

**Mandatory Disclosure, Asymmetric Information and Liquidity:  
The Impact of the 1934 Act**

Robert Daines  
*Stanford University*

Charles M. Jones  
*Columbia University\**

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\* Email: [daines@stanford.edu](mailto:daines@stanford.edu) and [cj88@columbia.edu](mailto:cj88@columbia.edu). We thank Yakov Amihud and seminar participants at Columbia, NYU, and Stanford for helpful comments. We are grateful to a number of research assistants who hand collected data, and especially Lisa LaPierre. We also thank the Center for Research in Securities Prices at the University of Chicago for providing pre-release data.

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**ABSTRACT**

The Securities Exchange Act of 1934 was the central regulatory event of the modern financial era. The Act required firms listed on U.S. exchanges to provide detailed financial disclosure and provided means for private investors and public agencies to enforce these new disclosure duties. We ask whether the Act was an improvement on the prior legal regime, where disclosure was left to private contract and state legal rules. We also examine whether mandatory disclosure provided investors with useful information. We use differences in firms' pre-Act disclosure policies to study the Act's effects on bid-ask spreads and other measures of the information environment. We are unable to identify specific newly required disclosures that reduce information asymmetries or improve liquidity. There is evidence however that the Act's new enforcement mechanisms allowed firms to credibly commit to making their disclosures accurate.

## **1. Introduction**

American securities markets are the largest in the world. At year end 2005, the US stock market was valued at about \$17 trillion and the public bond market was valued at about \$25 trillion, based on data from the Center for Research in Securities Prices (CRSP) and the Bond Market Association. New securities are issued at a brisk pace. On average, more than \$100 billion of new equity and \$5 trillion of new debt securities have been issued annually in the U.S. for the past 5 years.

These large and liquid capital markets are valuable because they reduce transaction costs and promote long-run economic growth (Levine and Zervos, 1998). Liquid markets encourage saving, provide firms with ready access to capital, allocate capital to effective uses and provide investors with incentives to improve firm governance (Holmstrom and Tirole, 1993), all of which may help to lead to economic growth that benefits even non-investors..

One reason US capital markets are so large and liquid may be the extensive network of disclosure regulations that govern public firms. Beginning in 1934, firms that wished to raise money from the American public have been required to provide investors with detailed information about the firm's historical performance, assets, operations, risks, funding, and management and compensation. These initial disclosures must be updated quarterly and annually and the firms results must be audited by independent accounting firms. As a result, firms must disclose what they would otherwise keep private and sometimes must remain silent when they would otherwise wish to disclose. Disclosure regulations compel not only what firms must disclose, but when.

The creation of these disclosure requirements and the Securities and Exchange Commission that administers them is probably the central regulatory event in modern US financial history. Subsequent SEC Chairmen and proponents in their turn have all heralded this mandatory disclosure and vigorous public and private enforcement because it "improves liquidity, reduces capital costs and makes fair market prices possible." (Levitt, 1997). Mandated disclosure is thus a favorite regulatory tool. In the wake of massive financial fraud and the series of scandals involving Enron, World-Com, and Adelphia, Congress' primary response was to pass new disclosure rules.

It is easy to see why detailed and credible disclosure can be valuable to investors. Accurate information allows investors to price securities and to monitor managers' performance, thereby reducing agency costs. In fact, firms that fail to disclose information may find it hard and even impossible to raise money, as investors may take silence for bad news and refuse to invest (Ross, 1979, Grossman, 1981, and Milgrom, 1981). Thus, firms and entrepreneurs may even find it in their own interest to commit to disclose information, whether or not it is required by law.

However, while disclosure has benefits, it also has costs and it is not obvious that the benefits of disclosure will always exceed the costs. Disclosure imposes direct costs, as firms produce and verify the information, and indirect costs, to the extent that competitors, customers and others can use the disclosed information to the firm's disadvantage. Too much disclosure may even exacerbate agency costs if investors are relatively uninformed and managers therefore myopic (Stein, 1988).

Because the costs and benefits of disclosure are likely to vary between firms and because a firm's owners have an incentive to make cost-justified disclosures voluntarily, many argue that disclosure should be left to market transactions and contractual commitments and that regulators should refrain from imposing mandatory, uniform requirements. Absent mandatory requirements, firms and investors can customize contractual disclosure obligations and select the optimal levels of disclosure for each firm. If they can create credible mechanisms of enforcement, these contractual disclosures may be preferable. Thus, mandatory, uniform disclosure requirements may thus be unnecessary and even harmful (Romano, 2002; Mahoney, 1997; Choi and Guzman, 1997). Investors and firms may be better off if they are given freedom to determine their own disclosure policy, or to select from competing state regulatory schemes. Just as firms currently elect which state's corporate law rules they will be governed by or which rules govern the rights of bondholders, firms could elect to be governed by state or optional federal disclosure regulations. States currently provide differing corporate law rules and enforcement mechanisms that govern manager's fiduciary obligations and could do the same for a firm's disclosure obligations. Private groups, such as exchanges, provide and enforce disclosure rules and, absent mandatory federal regulations, may provide more responsive and flexible rules. A mandatory federal disclosure regime may thus be inferior to a system of contractual obligation backed by the firm's choice of state regulation and private exchanges.

As a theoretical matter, both contractual and mandatory regulatory schemes are plausible. The choice between the two different regimes turns largely on empirical issues. First, will investors actually benefit from the additional information required by law (above and beyond) what the firm would otherwise have voluntarily disclosed? Second, do a firm's disclosures affect other firms? Even if firms adopt privately optimal disclosure policies, mandatory disclosure may be socially beneficial if there are meaningful externalities to a firm's decisions about what to disclose (Easterbrook and Fischel, 1981; Coffee, 1984, Dye, 1990; Admati and Pfleiderer, 2000). If there actually are, in practice, significant externalities to a firm's disclosure practices, mandatory disclosure may be preferred to a regime where firms voluntarily select their disclosure practices. Finally, a related question is whether a regime of uniform mandatory disclosure offers benefits in enforcement – either in economies of scale or in the types of remedies available to it (such as jail terms) – that would make it preferable.

Unfortunately, we know little about the effects of or need for mandatory disclosure. Thus, according to the Council of Economic Advisors, “whether SEC enforced disclosure rules actually improve the quality of information that investors receive remains a subject of debate among researchers almost 70 years after the SEC's creation.” (Economic Report of the President, 2003). One reason for this is that the basic questions are in large part empirical, but empirical investigation of mandatory disclosure has been “virtually non-existent” (Healy and Palepu, 2001; Fox, Durnev and Morck, 2004). One reason for this lack of empirical investigation is that there is so little variation in firm disclosure policies. Since 1934, all publicly traded firms have been under the identical regime of mandatory and uniform disclosure requirements, so there is little opportunity to investigate the effect of variation in disclosure rules.

We therefore examine the effect of mandatory disclosure and supporting rules and enforcement provisions by focusing on the birth of U.S. disclosure regulation with the Securities Exchange Act of 1934 (the 1934 Act). Prior to 1934, firm disclosure was voluntary and firms largely decided what to disclose. Exchanges offered different disclosure requirements and states offered different regulatory schemes. Following the Act, all firms were required to comply with the same SEC disclosure requirements in order to have their securities publicly traded on national exchanges.

We therefore examine the change from a regime of state-based regulation and voluntary disclosure to a regime of mandatory disclosure rules. The basic questions we address are: (1)

did the Act provide investors with meaningful information they did not already have? That is, did it reduce information asymmetries between insiders and outsiders? and (2) is there evidence that a firm's disclosure policies affect other firms?

In order to evaluate the Act's effect on the information environment, we examine changes in liquidity variables such as bid-ask spreads at the time the Act became effective. Bid-ask spreads are a common proxy for information asymmetries because they reflect the risk that market makers will lose money when trading against informed parties (Glosten and Milgrom, 1985, Copeland and Galai, 1983). The idea is that because informed parties only buy (sell) when the stock is under (over)-priced, specialists face an adverse selection problem. As information asymmetries increase, market makers must widen the spread to prevent losses to informed parties and to recoup losses by profiting more in trades with uninformed parties.

Thus, if the 1934 Act reduced information asymmetries, bid-ask spreads should narrow following its enactment. However, the Act's effect on firms would depend on the level of disclosure the firm had agreed to previously. If the substantive disclosures required by the Act provided useful information, bid-ask spreads would drop for firms that were not previously disclosing information, but disclosed only when forced to by the Act; firms that were already disclosing the information required by the Act would be relatively unaffected.

A second hypothesis is that 1934 Act, with its array of private and public enforcement provisions, allowed firms to credibly signal information that they were previously unable to convey credibly. Disclosure might be especially valuable to firms that have more severe information asymmetries (small, young, less visible, or unusual firms in industries where investors had little information beyond that conveyed directly by managers). Such firms were likely trying to disclose information even before compelled to. However, such disclosers may not have been credible. Given the lack of serious enforcement from either exchanges or state regulators, firms may have had an incentive to misstate their results and investors may have discounted disclosures generally. The Act's enforcement mechanisms, especially its provision for civil and criminal liability, may have allowed firms to make credible disclosures. In this case, firms that benefited from disclosure and that were therefore already disclosing information would see significant liquidity improvements. We call this the signaling hypothesis and it predicts greater liquidity changes for firms that were already disclosing information before

required to by the Act. In this account, the Act's enforcement provisions function like a technology innovation that allows firms to better signal the accuracy of their financial reports.

We find little evidence to suggest that the 1934 Act compelled the disclosure of additional data that were useful to investors. However, the evidence is consistent with a signaling story that suggests that the 1934 Act's primary effect was to create enforcement mechanisms that allowed firms to credibly commit to make their disclosures accurate.

The value of mandatory disclosure regulation is an important question because disclosure regulations are an important regulatory tool. If the costs of these rules exceed their benefits, as some suggest about the recent Sarbanes-Oxley regulations, then perhaps voluntary disclosures remain preferable (Ribstein, 2002; Romano, 2005). Second, many developing financial markets are also considering their own disclosure regulations and may be choosing between relatively full, mandated disclosure or greater reliance on private contracts. Finally, as exchanges become for-profit entities and international competition between exchanges for listings intensifies, the relative value of voluntary and mandatory disclosure regulations becomes a salient issue. The choice between mandatory and voluntary disclosure regimes is thus an important one.

Part 2 describes disclosure practices prior to 1934 and the disclosure regulations that went into effect following the Act. Part 3 describes prior research on the Act's effects. Part 4 describes our results. Part 5 examines the effect of similar disclosure regulations that went into effect for a subset of firms the prior year (the 1933 Act). Part 6 examines whether a firm's disclosure policies affect other firms. Part 7 concludes.

## **2. The Securities Exchange Act of 1934**

Prior to the 1934 Act, disclosure of financial results was largely voluntary. Firms were free to disclose or withhold information as they saw fit and could elect whether or not to have these statements audited. Even large publicly traded firms chose not to disclose much. Large firms such as the New York Central Railroad, Westinghouse, Singer, Gillette and the National Biscuit Company provided either no balance sheet, no income statement, or neither.<sup>1</sup> Banks and insurance companies often sought to avoid the disclosure requirements on the grounds that fluctuations in their stock price might reduce depositor confidence.

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<sup>1</sup> Ripley 156-207; Hawkins, 135-45

Over time, exchanges raised disclosure requirements for firms that were applying for new listings, but typically did not increase requirements for firms that were already publicly trading. They instead took the position that their original listing agreements with the firm prevented them from increasing the disclosure requirements. In the 1920s, NYSE began requiring all newly-listing firms to provide an audited balance sheet and income statement and frequently required ongoing quarterly disclosure (Mahoney, 1997). As a result, by the late 1920's, gross income was reported by only 43% firms and less than half disclosed depreciation expense (Seligman, 1983). As of 1934, only about half of all firms disclosed sales and the cost of goods sold, while 90% of firms disclosed depreciation expense, current assets and current liabilities (Benston, 1973).

Supplementing these exchange-based requirements was a web of state-based anti-fraud regulations. By 1933, 47 states had enacted their own system of securities regulation that provided for fines and even criminal sanctions against those found guilty of securities fraud. There is little data about how these state regulatory schemes were enforced, but was limited because they could not be enforced across state lines or stop offers made by mail. The extent to which the exchanges enforced their own disclosure requirements is also unclear (Coffee and Seligman, 2003).

Following the Crash of 1929 and the Great Depression, calls for increased mandatory disclosure came as part of the general reform effort aimed at cleaning up Wall Street's misleading and harmful practices. According to the Act's preamble, voluntary disclosure produced too little information, leading in turn to "excessive speculation," "excessive volatility", inaccurate share prices, stock manipulation and fraud. According to an SEC history of the time, pre-1934 disclosure practices

resulted in a situation in which no one could be sure that market prices for securities bore any reasonable relation to intrinsic values or reflected the impersonal forces of supply and demand. In fact, the investigation record demonstrated that during 1929 the prices of over 100 stocks on the New York Stock Exchange were subject to manipulation by massive pool operations. One of the principal contributing factors to the success of the manipulator was the inability of investors and their advisors to obtain reliable financial and other information upon which to evaluate securities. (SEC – A 25 year Summary, 1959)

Greater disclosure of previously private information would protect investors from informed insiders and stock manipulators, assure reliable prices and thereby increase investor confidence.



