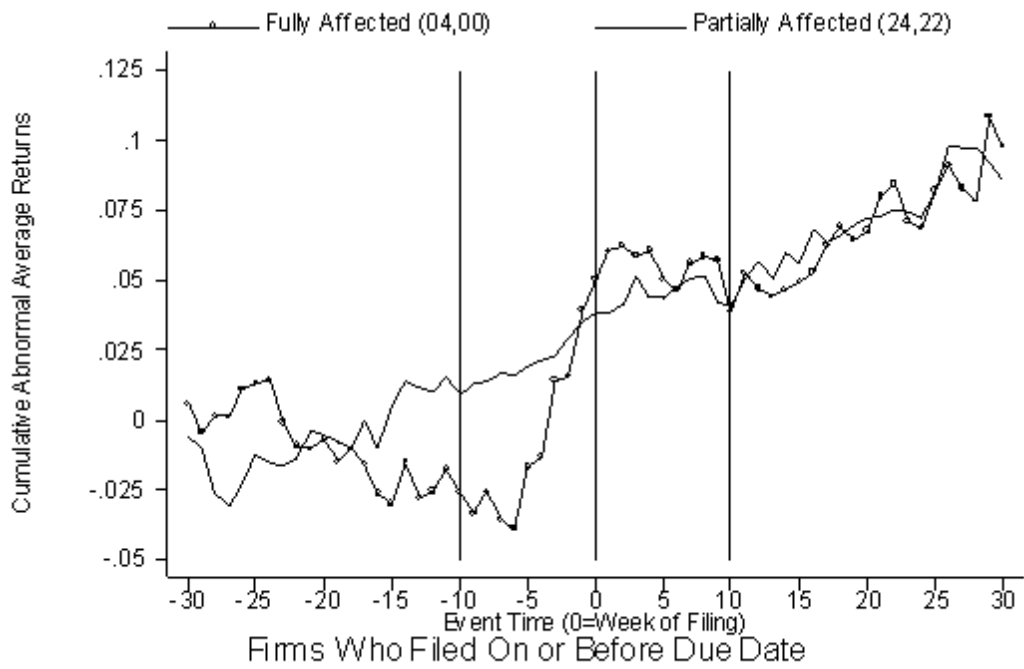


Figure 5: Abnormal returns near SEC registration

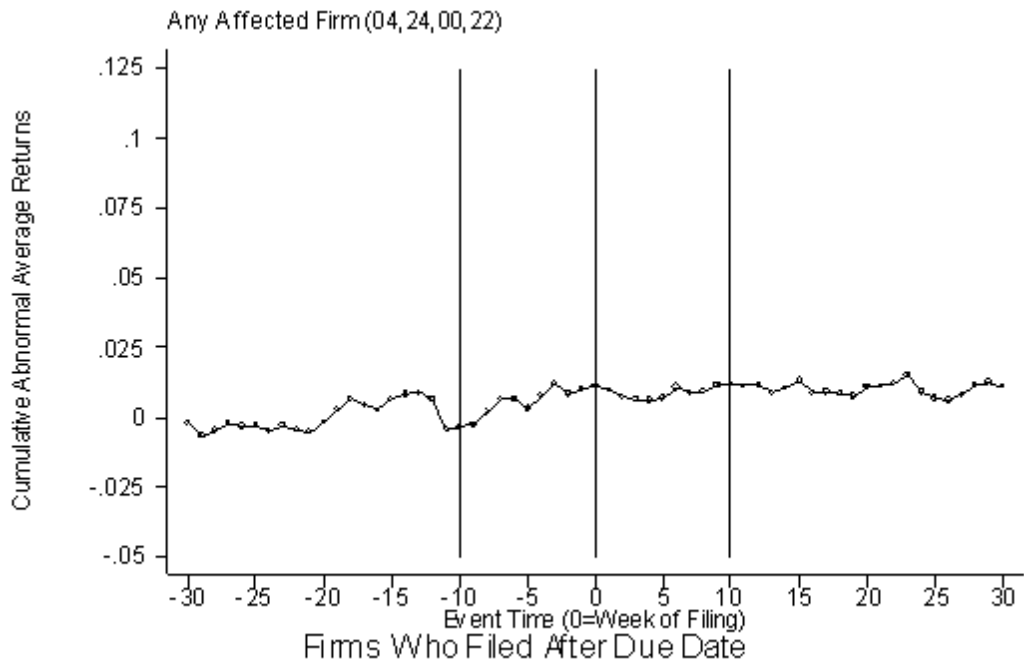


Figure 6: Abnormal returns near expected SEC registration date for late filers

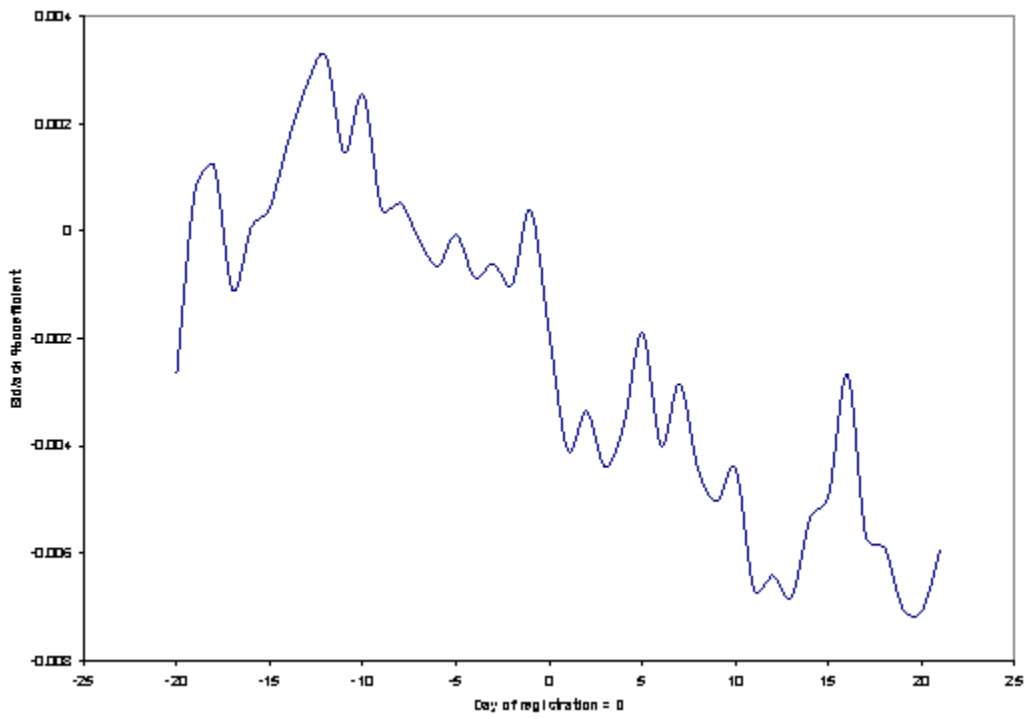


Figure 7: Bid/ask spreads around date of initial SEC registration