The basic thought was that lack of disclosure encouraged fraud, which reduced investor confidence and led ultimately to a crash.

Thus, Senator Fletcher introduced the bill to considerable fanfare on February 9, 1934, claiming that the bill would “bring safety to the general public in the field of investing.” “Under this bill”, as reported on the front page of the Wall Street Journal, “the securities exchanges will not only have the appearance of an open market place for investors, but will be truly open to them, free from the hectic operations and dangerous practices which in the past have enabled a handful of men to operate with stacked cards against the general body of outside investors.”

Concern for the investing public was only one justification for the Act, however. Another, perhaps more pressing issue, was the widespread concern that the lack of disclosure had widespread negative effects on the real economy. Critics argued that lack of regulation led to the Crash and ultimately prolonged the Depression. As spelled out early in the Act, “National Emergencies, which produce widespread unemployment and the dislocation of trade, transportation and industry and which burden interstate commerce and adversely affect the general welfare, are precipitated, intensified and prolonged by manipulation and sudden and unreasonable fluctuations of securities prices on such exchanges and markets.” (Section 2(4)). Thus, the new regime of full disclosure and fair trading would add increased investor confidence and assure that unregulated stock exchanges would “never again intensify a business depression or help precipitate a business depression.”

The Act’s supporters argued that a regime of mandatory public disclosure of the firm’s results would change this. The Act’s preamble stated that

Manipulation and dishonest practices of the market place thrive upon mystery and secrecy. The disclosure of information materially important to investors may not instantaneously be reflected in market value, but truth does find a relatively quick acceptance on the market. ...delayed, inaccurate and misleading reports are the tools of the unscrupulous market operator and the recreant corporate officials who speculate on inside information.<sup>2</sup>

Thus, the essential elements of mandated disclosure were present from the bill’s inception. For example, Section 12 of the original bill mandates that all firms listed on an exchange provide:

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<sup>2</sup> HP Rep. No. 1363, 73rd Congress, 2d session

- (2) Annual and quarterly reports, including, among other things, a balance sheet and profit-and-loss statement certified by an independent public accountant;
- (3) Monthly reports including, among other things, a statement of sales or gross income.

The specific disclosure items required of all firms were largely taken from the then-current NYSE listing requirements for new firms; the only additional financial statement disclosure requirement was the disclosure of sales. Management was also expected to provide narrative disclosure concerning its most recent results. And, for some firms, shareholders learned for the first time about insider holdings and executive pay arrangements. These required disclosures were buttressed by provisions giving private and public parties authority to impose penalties should the firm fail to meet its disclosure duties. Indeed, the creation of civil liability drew the most vigorous criticism (Seligman, 1982). Under the Act, civil liability arise arise from misleadings statemensts in a variety of documents, including registration statements for securities, periodical reports, and statements filed by directors, officers or 10 percent stockholders.

Section 18 provided that anyone making a false or misleading statement in a filing would be liable for damages to any one who “purchased or sold a security at a price which was affected by such statement.” Furthermore, in order to encourage such suits and to help alleviate the collective actions problems that would otherwise make a suit unlikely, the Act gave courts the ability to award plaintiff attorney’s fees. The Act also prohibited every “manipulative or deceptive device,” which was interpreted as allowing private parties to sue for misstatements and omissions of material facts. To enforce these rules and develop specific procedures, Congress created the Securities and Exchange Commission, which began to act quickly. Over the next 5 years, roughly 15% of all filings were withdrawn by the company (many because of SEC comment or request) and roughly 4% were ordered withdrawn subject to stop order. The commission was given broad powers to investigate possible violations, conduct inquiries, compel witnesses to provide testimony, sue in court to enjoin certain practices or disclosures, the power to compel compliance, suspend or withdraw licenses of the exchanges themselves, the power to change the rules of exchanges, or suspend trading in securities, and ultimately, in the case of willful violation, criminal sanctions could apply.

The final version of the Act was agreed to by House and Senate conferees on May 30, 1934 and signed by President Roosevelt on June 6, 1934. For largely practical reasons, the SEC

did not initially require new disclosure. Under Rule KC-1, listed companies simply had until September 15, 1934 to provide to the exchange and the Commission “copies of all reports and financial statements which are made available to security holders and/or the exchange at the time they are so made available.” However, by June, 1935, firms were required to file a more detailed Form 10 with the SEC, which required detailed balance sheets, income statements and supporting materials. The reporting requirements applied to all firms trading on national exchanges. Firms that were not listed on any securities exchange (known as over-the-counter or OTC stocks) were exempt until 1964 (Greenstone, Oyer, Vissing-Jorgensen, 2006; Ferrell, 2003)

### **3. Prior research on the 1934 Act**

Other than a cursory market-wide examination in Dolley (1938), we are not aware of any other research that examines whether the Act affected investor information as measured by bid-ask spreads and other liquidity variables. [Add Mahoney] Welker (1995), Healy, Hutton and Palepu (1999) and Leuz and Verrechia (2000) find relationships between a firm’s voluntary disclosure and its bid-ask spread, but a firm’s disclosure policy may be endogenous, making it hard to conclude that observed effects were caused by the disclosure, and moreover the evidence is not informative about the value of mandatory disclosure.

Prior studies of the Act focus on the impact on stock price and returns, measures only indirectly related to the core legislative concern of widely distributing new information. They therefore leave open the central question of whether mandatory disclosure provided investors with new information or reduced information asymmetry. Prior research also does not distinguish between the impact of substantive disclosure provisions and the effect of the enforcement provisions. Partly for this reason, the Council of Economic Advisers has noted that it is unclear whether “SEC enforced disclosure rules actually improve the quality of information that investors receive” (Economic Report of the President, 2003).

Stigler (1964) finds that securities issued just before the advent of SEC regulation have significantly higher returns than those issued just after SEC regulation, though the post-SEC new issues did have a lower variance. Stigler suggests that mandatory disclosure did not improve investor welfare, but instead drives riskier issuers away from the public market. However, Stigler examines only new issues and there is a risk that what he observes is attributable to

differences in the type of firms that went public, the composition of which may have changed for reasons unrelated to the regulation, or to changes in expected returns over time.

Benston (1969 and 1973) examines whether firms' stock prices improved when they were required to disclose sales data. Benston compared the annual stock price returns of firms that voluntarily disclosed data on annual sales (disclosers) with the returns of firms that disclosed this data only when required by the new law (non-disclosers). He finds that non-disclosers did not perform better with the enactment of the Act. He interprets this as evidence that the 1934 Act disclosures provided little value to investors. However, there are several limitations to this study. It is not clear that the best measure of the Act's effect is its net impact on share price. The Act may have resulted in the disclosure of new information, but since disclosure is costly, the costs may offset the benefits, resulting in no net change in stock price. Moreover, Benston examines only NYSE firms, large firms about which there was already substantial information available, while it is possible that the Act had different effects on smaller firms. The consideration, adoption, and implementation of the 1934 Act is a very diffuse event, and Benston faces statistical power issues in the resulting long-term stock returns. Finally, his results are limited to differences in sales disclosure, whereas a richer measure of disclosure may better capture variation between firms.

Simon (1989) examines the dispersion of new issue returns following the 1933 Act, and finds lower variances for all firms following the legislation, but does not examine measures of information asymmetry. By examining different cohorts of new issues, there are also potential time-series and selection bias problems.

Stock price return and volatility are however incomplete measures of the Act's effect and miss a primary goal of the legislation. The Act's primary goal was not to produce new information, but rather to force firms to publicly share information previously held only by insiders, and share price effects are not the best measure of information dispersal. If informed managers, customers or suppliers traded on information prior to the Act, the information would already be impounded in the firm's stock price even if not widely known. Subsequent revelation of this information under mandatory disclosure rules would have little effect on a firm's stock price or volatility, even if the rules made public information that was previously private.

We therefore examine the Act's impact on a more direct measure of information asymmetries, the bid-ask spread. Glosten and Milgrom (1985), Copeland and Galai (1983)

model the trading process where the bid-ask spread reflects the level of private information. The idea is that because informed parties only buy (sell) when the stock is under(over)-priced, specialists face an adverse selection problem. As information asymmetries increase, market makers must widen the spread to prevent losses to informed parties and to recoup losses by profiting more in trades with uninformed parties. Therefore, if the Act increased the amount of information available to specialists and liquidity providers relative to informed insiders, bid-ask spreads should decrease. The intuition is that changes in information environment may not be detectible in changes in stock price, but will change the way trading profits are distributed. Bid-ask spreads should more directly measure this information environment. More generally, we examine liquidity measures which should be correlated with quality of investors' information about the firm's performance.

Bid-ask spreads are measured two different ways. Quoted bid and ask prices at the close of trading are reported in financial newspapers of the day, and the proportional quoted spread is defined as the difference between the bid and ask divided by the average of the bid and ask. We also use the daily time series of transaction price returns to calculate the Roll (1984) estimate of the effective bid-ask spread. This model assumes that the transaction price  $p_t$  differs from the true price  $m_t$  by the half-spread  $s$ :

$$p_t = m_t + q_t s,$$

where  $q$  is a buy-sell indicator equal to 1 if the time  $t$  trade is a buy and  $-1$  if the trade is a sell.

Combining this expression for times  $t$  and  $t - 1$ , we have:

$$\Delta p_t = p_t - p_{t-1} = m_t - m_{t-1} + (q_t - q_{t-1}) s = s \Delta q_t + u_t.$$

Assuming  $q_t$  is i.i.d. over time and orthogonal to the change in the true price, it can be shown that:

$$s = \sqrt{-\text{cov}(\Delta p_t, \Delta p_{t-1})},$$

where the covariance is the first-order autocovariance of price changes. The intuition is that the bigger the spread, the bigger are the reversals on average from trade to trade. In empirical applications, the estimated spread is based on the sample covariance using daily returns and is set to zero if the first-order autocovariance is positive.

The final liquidity measure is the Amihud (2002) measure of illiquidity, which is defined for stock  $i$  on day  $t$  as:

$$I_{it} = \frac{|R_{it}|}{V_{it}},$$

where  $V_{it}$  is the trading volume in dollars and  $R_{it}$  is the return. The measure is undefined on days where there is no trading in stock  $i$ . This measure captures an element of the price impact of trades, as it measures how far the price moves for a given trading volume. Because it is a ratio, it can be quite skewed, particularly if there are days where trading volume is close to zero.

Therefore, to rein in the right tail of the distribution, we take the square root of  $I_{it}$  and use this as the measure of illiquidity.

#### 4. Data, methods, and results

We work with two different samples in the paper. Our initial sample consists of common stocks listed on the New York Stock Exchange (NYSE) or the New York Curb Exchange (Curb), or traded over-the-counter (OTC). There are a large number of OTC stocks, so we limit ourselves to the subsample of industrial OTC stocks that are quoted daily by the Wall Street Journal.

To measure liquidity and gauge the information environment, we collect month-end closing bid and ask prices on all these stocks from 1934 through 1936. These are obtained from the New York Times and Wall Street Journal, with each price double-checked to ensure accuracy. In cases of conflict, we use New York Times data, which have lower error rates. Table 1 Panel A provides overall summary statistics on these stocks during the sample period. In an average month, there are 706 NYSE stocks in our sample, 457 Curb stocks, and 74 OTC stocks.

Table 1 Panel A also provides average share prices and proportional bid-ask spreads for these stocks. Bid-ask spreads are generally somewhat skewed, but this skew was quite substantial during the depths of the depression. Among NYSE stocks, the mean spread is 6.2% during 1934-1936, while the median bid-ask spread is only 2.9%. The widest spread observations tend to come from low-priced stocks, and there were many of these stocks during these trough years. For example, the time-series average of the cross-sectional median NYSE share price for our sample is \$17.45. The minimum tick plays a role here as well. Until 1997, the smallest possible price increment for most listed stocks was 1/8 of a dollar, and a bid and ask

price separated by the smallest possible increment would correspond to a 10% spread on a stock trading at \$1.25 per share.

These spreads are even wider for the smaller-cap stocks that traded on the Curb and OTC. During our sample, Curb stocks have a mean (median) proportional spread of 13.4% (8.2%), with a mean (median) share price of \$14.03 (\$5.81). The OTC stocks in our sample are similarly illiquid but generally have higher share prices: the mean (median) proportional spread is 16.2% (9.5%), while the mean (median) share price is \$40.59 (\$25.40). To a modern observer, these spread levels are somewhat breathtaking. On average, an investor in these OTC stocks would lose roughly a sixth of his portfolio value to transaction costs by buying and selling the same stock at quoted prices within a brief period.

#### **4.1 Information asymmetry and liquidity effects**

Our basic empirical strategy is fairly straightforward. If mandatory disclosure reduces information asymmetries among market participants, it should result in narrower bid-ask spreads, as exchange specialists and other liquidity suppliers worry less about large losses to informed traders with material inside information. Since the 1934 Act applied to firms listed on all national exchanges, the resulting improved disclosure should in principle reduce spreads on all listed stocks, all else equal. Thus, one might look at the time series of aggregate bid-ask spreads on listed stocks to look for a secular decline.

Using daily data on NYSE quoted spreads that has just become available from the Center for Research in Securities Prices (CRSP), Figure 1 shows that there was indeed a decline in aggregate spreads around the consideration, enactment, and implementation of the 1934 Act. Stock prices also rose sharply over the same time period. However, it is extremely difficult to ascribe either of these effects to the change in law, since earnings and dividends were recovering from their deep cyclical trough at the same time, and the American economy and regulatory environment were undergoing a number of wrenching changes during this time period. Stock prices and liquidity tend to be inversely correlated (see, for example, Jones (2003)), and this relationship is clearly present during the 1934-1935 sample period, with a strong trend of declining spreads and rising prices beginning in 1933, and then again beginning in mid-1935. These could in fact be directly related to the 1933 and 1934 Acts and various other measures

designed to build confidence in the nation's securities markets, but it seems impossible to tell with any degree of certainty using only the time-series evidence.

A second approach is to identify treatment and control groups and then look for a differential impact of the legal change. All firms listed on a national securities exchange were subject to the 1934 Act, but firms trading over-the-counter were not subject to the Act's disclosure requirements. Thus, both NYSE and Curb stocks were affected by the new rules, while OTC stocks were not. If increased disclosure is important in reducing information asymmetries among market participants, then bid-ask spreads should reflect the change in the information environment, and liquidity on the NYSE and Curb should improve relative to OTC liquidity. This suggests a simple differences-in-differences approach across exchanges: firms trading on the Curb or the NYSE should see their spreads narrow in comparison to firms trading over-the-counter. We begin with that analysis.

The main challenge in this analysis is pinpointing the exact date at which disclosure actually increased. The 1934 Act was introduced in Congress on February 9, 1934, signed into law on June 6, and implemented gradually by the nascent Securities and Exchange Commission. Firms completed their registration gradually during the spring of 1935. Western Auto was the first NYSE firm to file Form 10 on March 15, 1935. Approximately 160 NYSE firms had filed by April 2, 1935, but many waited until close to the final June 30, 1935 deadline, by which time nearly all NYSE firms had filed. For simplicity, in this section we use the final filing deadline as the event date and examine liquidity for 18 months before and after. To be precise, we define the pre period as January 1934 through June 1935, while the post-event period extends from July 1935 through December 1936.

We use the following panel regression to compare changes in liquidity across exchanges:

$$S_{it} - S_{i,pre} = \beta_0 + \beta_1 DCURB_{it} + \beta_2 DNYSE_{it} + \beta_3 \ln(MKTCAP_{it}) + \varepsilon_{it},$$

where  $S_{it}$  is the proportional spread for stock  $i$  in month  $t$ ,  $S_{i,pre}$  is the average proportional spread for stock  $i$  during the 18-month pre-event period, and  $DCURB_{it}$  and  $DNYSE_{it}$  are indicators set to one if firm  $i$  is listed on the Curb or the NYSE, respectively. Thus, implicitly we are comparing Curb and NYSE stocks to the untreated OTC stocks. While OTC and Curb stocks appear to be quite similar, with similar pre-event spreads and a similar distribution of market capitalizations, NYSE stocks are significantly larger and more liquid. We control for these size differences using the log of market capitalization, measured at the beginning of the pre-event period in



January 1934. Standard errors are clustered by date in order to account for any market-wide common factor in liquidity that might be present.

Before looking at the results, it is useful to discuss our priors. If mandatory disclosure has any value at all, we would certainly expect Curb and NYSE stock liquidity to improve relative to unaffected OTC stocks. In comparing Curb and NYSE stocks, we noted earlier that required disclosures in the 1934 Act were largely modeled on the NYSE's existing disclosure requirements. NYSE firms became subject to some additional disclosure requirements, particularly with respect to detailed accounting statements and information about executives and other insiders. However, the 1934 Act had a much bigger effect on Curb stocks. If the increased disclosure improves the information environment for Curb stocks, we would expect to see a Curb dummy that is more negative than the corresponding coefficient for NYSE stocks. There is some suggestive evidence in Figure 2, which shows the evolution of median spreads over time by exchange. Relative to NYSE stocks, Curb stocks spreads narrow considerably during 1935 as firms comply with the increased disclosure requirements. But Curb stocks are generally smaller and less liquid than NYSE stocks, so one might worry, say, that rising stock prices unrelated to the 1934 Act differentially impact the liquidity of these two groups, hence the control variables in the formal statistical tests.

The panel regression results are reported in Table 2 Panel B. The regressions are run on two different samples: one that includes all stocks, and one that excludes the 30% of stocks that are larger than the largest Curb stock in order to get a set of firms that is more comparable. The latter "matched market cap" sample shows that spreads on both Curb stocks and NYSE stocks narrow significantly in comparison to OTC stocks. Curb stock spreads narrow by 1.32% more than comparable OTC stocks, and NYSE stock bid-ask spreads narrow by 0.84% compared to the OTC controls. Curb stocks exhibit a bigger effect than NYSE stocks; the null hypothesis that these two coefficients are equal is rejected at the 5% level. This is consistent with the hypothesized pecking order: Curb stock liquidity is affected the most by the new rules, NYSE stocks are affected less, and OTC stocks benefit least from the new rules.

When the model is estimated on all stocks, the Curb continues to show a strong liquidity improvement relative to OTC stocks, but NYSE spreads do not narrow much. The coefficient on the NYSE indicator is a modest -0.27%, and we can no longer reject the hypothesis that NYSE stocks and OTC control stocks saw similar changes in liquidity. By using this larger sample we

are trying to compare very disparate stocks: for example, NYSE-listed Pennsylvania Railroad has a 1934 market cap 20 times as large as the largest Curb stock. For these reasons we favor the smaller, more comparable sample. Nevertheless, the result on all stocks is not implausible, since the new disclosure regime was in many ways modeled on NYSE requirements, so that it is possible that NYSE stocks were only modestly affected by the new rules. Overall, though, there is strong evidence that the 1934 Act improves liquidity in the affected stocks.

As noted earlier, the 1934 Act changed both the financial results that were disclosed, as well as the threat of civil and criminal sanctions that were associated with misstating results. It would be interesting to know the relative importance of these different features of the 1934 Act in improving liquidity. In general, however, we do not have much cross-sectional variation, and thus we have little prospect of separating and econometrically identifying the various effects.

As Benston (1969) notes, there is firm-level variation in the disclosure of specific income statement line items, including sales, cost of goods sold, and depreciation expense. In addition, prior to the 1934 Act some firms released audited financials, while others released only unaudited results. For the rest of the paper, we investigate the importance of these firm-specific particular disclosure elements.

In building a sample to do this, we begin with the initial sample discussed above and consider only NYSE stocks that are included in the 1934 Moody's Industrial Manual. Banks, railroads, and utilities were already heavily regulated and typically had to provide detailed disclosure about their business in other regulatory filings. Mandatory disclosure would likely have had little impact on these regulated firms and we omit them. This excludes about one-quarter of the NYSE stocks and about one-half of the Curb stocks. For example, there are 712 stocks listed on the NYSE at the end of 1934, and 558 of these were listed in Moody's Industrials.

Each annual issue of Moody's provides detailed financial data for industrial firms for the previous fiscal year, including income and balance sheet data. Thus, the relevant Moody's issue is 1934, which contains financial data through the end of 1933, the last year before consideration and passage of the 1934 Act. At this time, there were no generally accepted accounting standards, and prior to the 1934 Act, there was considerable variety in the presentation of financial statements, especially the income statement. For each firm, we code three dummy

variables reflecting whether it disclosed sales, cost of goods sold or depreciation in the firm's 1933 income statement.<sup>3</sup>

Moody's also provides data about the audit status of the financial data. It reports the identity of the audit, and it categorizes the nature of the auditor's conclusions. Audits can be completed "with reservations", "with comments", or "with qualifications." We classify any of these as a conditional audit. If there is no discussion of the auditor's conclusions, we classify the audit as clean. Finally, if there is no mention of an audit, we characterize the results as unaudited. Most firms received a clean audit in 1933.

We combine the Moody's data with two other data sources. First, CRSP provides stock return, capitalization, volume, bid-ask spread, and industry data for all NYSE common stocks. Second, in order to pinpoint the exact date when firms come into compliance with the increased disclosure requirements, we hand collect the date of each firm's initial Form 10 filing in 1935. This required a lengthy process of combing through SEC files now stored at the National Archives, and our research assistants were able to locate Form 10 filings and identify initial filing dates for 172 NYSE firms that also had disclosure information in Moody's. Of these, 34 firms are removed from the sample because they trade on fewer than half the trading days during 1935, leaving a total of 138 firms.

Table 1 Panel B provides summary statistics for these firms partitioned by disclosure status. Firms that disclose 1933 sales are slightly less numerous (64 vs. 74), and they tend to be somewhat smaller (\$46.1 million average market cap vs. \$73.6 million for non-disclosers) and less liquid (3.52% average proportional spread vs. 2.63% for non-disclosers). Only a few firms (9 out of 138) did not disclose depreciation expense, which is consistent with Benston (1969), and only a few firms (11 out of 138) did not report an audit in 1933.

Table 1 Panel B also reveals evidence that supports our signaling story. Firms that disclose sales tend to be smaller, with a mean market cap of \$46.1 million vs. \$73.6 million for non-disclosers. Sales disclosers are also less liquid, with a mean quoted spread of 3.52% vs. 2.63% for non-disclosers and slightly less trading volume (\$46.7 million of trading per day for disclosers vs. \$48.9 for non-disclosers). This suggests that disclosing firms have more severe

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<sup>3</sup> A spot check indicated that this disclosure policy was nearly perfectly persistent. Firms did not choose to disclose in one year and then become more opaque the next, or vice versa.

information asymmetries and thus more incentive to convey information about financial performance. There are similar results for depreciation disclosers vs. non-disclosers. During this time period, audits are also consistent with a signaling story. Firms without an audit in 1933 are much larger and more liquid than audited firms, suggesting that these firms has less need for the outside verification provided by an audit.

What elements of required disclosure are important? To answer this question, we focus our attention in places where there was heterogeneity prior to the new requirements. Specifically, we look at specific income statement disclosure requirements as well as the requirement for an audit. We look at partitions based on 1933 sales disclosure, cost-of-goods-sold disclosure, depreciation expense disclosure, and audit status.

Table 3 tests for differential information and liquidity effects across the disclosure groups using a simple difference-in-difference approach. We use the sample of 138 NYSE industrial firms where we know the exact date of the initial Form 10 filing in 1935, and for each firm we calculate information environment measures during calendar year 1935 both pre- and post-filing.<sup>4</sup> For each stock in each period we calculate three different liquidity measures: the average proportional bid-ask spread based on closing quotes, the effective bid-ask spread using the Roll (1984) approach, and the Amihud illiquidity measure.

The basic idea of the difference-in-difference approach is that if the treatment and control groups are identical, we can identify the treatment effect by comparing the changes in the liquidity measures. However, as noted earlier, the disclosers and non-disclosers are not identical, as disclosers tend to be smaller and less liquid, which suggests they may have more incentive to provide fuller disclosure. This can be seen in Table 3, where during the pre-filing period disclosers are always less liquid than non-disclosers, and audited firms are similarly less liquid than unaudited firms.

After the initial 1934 Act Form 10 filing, liquidity improves for both disclosers and non-disclosers. For quoted spreads and the Amihud illiquidity measure, the average change is almost always statistically significant. In contrast, the Roll spreads do not always reject the null of no change. This is almost certainly due to low power for these measures. Unlike the quoted spread,

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<sup>4</sup> Recall that the earliest filing date is March 15, 1935, and almost all NYSE firms filed by the June 30, 1935 deadline. Since the sample period extends through all of 1935, there is slightly more post-filing data than pre-filing data. The results are identical if we add more pre-filing data by starting the pre-filing period at the beginning of the fourth quarter of 1934.

the Roll measure is not available on a daily basis. Instead, we must use the entire pre- or post-filing time series of daily returns for an individual stock to calculate the Roll measure, and the result tends to be quite noisy (see, for example, Hasbrouck, 2006). Because of this low power, we do not focus on the Roll measure from now on.

As a concrete example, consider quoted spreads based on previous sales disclosure policy. The basic results are similar for other disclosure partitions and for the Amihud measure. The average proportional spread in 1935 for sales disclosers is 4.41% before filing and 3.10% afterward, a narrowing of 1.31%. Non-discloser spreads narrow from 3.12% to 2.39%, a 0.72% change, and the difference-in-difference is significantly different from zero. Disclosers narrow more than non-disclosers, which means that untreated firms improve more than treated firms. These results are in some sense backward and certainly provide no support for the simple hypothesis that improvements in information (and thus increases in liquidity) were positively related to these particular elements of the new mandates.

These results are similar in the broader sample. For the 706 NYSE firms during 1933-1935 where we have monthly quoted spreads, Figure 4 shows that sales disclosers are slightly less liquid than non-disclosers, and there is little difference in how their average spreads evolve. Figure 5 provides a similar graph partitioned by audit status, and it indicates that with the imposition of mandatory audits, previously unaudited firms did not see their liquidity improve as much as audited firms (in results not reported, we find that the difference is not statistically significant).

The simple differences-in-differences approach has a number of weaknesses. First, it does not allow simultaneous consideration of all the disclosure variables. Second, it does not easily accommodate other control variables, thereby implicitly assuming that the two samples are identical, except for an arbitrary aggregate fixed effect. Finally, the simple approach is unable to make use of the staggered filing dates for identification of the filing effect.

To overcome these weaknesses, we turn to a regression approach using a daily panel of NYSE stocks during 1935. We have various liquidity measures for each NYSE stock each day, and we know the exact date the firm files initially under the 1934 Act. Identification of the liquidity effect of filing is based on the following specification or subsets thereof:

$$L_{it} = \alpha_i + \gamma_t + \beta_0 DPOST_{it} + \beta_1 DPOST_{it} DSLS_i + \beta_2 DPOST_{it} DDEP_i + \beta_3 DPOST_{it} DAUD_i + \varepsilon_{it},$$

where  $L_{it}$  is the liquidity measure for stock  $i$  on day  $t$ ,  $DPOST_{it}$  is an indicator set to one if firm  $i$  has filed under the 1934 Act on day  $t$ ,  $DSLS_i$  and  $DDEP_i$  are indicators set to one if firm  $i$  discloses its 1933 sales or depreciation, respectively, and  $DAUD_i$  is set to one if firm  $i$  is audited in fiscal 1933. The liquidity measure is either the quoted proportional bid-ask spread at the close, the Amihud square-root illiquidity measure based on the day's return and trading volume, or the trading volume in thousands of shares. There is a firm fixed effect which eliminates the need for stock-specific control variables, and in some specifications there is also a calendar day fixed effect. Standard errors are clustered by date.

The results can be found in Table 4. We start with Regression A, a simple specification that includes only the filing dummy and fixed effects. With a firm fixed effect, this matches the earlier evidence. Quoted spreads, for example, are on average 1.19% narrower after the initial 1934 Act filing, Amihud illiquidity declines, and trading volume increases substantially. But there is a strong secular trend at work here. Over the course of 1935, average spreads decline for all subsets of stocks, and the cross-sectional dispersion in spreads seems to decline as well. This suggests that there is a common factor in liquidity. In modern data, Huberman and Halka (2001), Hasbrouck and Seppi (2001) and Chordia, Roll and Subrahmanyam (2000) have documented a strong common factor in liquidity, so it is unsurprising to find this relationship in older spread data. As discussed earlier, improved liquidity over the course of 1935 could be due to broad-based effects of increased disclosure, but perhaps it is due to other macroeconomic improvements such as the improved financial health of banks and other financial intermediaries. Looking more closely, we can use the staggered filing dates to determine if the 1934 Act filing itself improves liquidity. Once this is done by adding calendar day fixed effects, there is no significant improvement in liquidity upon filing, and in fact there is a significant decline in trading volume for firms that have filed vs. firms that have not yet filed. Thus, there is no evidence that this particular filing immediately improves liquidity.

Regressions B, C, and D add interacted dummies one at a time for sales disclosure, depreciation disclosure, and audit status. All of the dummies are combined in Regression E, where we can identify the marginal effects of each individual type of disclosure. The results are quite similar to those discussed earlier. Again, for concreteness we use the quoted spread figures; other liquidity variables generate similar results. When there is no calendar fixed effect, the insignificant panel regression intercept of  $-0.01\%$  indicates that a sales and depreciation non-

discloser without an audit ( $DSLS_i = DDEP_i = DAUD_i = 0$ ) experiences virtually no change in quoted spreads after its initial 1935 filing. Note that such a firm is affected most by the 1934 Act requirements, as it must begin to disclose its income statement items and obtain an auditor's opinion. In contrast, audited firms that previously disclosed sales but not depreciation ( $DDEP_i = 0, DSLS_i = DAUD_i = 1$ ) see their spreads narrow by an average of  $0.30\% + 0.33\% = 0.63\%$  after filing, and audited firms that disclose both sales and depreciation ( $DSLS_i = DDEP_i = DAUD_i = 1$ ) experience a narrowing of 1.42% on average. The coefficients on the interacted disclosure dummies do not change much at all when a calendar fixed effect is added, but the intercept becomes substantially positive, indicating that non-disclosers' relative liquidity is actually hurt upon filing. For example, an unaudited non-discloser sees its quoted spread widen by 1.29% relative to other firms upon its initial 1934 Act filing. An audited full discloser sees its spread relative to other firms change by  $1.29\% - 0.30\% - 0.77\% - 0.34\% = -0.12\%$  upon filing, which is statistically indistinguishable from zero.

While the evidence might seem puzzling at first glance, it is consistent with the signaling story outlined earlier. For certain firms, such as small or less visible firms, investors may have little information beyond that conveyed by the firm directly. Faced with these severe information problems, such firms have a strong need to credibly convey their financial state and choose to disclose voluntarily. While their spreads are narrower than they would be had they not disclosed, they remain wider than the spreads of non-disclosers due to the endowed information asymmetry. If penalties associated with the 1934 Act make the post-1934 information more credible, the information asymmetries for disclosers are markedly reduced. For non-disclosers, information asymmetries were not as severe beforehand, and the enforcement provisions of the 1934 Act did not substantially improve the quality or credibility of the information supplied by these firms.

For the results with calendar fixed effects, another possible explanation is that, at least for these specific disclosures, the previous voluntary disclosure regime was optimal. Previous disclosers did not have to change their income statement disclosure behavior, and they did not see a change in liquidity after their first 1934 Act filing. In contrast, the relative decline in non-discloser liquidity might indicate that these firms were forced away from their optimum by the additional disclosure requirements. We wouldn't expect the new disclosure requirements to actually hurt liquidity, and they don't in absolute terms, but simply suffer relative to prior disclosers. This

suggests that there isn't much benefit from filing for these firms. Perhaps they did not disclose previously because there are costs. And we next look at stock price effects to see whether investors considered the disclosures on balance valuable.

## **4.2 Returns**

We next look at stock price behavior. While Benston (1973) and Simon (1989) look at a very diffuse event period around the adoption and implementation of the 1934 Act, we closely examine the stock price reaction to the initial 1934 Act filing event by various firms. We look at the whole sample of NYSE stocks as well as subsamples based on 1933 sales disclosure, depreciation disclosure, and audit status. Excess returns are market-adjusted and value-weighted. Figure 3 displays the cumulative excess returns along with bands that reflect two standard errors. For the most part, there are no statistically significant excess returns on the initial filing day or at anytime thereafter. The only exception is previously unaudited firms. When these firms file their Form 10 in 1935, the stock price reaction is positive, with excess returns of about 4% over the next five trading days. This result is marginally significant and suggests that for these firms investors are pleased either by the financial results or perhaps simply by the fact that these firms have decided to obtain audited results and otherwise comply with the 1934 Act.

Of course, in an efficient market stock prices incorporate expectations, and stock returns reflect only surprises, or equivalently the arrival of new information. If the market expected each of these firms to file, and the market was not surprised on average by the contents of the filings, then that would explain an excess return of zero on these initial filing days. Since the 1934 Act was proposed, considered, and enacted during 1934, one might expect any return effect to be found there, because 1934 is probably when the market changed its expectations about future disclosure policy. As noted above, the 1934 Act was introduced in February 1934 and after modest revision passed both houses of Congress by the end of May 1934. During this time period, market participants would have gradually realized that the bill would be passed, and an efficient market would have evaluated the effect of the 1934 Act on particular stocks. As noted above, the precise nature of the required disclosures was unknown during this time period, but given the language in the bill, during the Feb-May period participants would have revised upward the probability of more detailed income statement disclosure and more stringent audit



requirements. Thus, in unreported results, we look at returns from Feb 1934 through May 1934, again partitioned by prior year disclosure policy, but we find no cross-sectional difference in returns. However, it is worth noting that this exercise probably still has fairly low statistical power, since we cannot refine the event date any more precisely, and the event date is common across stocks.

### 4.3 Idiosyncratic risk

Morck, Yeung, and Yu (2000) suggest an alternative measure of the information environment in pricing stocks. They argue that better public investor property rights lead to more trading on firm-specific information, which yields less comovement between stocks. Their ideas easily extend to this disclosure framework. If mandatory disclosure increased the precision of firm-specific information, affected stocks would exhibit more idiosyncratic return variance. If mandatory disclosure affects some stocks and not others, we would expect to see an increase in firm-specific variance in the affected stocks but not in the other stocks (Fox, Durnev, Morck, and Yeung, 2004).

In this case, we look at differences-in-differences for idiosyncratic returns. We partition the sample by one of the four disclosure status variables (sales, cost-of-goods sold, depreciation expense, or audit status) and compare idiosyncratic returns before and after their initial filing under the 1934 Act. To be precise, for each stock in each subperiod, we estimate a standard market model regression on daily returns:

$$R_i = \alpha + \beta R_{mt} + \varepsilon_{it}.$$

The  $R^2$  of this regression is the importance of the single common factor, and  $1 - R^2$  of this regression is the fraction of return variance that is idiosyncratic, which is the quantity of interest. We average these  $1 - R^2$  quantities across all stocks in a particular disclosure category, and we then look at differences in differences.

The results are at the far right of Table 3. In general, idiosyncratic returns become more important in the post period, with idiosyncratic return fractions going from about 75% in the pre period to about 80% in the post period. These increases are not always statistically significant, because like the Roll effective spread measure, this measure requires a long time-series to calculate and as a result is quite noisy. More importantly, there is no evidence that the effect is any different for income statement disclosers than for non-disclosers. None of the differences-

in-differences are statistically different from zero. Again, we find no evidence that the 1934 Act improved the information environment more for non-disclosers or unaudited firms.

## **5. The effects of 1933 Act disclosure**

One year before the 1934 Act imposed ongoing mandatory disclosure on firms whose securities were being publicly traded, the Securities Act of 1933 put in place a similar registration and disclosure regimen that applied to the initial issuance of public securities. As an additional test of the effects of mandatory disclosure, we examine changes in liquidity at the time firms made public disclosures in preparation for a public offering. We focus on debt issues, as equity or equity-linked issues tend to be a negative signal about firm value and might also signal severe information asymmetries, leading a specialist or other liquidity supplier to widen bid-ask spreads as a defensive measure. More recent empirical evidence indicates that new debt issues do not convey much of a signal about firm value on average.<sup>5</sup>

Using new securities listings from the Commercial and Financial Chronicle, along with New York Times and Wall Street Journal articles, we identify 19 firms that filed registration statements for the first time with the SEC in 1935 under the 1933 Act in order to issue new public debt. Some firms filed to issue debt and equity at the same time, but we exclude these issues because of the potential signal implicit in an equity issue. Some firms made multiple 1933 Act filings in 1935 for additional issuances, but we exclude all subsequent filings made by a firm in 1935, as these are unlikely to contain much new information. We look at returns and spreads within 50 days of the 1933 Act filing, and the results are in Table 5.

The quoted spread shrinks from an average of 1.80% to 1.70% after the filing, and this is a significant change, but the Amihud illiquidity measure does not improve significantly for these filers. However, as discussed earlier, spreads were generally narrowing during 1935, and perhaps this decline in issuing firm spreads is simply indicative of the broader improvement in liquidity. To control for this, each day for each of the 19 filing firms we take the cross-section of all NYSE proportional spreads and Amihud illiquidity measures and determine the quantile of

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<sup>5</sup> We would like to compare firms issuing public debt to firms that privately placed new debt. Unfortunately, there are very few private placements of straight debt during this time period, and we are unable to draw any meaningful inference from the handful of firms that went this route.

the issuing firm. A quantile of zero is the narrowest proportional spread or the smallest Amihud measure, while a quantile of one means the firm in question has the widest proportional spread or the highest Amihud illiquidity measure among all NYSE stocks. We then follow the evolution of this quantile over the event period to see if the issuing firm moves around in the cross-sectional distribution after filing a registration statement. If the 1933 Act filings improve liquidity for these firms compared to other firms, we should see a decline in these quantile measures. There is no significant decline in the quantile measures, and in fact if anything firms filing under the 1933 Act experience wider quoted spreads afterward compared to other firms. Note that, on average, the firms issuing debt are more liquid than the typical firm. This suggests a similar selection effect: especially during this period of new regulation, firms might select the public markets only if the increased disclosure is relatively less onerous for them, perhaps because the relevant information is already available to the market.

We also look at returns around these new debt issues. There are substantial stock price increases after the 1933 Act filing, from event day +4 to event day +50. In fact, debt issuer stock prices rise an average of 20% over this short period, which is strongly distinguishable from zero. However, stock prices are generally rising over this period, and when we simply subtract off the value-weighted market return, the average post-filing excess return becomes insignificant. Figure 6 shows this graphically, and while the cumulative excess return becomes significantly positive at the 5% level for an instant around event day +40, overall there is no reliable evidence that a 1933 Act debt filing during this time period affects either stock prices or bid-ask spreads.

## **6. Disclosure and Informational Externalities**

Disclosure by one firm may indirectly provide information about others. For example, if US Steel reports an increase in operating margins, this most likely implies an increase in operating margins for other steelmakers, and investors would update their earnings and cash flow expectations accordingly for these other steelmakers. Thus, the information environment may be relatively rich in industries where most companies practice detailed disclosure. For firms in industries that do not disclose as fully, investors may be able to accurately infer the missing disclosures. In such industries, one might expect a relatively small effect if disclosure mandates affect only a few firms. In contrast, we would expect to see the biggest effects in previously opaque industries where the disclosure mandates affect all firms.

To investigate this, we assign each of our NYSE firms to one of 48 industries based on the Fama and French (1997) classification scheme. For each industry and each type of disclosure, we calculate the fraction of firms in that industry that disclose.  $FSLS_i$  is the fraction of firms in industry  $i$  that disclose sales in 1933,  $FDEP_i$  is the fraction that disclose depreciation expense in 1933, and  $FAUD_i$  is the fraction of firms in industry  $i$  that were audited in 1933. These variables are interacted with the filing dummy  $DPOST_i$  in a daily panel covering all of 1935. To take sales disclosure as an example, the estimated regression is:

$$L_{it} = \alpha_i + \gamma_t + \beta_0 DPOST_{it} + \beta_1 DPOST_{it} DSLS_i + \beta_2 DPOST_{it} FSLS_i + \varepsilon_{it},$$

where  $L_{it}$  is the liquidity measure for stock  $i$  on day  $t$ , either the quoted proportional bid-ask spread at the close or the Amihud square-root illiquidity measure based on the day's return. Note that there is a firm fixed effect and in some specifications a calendar day fixed effect. Similar regressions are estimated for depreciation disclosure and audit status. Standard errors are based on time-series independence.

If these disclosure externalities are important, we would expect the biggest spread declines for firms in industries with little previous disclosure (low  $FSLS$ ). Thus, we would expect declines to be decreasing in  $FSLS$ , or equivalently spread changes should be increasing in  $FSLS$ . The results are in regression A of Table 6, and at least for sales there is evidence consistent with a disclosure externality. On filing in a hypothetical industry with no sales disclosers, spreads narrow by 73 basis points compared to the effect in a hypothetical industry where every firm discloses. The greater the fraction of sales-disclosing firms in an industry, the less the spread narrows on filing.

There is another possible interpretation of this evidence in light of our signaling story. The value of the signal could vary depending on what others do. The selection bias is strongest (a firm must benefit from disclosure the most) if it previously disclosed sales while nobody else in the industry did. In that case, with  $DSLS = 1$  and  $FSLS$  close to zero, spreads narrow significantly after filing. In contrast, if everybody in an industry discloses already, there doesn't seem to be much differential effect from filing. The net of the two coefficients (-0.57% + 0.73%) is about zero, suggesting that the signaling story is much less important when disclosure is part of normal industry practice.

These results do not extend to the industry fractions for depreciation disclosure and audit status (regressions B and C). The relevant coefficients are probably insignificant because most

of the sample firms disclose depreciation and audit status. This means that both *FDEP* and *FAUD* are typically quite close to one, with little variation across industries, and little statistical power as a result.

## **7. Conclusion**

In this paper, we study the effect of the Securities Exchange Act of 1934 on bid-ask spreads and other measures of the information environment surrounding common stocks. We show that bid-ask spreads narrow and stock prices climb as the 1934 Act is considered and implemented, but given the economic tumult of the time it is impossible to ascribe these liquidity changes purely to the 1934 Act. To identify the effects of the Act, we focus instead on cross-sectional differences. Prior to the 1934 Act, for example, the OTC and the Curb had similar types of stocks with similar liquidity. The Act applied to the Curb, not the OTC, and liquidity improves more for Curb stocks. Curb stocks are affected more than NYSE stocks by the new rules, and we find that Curb liquidity improves much more than NYSE liquidity. All these results are consistent with the hypothesis that the 1934 Act had salutary effects on the information environment for publicly traded U.S. firms.

What parts of the Act were important? Disentangling the effects is difficult, because there were multiple, simultaneous changes that applied to all listed firms. However, there were some previous cross-sectional differences in voluntary disclosure that we use for identification. Even among NYSE firms, some firms disclosed sales or depreciation prior to the 1934 Act, and some chose not to. Some firms reported audited financials; some did not. The 1934 Act mandated audited financials, including a complete income statement and balance sheet.

When we partition the sample by previous disclosure status, there is no evidence that previous non-disclosers were helped by the initial 1934 Act filing. These firms experience no improvement in liquidity upon filing, and there is no stock price reaction to the filing. There is no evidence therefore that the information the Act forced investors to disclose was useful to investors or was successful at reducing informational asymmetries. In contrast, previous disclosers saw significant liquidity improvements. This evidence is consistent with a signaling story, where these firms faced more severe information problems and wanted to credibly signal their financial state. These firms found that the 1934 Act improved the credibility of their

disclosures, consistent with the idea that the public and private enforcement mechanisms created by the Act allowed investors to credibly signal the accuracy of their disclosures.

A very small number of firms chose not to comply with the provisions of the 1934 Act and delisted from all national exchanges. In future work, we intend to examine these firms in some detail. We expect to see sharp price declines and a worsening of liquidity when these firms announce and implement the delisting, indicating that on balance the market valued the new regulatory regime.

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**Table 1. Summary statistics.** Time-series averages of cross-sectional means, medians, and standard deviations. Panel A is month-end data 1934-1936 for common stocks listed on the New York Stock Exchange, the New York Curb Exchange, and active stocks quoted over-the-counter. Panel B is daily data during 1935 for NYSE common stocks.

Panel A: Monthly 1934-1936 Cross-exchange sample								
	<u>N</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>				
<u>Share price</u>								
NYSE	706	26.77	17.45	32.67				
Curb	457	14.03	5.81	21.04				
OTC	74	40.59	25.40	53.16				
<u>Market capitalization (\$millions)</u>								
NYSE	706	44.8	7.8	141.0				
Curb	457	8.7	1.4	33.8				
OTC	74	8.7	2.1	24.6				
<u>Proportional bid-ask spread</u>								
NYSE	706	6.2%	2.9%	10.0%				
Curb	457	13.4%	8.2%	15.3%				
OTC	74	16.2%	9.5%	17.3%				
Panel B: Daily 1935 NYSE Sample								
	<u>Market cap (\$millions)</u>				<u>Proportional spread</u>			
	<u>N</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>N</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>
1933 sales disclosers	64	46.1	13.3	114.9	64	3.52%	1.94%	5.36%
1933 sales non-disclosers	74	73.6	18.2	199.7	74	2.63%	1.73%	3.39%
1933 depreciation disclosers	129	60.4	15.0	169.2	129	3.11%	1.85%	4.75%
1933 depreciation non-disclosers	9	68.3	20.6	117.6	9	2.07%	1.46%	1.88%
Audited in 1933	127	51.8	15.1	139.9	127	3.10%	1.83%	4.47%
No 1933 audit	11	167.4	29.5	342.8	11	2.40%	1.71%	3.97%
	<u>Trading volume (\$millions)</u>				<u>Daily return</u>			
	<u>N</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>N</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>
1933 sales disclosers	64	46.7	13.0	91.2	64	0.15%	0.02%	2.58%
1933 sales non-disclosers	74	48.9	13.6	104.5	74	0.15%	0.01%	2.33%
1933 depreciation disclosers	129	47.2	12.9	100.7	129	0.15%	0.01%	2.50%
1933 depreciation non-disclosers	9	57.4	24.9	80.1	9	0.09%	-0.02%	1.93%
Audited in 1933	127	44.5	13.3	88.3	127	0.15%	0.01%	2.51%
No 1933 audit	11	86.7	13.2	178.9	11	0.10%	0.02%	1.85%

**Table 2. Comparison of 1934 Act liquidity effects across exchanges.** Panel A displays median proportional quoted spreads, measured monthly, on NYSE, Curb, and a sample of over-the-counter (OTC) stocks. The pre-event period is Jan 1934 through Jun 1935, and the post-event period is Jul 1935 through Dec 1936. Panel B reports panel regressions for each stock and month from Jul 1935 through Dec 1936. The dependent variable is the stock's change in proportional spread relative to its average value in the pre-event period. Matched market caps excludes NYSE and OTC stocks that are larger than the largest Curb stock. Standard errors are in parentheses below the means and are based on independence over time. Regression coefficients and changes that are reliably different from zero at the 10%, 5%, and 1% levels are denoted by one, two, and three asterisks respectively.

Panel A: Spreads before and after July 1, 1935			
	<u>Pre</u>	<u>Post</u>	<u>Change</u>
NYSE	0.0373 (0.0017)	0.0207 (0.0009)	-0.0166 *** (0.0019)
Curb	0.1116 (0.0044)	0.0523 (0.0030)	-0.0593 *** (0.0053)
OTC	0.1168 (0.0067)	0.0731 (0.0020)	-0.0437 *** (0.0070)

Panel B: Spread change panel regressions			
	<u>Full sample</u>	<u>Matched Mkt Caps</u>	
Intercept	-0.1951 *** (0.0155)	-0.2518 *** (0.0229)	
D(Curb)	-0.0136 *** (0.0032)	-0.0132 *** (0.0036)	
D(NYSE)	-0.0027 (0.0021)	-0.0084 *** (0.0031)	
Ln(mktcap)	0.0176 *** (0.0016)	0.0257 *** (0.0026)	

**Table 3. 1934 Act liquidity effects: NYSE disclosers vs. non-disclosers.** NYSE stocks partitioned by disclosure status in 1933. Bid-ask spreads are proportional, the Roll spread and Amihud square-root illiquidity measure (x 100) are calculated using daily transaction returns, and the idiosyncratic return fraction is the cross-sectional average  $1 - R^2$  in a regression of daily individual stock returns on current and lagged value-weighted market returns. The sample period is 1935, before and after a firm files initially under the 1934 Act. Standard errors are in parentheses below the means and are based on independence over time (quoted spreads and illiquidity measure) or based on independence across firms (Roll effective spreads and idiosyncratic return fractions). Significance at the 10%, 5%, and 1% levels is denoted by one, two, and three asterisks respectively. The difference-in-difference should be positive for spreads and the Amihud measure and negative for the idiosyncratic return fraction if the 1934 Act improved the information environment more for nondisclosers and unaudited firms.

Disclosure Status	Quoted Bid-Ask Spreads				Roll Effective Bid-Ask Spreads				Amihud Illiquidity Measure				Idiosyncratic Return Fraction			
	Pre	Post	Change		Pre	Post	Change		Pre	Post	Change		Pre	Post	Change	
Sales	0.0441 (0.0008)	0.0310 (0.0006)	-0.0131 (0.0011)	***	0.0159 (0.0019)	0.0135 (0.0017)	-0.0024 (0.0026)		0.5857 (0.0099)	0.4809 (0.0058)	-0.1048 (0.0117)	***	0.7422 (0.0239)	0.8122 (0.0199)	0.0701 (0.0311)	**
No Sales	0.0312 (0.0007)	0.0239 (0.0004)	-0.0072 (0.0008)	***	0.0151 (0.0017)	0.0103 (0.0012)	-0.0048 (0.0021)	**	0.5342 (0.0947)	0.4773 (0.0542)	-0.0569 (0.0111)	***	0.7290 (0.0198)	0.8178 (0.0154)	0.0887 (0.0251)	***
		Difference	-0.0059 (0.0012)	***		Difference	0.0024 (0.0033)			Difference	-0.0479 (0.0119)	***		Difference	-0.0187 (0.0400)	
COGS	0.0413 (0.0009)	0.0315 (0.0008)	-0.0098 (0.0013)	***	0.0171 (0.0024)	0.0121 (0.0020)	-0.0050 (0.0031)		0.5617 (0.0117)	0.4768 (0.0072)	-0.0848 (0.0138)	***	0.7194 (0.0325)	0.7965 (0.0258)	0.0771 (0.0415)	*
No COGS	0.0357 (0.0006)	0.0256 (0.0004)	-0.0101 (0.0008)	***	0.0149 (0.0015)	0.0117 (0.0012)	-0.0032 (0.0019)	*	0.5552 (0.0050)	0.4796 (0.0101)	-0.0755 (0.0126)	***	0.7406 (0.0173)	0.8218 (0.0140)	0.0813 (0.0223)	***
		Difference	0.0003 (0.0012)			Difference	-0.0018 (0.0037)			Difference	-0.0093 (0.0126)			Difference	-0.0041 (0.0471)	
Depr	0.0382 (0.0006)	0.0277 (0.0004)	-0.0105 (0.0008)	***	0.0159 (0.0013)	0.0120 (0.0011)	-0.0039 (0.0017)	**	0.5605 (0.0086)	0.4808 (0.0050)	-0.0798 (0.0101)	***	0.7340 (0.0160)	0.8147 (0.0130)	0.0807 (0.0206)	***
No Depr	0.0241 (0.0006)	0.0187 (0.0004)	-0.0054 (0.0008)	***	0.0084 (0.0030)	0.0083 (0.0041)	-0.0001 (0.0050)		0.5143 (0.0150)	0.4506 (0.0094)	-0.0637 (0.0173)	***	0.7506 (0.0538)	0.8228 (0.0394)	0.0723 (0.0667)	
		Difference	-0.0051 (0.0011)	***		Difference	-0.0038 (0.0053)			Difference	-0.0161 (0.0181)			Difference	0.0084 (0.0698)	
1933 Audit	0.0382 (0.0006)	0.0275 (0.0004)	-0.0107 (0.0007)	***	0.0153 (0.0013)	0.0120 (0.0011)	-0.0033 (0.0017)	*	0.5666 (0.0083)	0.4796 (0.0049)	-0.0870 (0.0098)	***	0.7320 (0.0159)	0.8140 (0.0127)	0.0821 (0.0202)	***
No Audit	0.0260 (0.0027)	0.0230 (0.0018)	-0.0030 (0.0033)		0.0167 (0.0054)	0.0087 (0.0032)	-0.0080 (0.0063)		0.4498 (0.0147)	0.4686 (0.0103)	0.0189 (0.0184)		0.7712 (0.0560)	0.8289 (0.0610)	0.0577 (0.0829)	
		Difference	-0.0077 0.0032	**		Difference	0.0047 (0.0065)			Difference	-0.1059 (0.0168)	***		Difference	0.0243 (0.0853)	

**Table 4. 1934 Act liquidity effects from panel regressions.** Identification of the liquidity effects of initial filing under the 1934 Act for a daily panel of NYSE common stocks during 1935 using the following specification:

$$L_{it} = \alpha_i + \gamma_t + \beta_0 DPOST_{it} + \beta_1 DPOST_{it} DSLS_i + \beta_2 DPOST_{it} DDEP_i + \beta_3 DPOST_{it} DAUD_i + \varepsilon_{it},$$

where  $L_{it}$  is the liquidity measure for stock  $i$  at time  $t$ ,  $DPOST_{it}$  is an indicator set to one if firm  $i$  has filed under the 1934 Act at time  $t$ ,  $DSLS_i$  and  $DDEP_i$  are indicators set to one if firm  $i$  discloses its 1933 sales or depreciation, respectively, and  $DAUD_i$  is set to one if firm  $i$  is audited in fiscal 1933. Bid-ask spreads are proportional, Amihud square-root illiquidity measures (x 100) are calculated using daily transaction returns, and trading volume is in thousands of shares. Standard errors are in parentheses and are based on independence over time. Significance at the 10%, 5%, and 1% levels is denoted by one, two, and three asterisks respectively.

Regression		Quoted Spread		Amihud Illiquidity Measure		Trading Volume	
A	DPOST	-0.0119 *** (0.0007)	0.0015 (0.0015)	-0.1263 *** (0.0099)	0.0226 * (0.0121)	0.7518 *** (0.0776)	-0.2222 *** (0.0791)
	DPOST * DSLS	-0.0039 *** (0.0011)	-0.0039 *** (0.0011)	-0.0329 *** (0.0108)	-0.0323 *** (0.0108)	0.2802 *** (0.0511)	0.2781 *** (0.0512)
B	DPOST	-0.0101 *** (0.0008)	0.0033 ** (0.0016)	-0.1117 *** (0.0107)	0.0368 *** (0.0127)	0.6220 *** (0.0729)	-0.3499 *** (0.0763)
	DPOST * DDEP	-0.0093 *** (0.0009)	-0.0091 *** (0.0009)	-0.0747 *** (0.0184)	-0.0719 *** (0.0181)	0.3632 *** (0.0628)	0.3481 *** (0.0631)
C	DPOST	-0.0032 *** (0.0006)	0.0097 *** (0.0014)	-0.0571 *** (0.0168)	0.0876 *** (0.0198)	0.4137 *** (0.0717)	-0.5374 *** (0.0789)
	DPOST * DAUD	-0.0069 ** (0.0030)	0.0064 * (0.0036)	-0.0509 *** (0.0166)	0.0948 *** (0.0176)	0.7788 *** (0.1337)	-0.1781 (0.1258)
D	DPOST	-0.0001 (0.0024)	0.0129 *** (0.0030)	-0.0056 (0.0205)	0.1374 *** (0.0224)	0.4786 *** (0.1115)	-0.4616 *** (0.1055)
	DPOST * DSLS	-0.0030 *** (0.0011)	-0.0030 *** (0.0011)	-0.0213 * (0.0114)	-0.0211 * (0.0114)	0.2736 *** (0.0526)	0.2740 *** (0.0527)
E	DPOST	-0.0079 *** (0.0011)	-0.0077 *** (0.0012)	-0.0574 *** (0.0185)	-0.0551 *** (0.0183)	0.3147 *** (0.0687)	0.3021 *** (0.0700)
	DPOST * DAUD	-0.0033 (0.0031)	-0.0034 (0.0031)	-0.0630 *** (0.0176)	-0.0617 *** (0.0175)	-0.0160 (0.0990)	-0.1774 * (0.0936)
	Firm fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
	Date fixed effect	No	Yes	No	Yes	No	Yes

**Table 5. Effects of 1933 Act disclosures on returns and spreads of firms issuing debt.** The sample consists of 19 NYSE firms issuing debt during 1935 and filing registration statements for the first time under the Securities Act of 1933. The sample extends from 50 trading days prior to the filing to 50 trading days afterward. Liquidity measures and returns are averaged cross-sectionally for each event date. Panel A reports the results of the time-series regression

$$L_t = \alpha + \beta D_{post} + \varepsilon_t,$$

where  $L_t$  is the specified liquidity measure and the indicator variable  $D_{post} = 1$  on event day zero and afterward. In Panel B, excess returns are relative to the value-weighted market return. In both panels, standard errors in parentheses are based on time-series independence.

Panel A -- Liquidity Changes Around Filing Date							
	Quoted Spread		Spread Quantile		Amihud Illiquidity Measure		Amihud Illiquidity Quantile
Intercept	0.0180 ***		0.2820 ***		1.72 ***		0.4351 ***
	(0.0003)		(0.0042)		(0.13)		(0.0049)
$D_{post}$	-0.0010 **		0.0223 ***		-0.29		-0.0036
	(0.0005)		(0.0059)		(0.18)		(0.0069)

Panel B -- Stock Price Changes Around Filing Date		
Event Day Interval	Mean Raw Return	Mean Excess Return
[-50, -4]	0.10% (0.09)	0.00% (0.09)
[-3, +3]	0.32% (0.23)	0.22% (0.22)
[+4, +50]	0.32% *** (0.09)	0.11% (0.09)

**Table 6. The effect of the 1934 Act – disclosure externalities.**

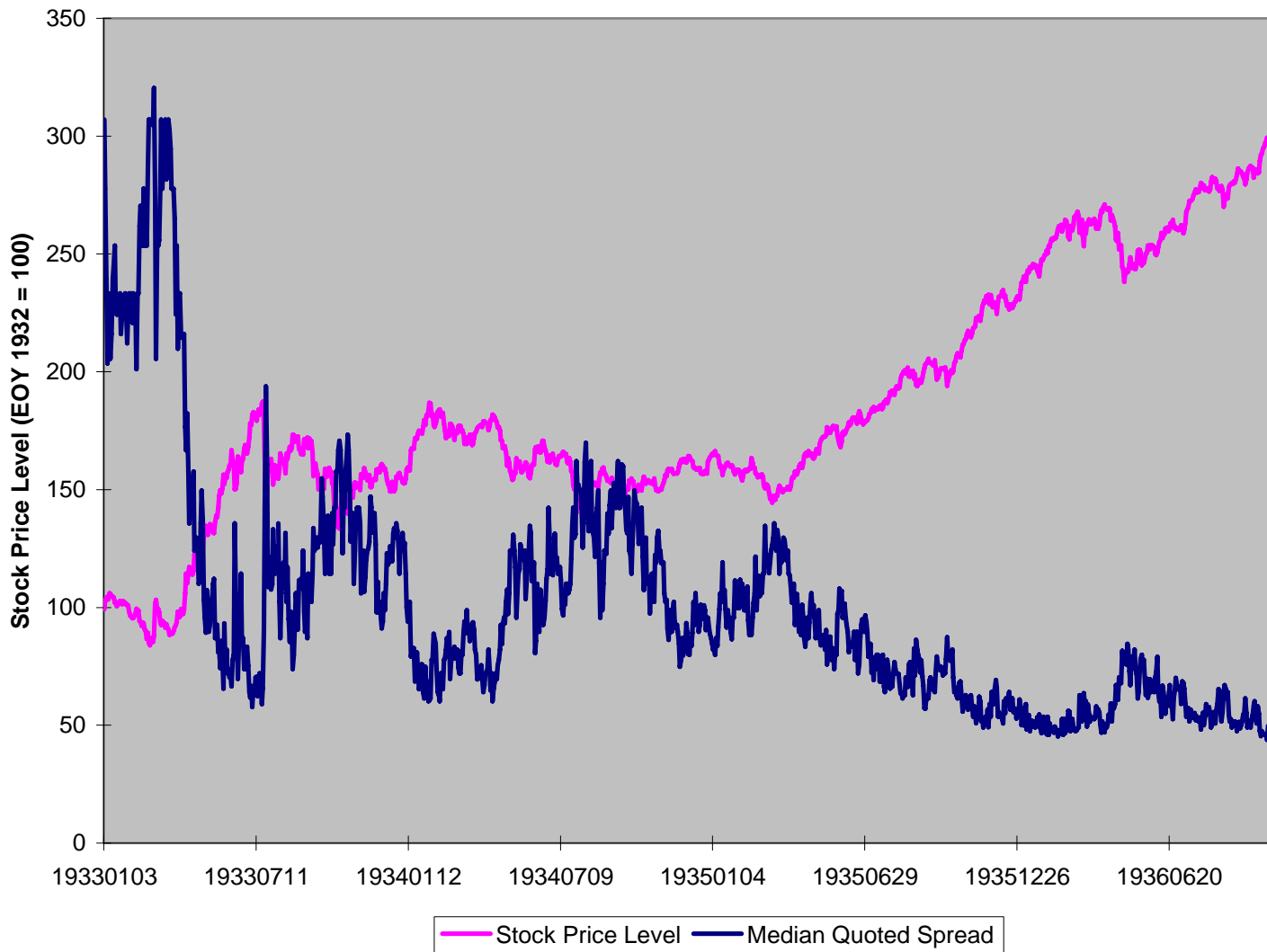
Determinants of the liquidity effects of initial filing under the 1934 Act for a daily panel of NYSE common stocks during 1935 using the following specification:

$$L_{it} = \alpha_i + \gamma_t + \beta_0 DPOST_{it} + \beta_1 DPOST_{it} DSLS_i + \beta_2 DPOST_{it} FSLS_i + \varepsilon_{it},$$

where  $L_{it}$  is the liquidity measure for stock  $i$  at time  $t$ ,  $DPOST_{it}$  is an indicator set to one if firm  $i$  has filed under the 1934 Act at time  $t$ ,  $DSLS_i$  is set to one if firm  $i$  discloses its 1933 sales, and  $FSLS_i$  is the fraction of firms in the same industry that disclose 1933 sales. Analogous regressions are performed using depreciation disclosure ( $DDEP_i$  and  $FDEP_i$ ) and audit status, with  $DAUD_i$  set to one if firm  $i$  is audited in fiscal 1933, while  $FAUD_i$  is the fraction of firms in the same industry that are audited in fiscal 1933. Bid-ask spreads are proportional, and Amihud square-root illiquidity measures (x 100) are calculated using daily transaction returns. Standard errors are in parentheses and are based on independence over time. Significance at the 10%, 5%, and 1% levels is denoted by one, two, and three asterisks respectively.

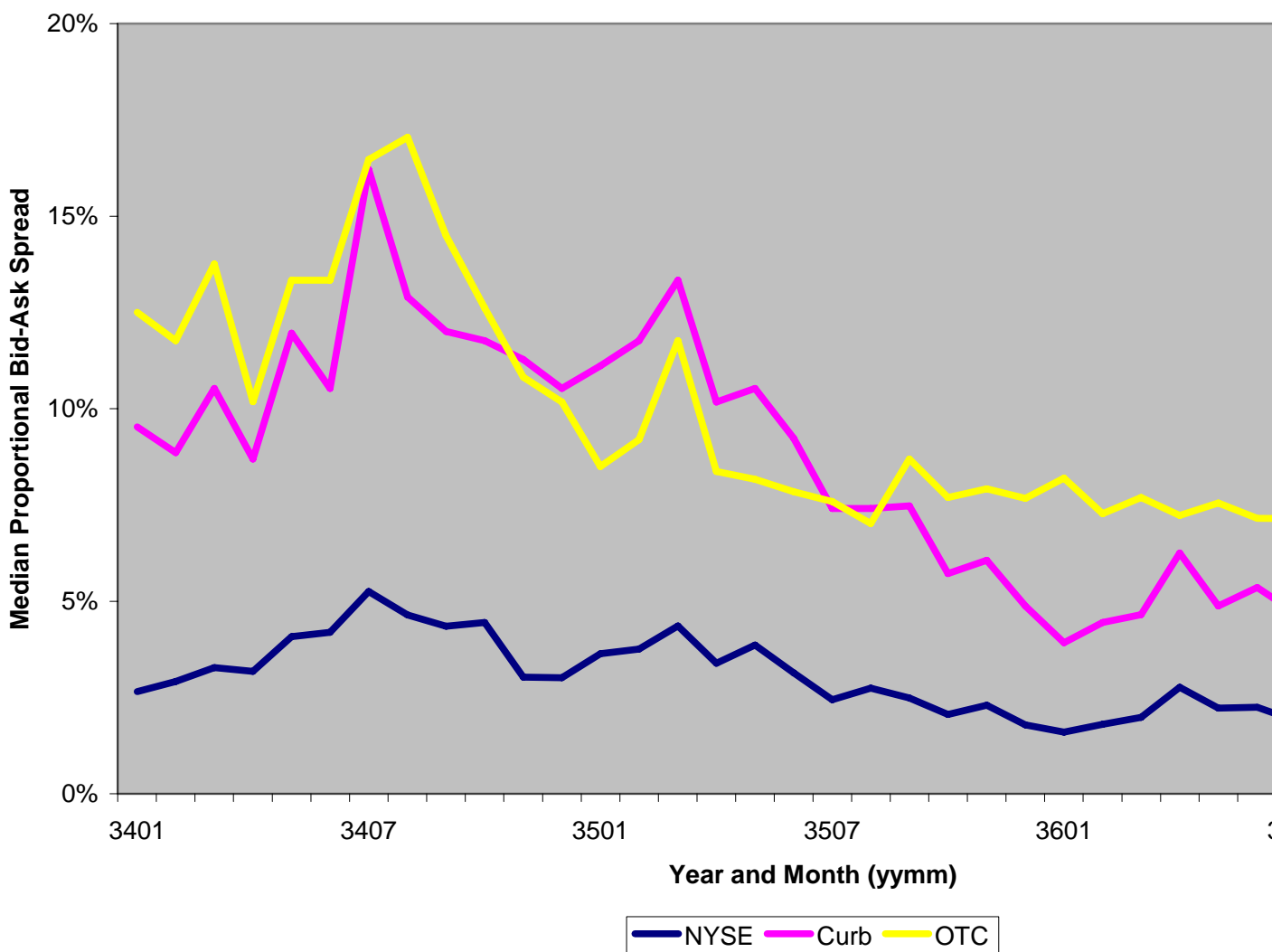
Regression		Quoted Spread		Amihud Illiquidity Measure	
A	DPOST	-0.0129 *** (0.0011)	0.0435 (0.0017)	-0.1519 *** (0.0179)	-0.0044 (0.0179)
	DPOST * DSLS	-0.0057 *** (0.0013)	-0.0058 *** (0.0013)	-0.0576 *** (0.0129)	-0.0579 *** (0.0129)
	DPOST * FSLS	0.0073 *** (0.0026)	0.0075 *** (0.0026)	0.1035 *** (0.0325)	0.1071 *** (0.0321)
B	DPOST	0.0063 (0.0055)	0.0189 *** (0.0057)	0.0324 (0.0652)	0.1830 *** (0.0641)
	DPOST * DDEP	-0.0079 *** (0.0010)	-0.0077 *** (0.0011)	-0.0609 *** (0.0216)	-0.0572 *** (0.0215)
	DPOST * FDEP	-0.0116 * (0.0066)	-0.0112 * (0.0065)	-0.1090 (0.0780)	-0.1163 (0.0778)
C	DPOST	-0.0088 (0.0081)	0.0044 (0.0082)	0.2889 *** (0.1037)	0.4326 *** (0.1062)
	DPOST * DAUD	-0.0056 (0.0034)	-0.0056 * (0.0034)	-0.0443 ** (0.0186)	-0.0426 ** (0.0186)
	DPOST * FAUD	0.0021 (0.0098)	0.0022 (0.0098)	-0.3971 *** (0.1199)	-0.3944 *** (0.1207)
	Firm fixed effect	Yes	Yes	Yes	Yes
	Date fixed effect	No	Yes	No	Yes

**Figure 1. Aggregate value-weighted stock returns and median quoted bid-ask spreads, 1933-1936 NYSE stocks.**

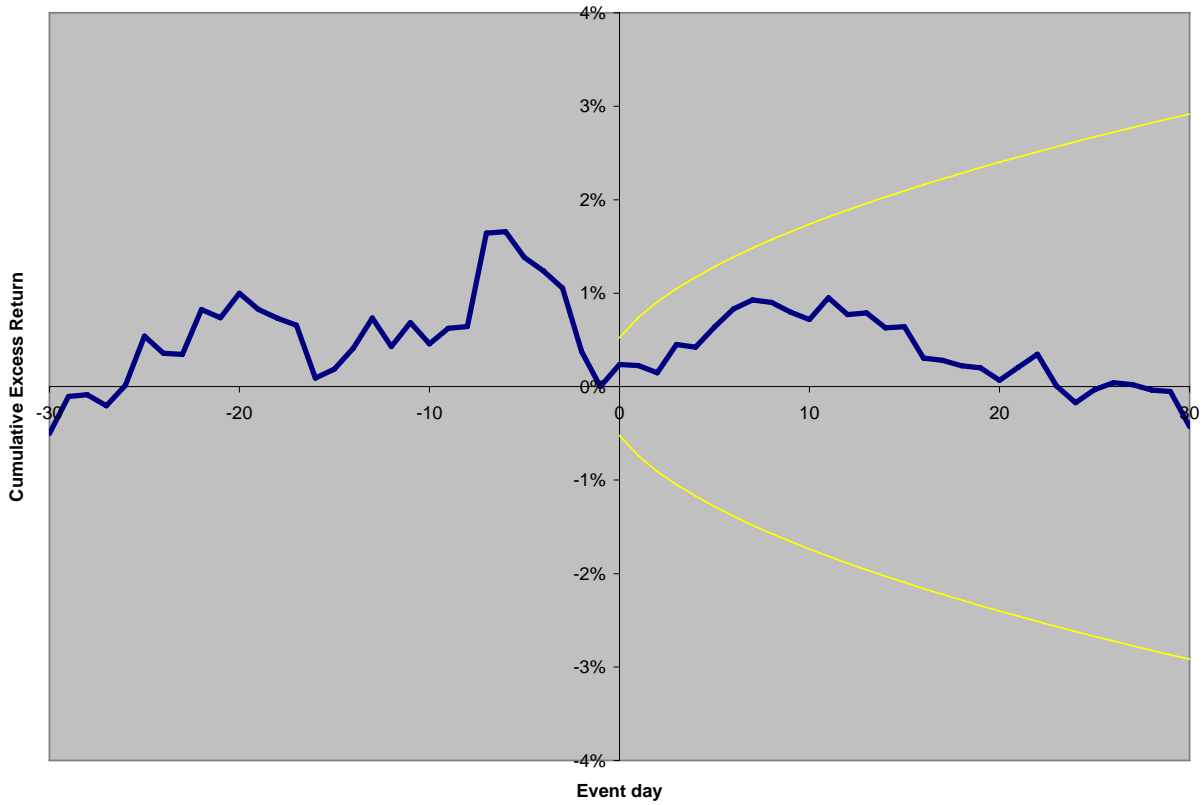




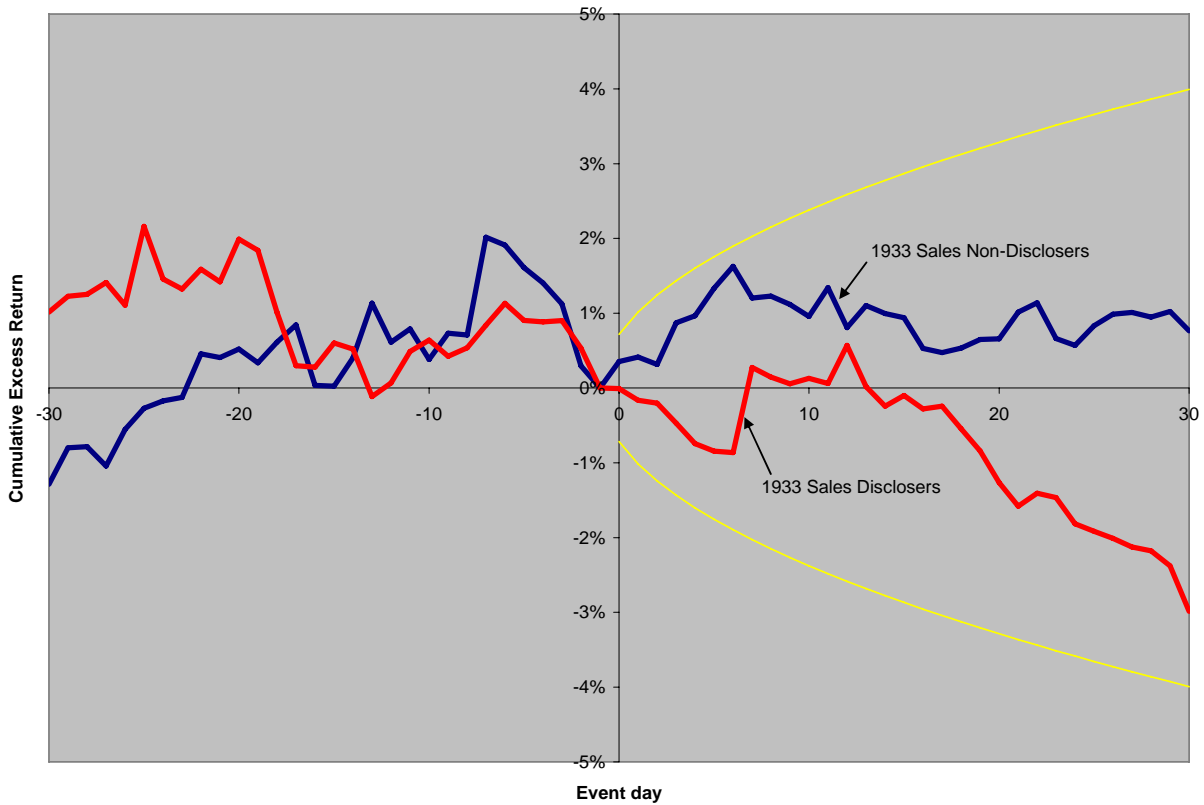
**Figure 2. Median month-end quoted bid-ask spreads on NYSE, Curb and OTC stocks, 1934-1936.** The NYSE and Curb samples consist of all listed common stocks with available data; the OTC sample consists of common stocks tabulated in the Wall Street Journal.



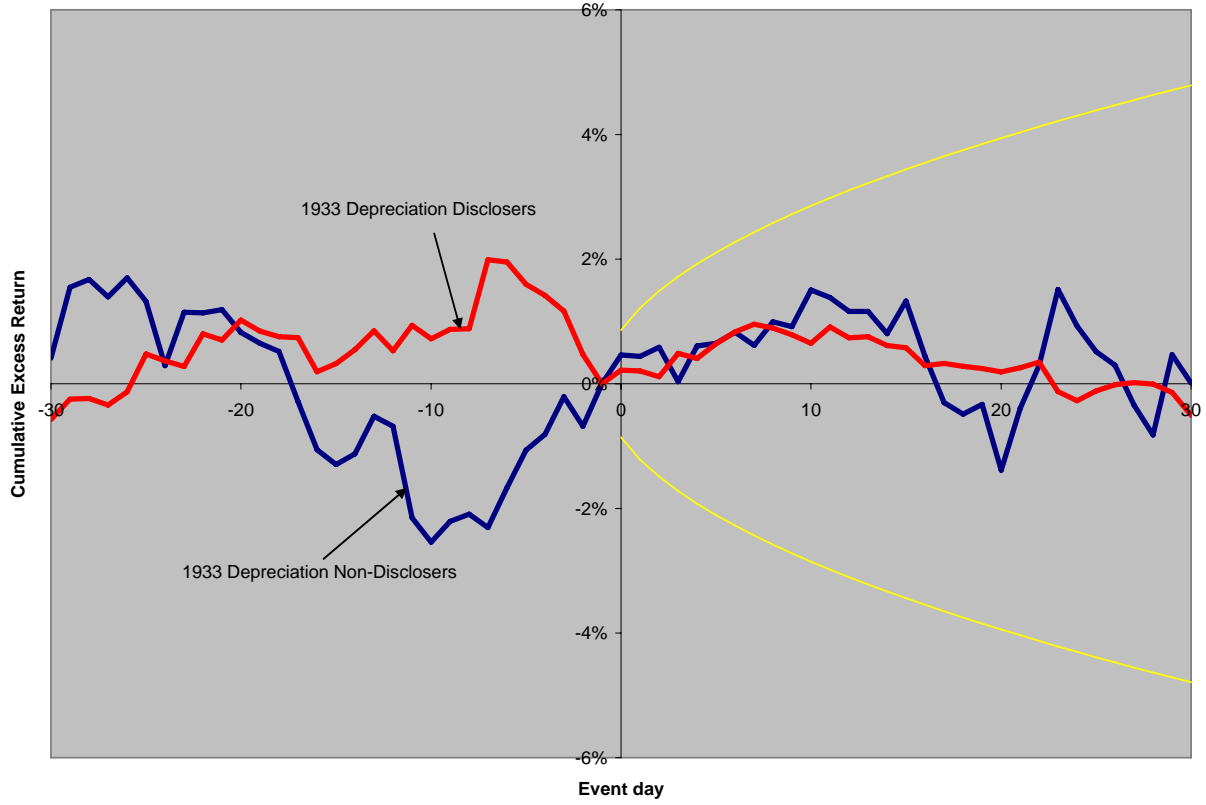
**Figure 3A. Cumulative value-weighted excess returns around initial 1934 Act filing date, all NYSE firms.** The parabola indicates two standard errors around a post-event CAR of zero.



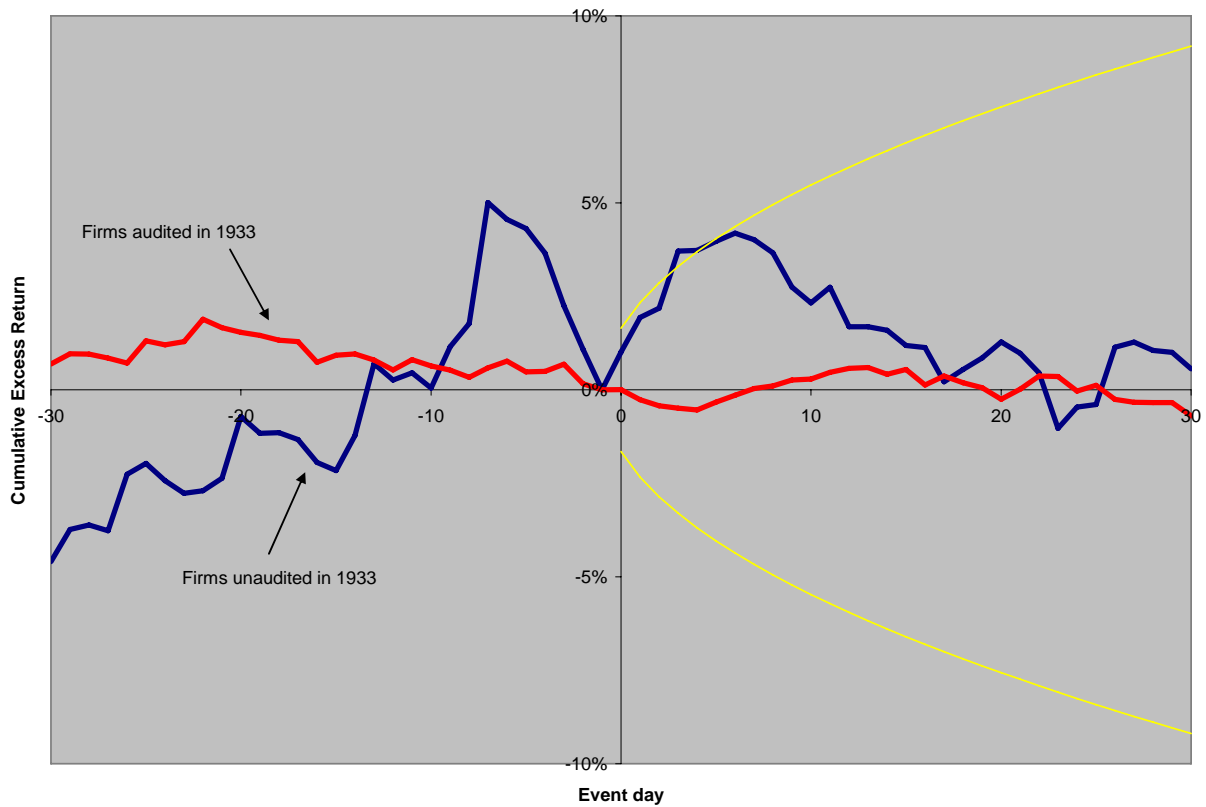
**Figure 3B. Returns around 1934 Act filing, NYSE firms by 1933 sales disclosure status.**



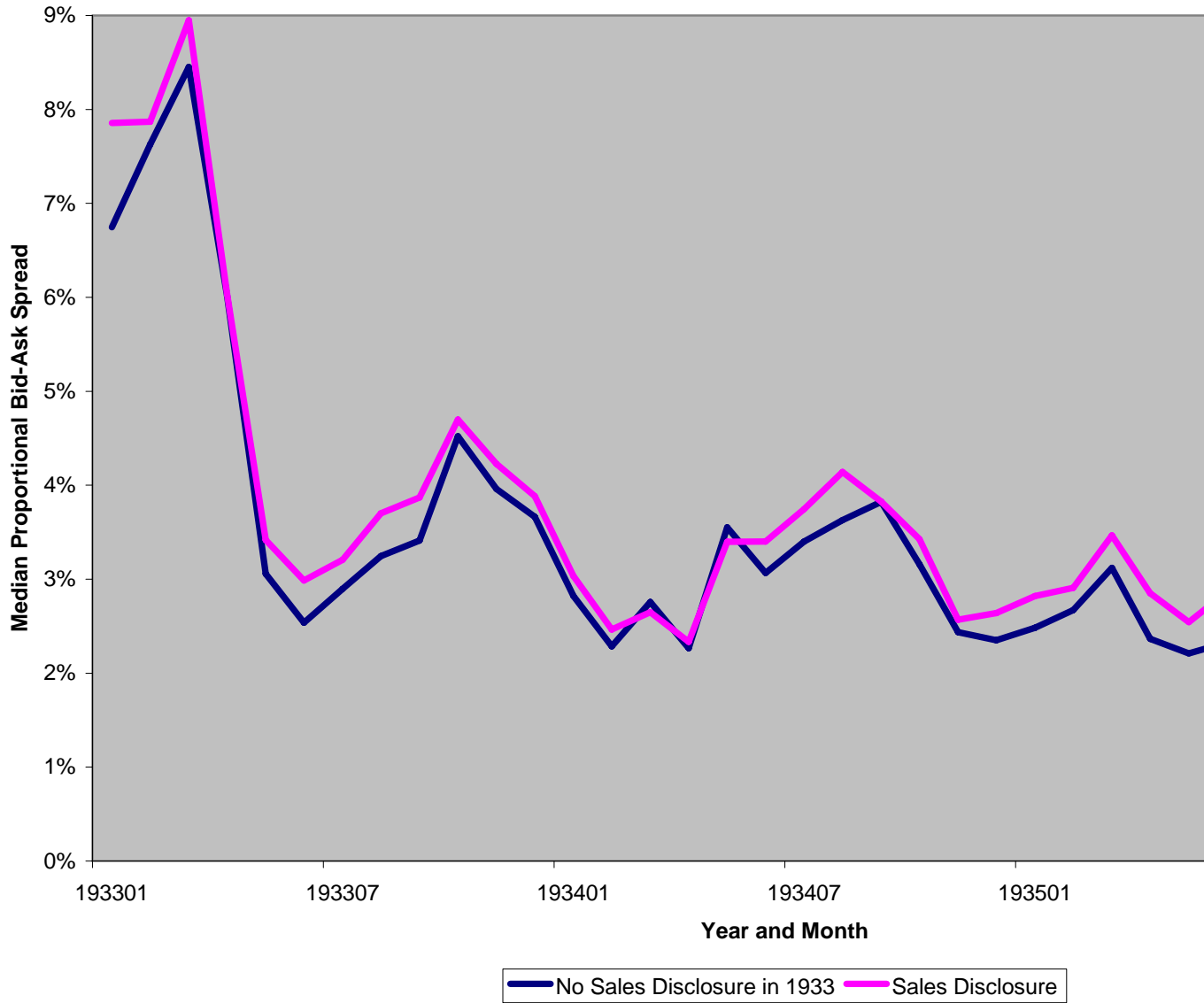
**Figure 3C. Returns around 1934 Act filing, NYSE firms by 1933 depreciation disclosure.**



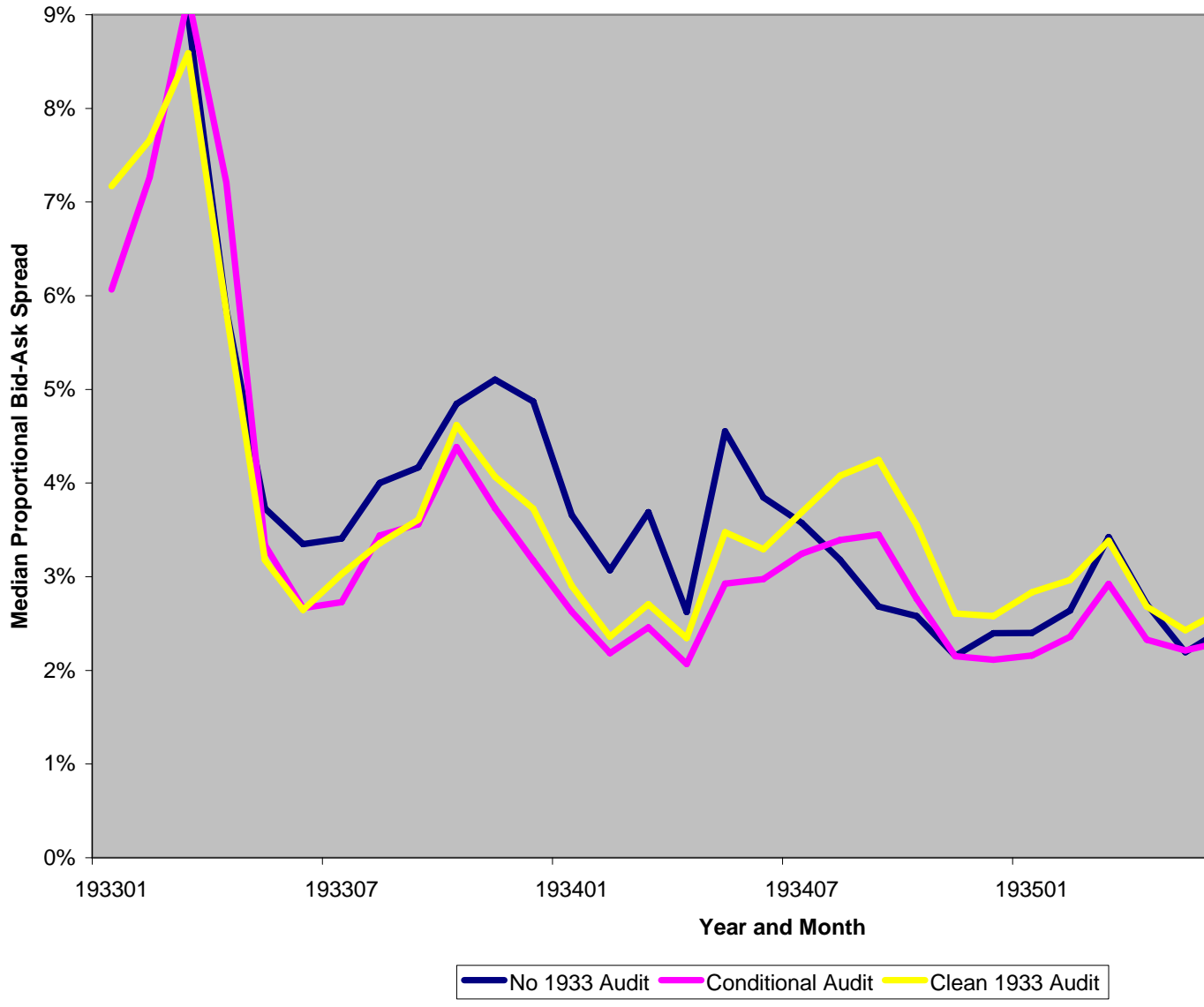
**Figure 3D. Returns around 1934 Act filing, NYSE firms by 1933 audit status.**



**Figure 4. Median quoted bid-ask spreads on NYSE stocks, 1933-1935, by sales disclosure status.**



**Figure 5. Median quoted bid-ask spreads on NYSE stocks by 1933 audit status.**



**Figure 6. Cumulative excess returns on firms issuing debt and filing under the 1933 Act for the first time.** Event day zero is the date of the initial 1933 Act filing. The parabola indicates two standard errors above and below a post-event CAR of zero.

