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The Costs and Benefits of Mandatory Securities Regulation: Evidence from Market Reactions to the JOBS Act of 2012

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Abstract

The effect of mandatory securities regulation on firm value has been a longstanding concern across law, economics and finance. In 2012, Congress enacted the Jumpstart Our Business Startups ("JOBS") Act, relaxing disclosure and compliance obligations for a new category of firms known as "emerging growth companies" (EGCs) that satisfied certain criteria (such as having less than $1 billion of annual revenue). The JOBS Act's definition of an EGC involved a limited degree of retroactivity, extending its application to firms that conducted initial public offerings (IPOs) between December 8, 2011 and April 5, 2012 (the day the bill became law). The December 8 cutoff date was publicly known prior to the JOBS bill's key legislative events, notably those of March 15, 2012, when Senate consideration began and the Senate Majority Leader expressed strong support for the bill. We analyze market reactions for EGCs that conducted IPOs after the cutoff date, relative to a control group of otherwise similar firms that conducted IPOs in the months preceding the cutoff date. We find positive and statistically significant abnormal returns for EGCs around March 15, relative to the control firms. This suggests that the value to investors of the disclosure and compliance obligations relaxed under the JOBS Act is outweighed by the associated compliance costs. The baseline results imply a positive abnormal return of between 3% and 4%, and the implied increase in firm value is at least $20 million for an EGC with the median market value in our sample.

1) Introduction

Securities law in the United States is governed by a regime of mandatory disclosure established by the Securities Act of 1933 and the Securities Exchange Act of 1934. Mandatory disclosure potentially benefits both issuers and investors to the extent that the information disclosed by the former is valuable to the latter, and the disclosures cannot be fully replicated using voluntary mechanisms. On the other hand, these mandatory disclosures entail compliance costs, and issuers and investors cannot contract to waive these requirements in situations where the costs exceed the benefits. Thus, there is a long-standing debate across law, economics and finance regarding the justification for a mandatory disclosure regime (e.g. Easterbrook and Fischel, 1984; Coffee, 1984; Mahoney, 1995) and whether, on balance, mandatory disclosure increases the value of firms. The latter question has been analyzed using a variety of different empirical approaches (e.g. Stigler, 1964; La Porta, Lopez de Silanes and Shleifer, 2006; Greenstone, Oyer and Vissing-Jorgensen, 2006).

The Jumpstart Our Business Startups ("JOBS") Act was passed by Congress in March 2012 and signed by the President on April 5, 2012. It relaxed disclosure and compliance obligations for a new category of firms defined by the Act, known as "emerging growth companies" (EGCs), that satisfied certain criteria (including, most prominently, generating less than $1 billion of revenue in its most recently completed fiscal year). The JOBS Act contained an element of partial retroactivity (as described below) that provides an unusual quasi-experimental setting in which to measure market expectations of the consequences of relaxing regulatory obligations for a subset of firms. It also appears to be unique, in relation to episodes studied in the prior literature, in relaxing rather than a strengthening regulation.

The JOBS Act relaxed existing requirements for EGCs conducting initial public offerings (IPOs) on US equity markets, and also relaxed EGCs' post-IPO disclosure obligations for a 5-year period. The latter provisions reduced the number of years of financial data that had to be disclosed, provided a longer timeframe for complying with new accounting standards, and exempted EGCs from certain executive compensation disclosure requirements. Perhaps most importantly, EGCs were permitted an exemption from auditor attestation of internal controls

under Section 404(b) of the Sarbanes-Oxley (SOX) Act of 2002, as well as exemption from certain future changes to accounting rules.[1]

While the JOBS Act's provisions were primarily prospective (applying largely to firms conducting IPOs after April 5, 2012), the Act's definition of an EGC involved a limited degree of retroactivity. In particular, the Act's definition of an EGC excludes firms whose first sale of common equity securities on public markets occurred on or before December 8, 2011. Conversely, firms that conducted IPOs after December 8, 2011 but prior to the enactment of the Act are eligible for EGC status and the associated reduced disclosure and compliance obligations (if they satisfy the other EGC criteria, such as the $1 billion revenue threshold). Moreover, it was known from at least the beginning of March 2012 that the legislation (if passed) would include a December 8 cutoff (as this was part of draft legislation produced by the House Committee on Financial Services on March 1, 2012). Thus, there is a group of firms that conducted IPOs after December 8, 2011 for which we can observe price data during the sequence of legislative events in March 2012 that propelled the JOBS bill into law. Firms within this group that satisfied the EGC criteria (notably, the $1 billion revenue threshold) were expected to become subject to the reduced disclosure and compliance obligations if the bill passed, while all other firms then trading on US markets would remain subject to the existing regime.

This paper uses an event study approach to measure abnormal returns for these affected ("treatment") firms around major legislative events in March 2012 that increased the probability of the JOBS bill's enactment. This provides a test of investors' expectations about whether or not the value of the mandatory disclosure and compliance obligations that the JOBS bill relaxed exceeds the associated compliance costs. As firms subject to the "treatment" (i.e. EGC status) are all newly traded on public markets, the rest of the market may not necessarily provide an ideal baseline. For the primary control group, we use firms that conducted IPOs from July 2011 to December 8, 2011 and that satisfied the EGC criteria (apart from their IPO date). This yields a control group that is of comparable size to the treatment group, and that has very similar observable characteristics.

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[1] The JOBS Act included a variety of other provisions, as described in Section 3 below. However, it is only the changed obligations for EGCs in Title I of the JOBS Act that are analyzed in this paper. It should also be noted that EGC status is elective, in the sense that eligible firms can choose whether to opt in to each of the relevant provisions of the JOBS Act or to comply with the obligations that apply to non-EGCs. As discussed in Section 5.6 below, election into EGC status was common with respect to the SOX-related provisions of the JOBS Act - about 75% of the EGCs in our sample eventually chose to opt in to these reduced compliance obligations.

Our empirical tests compare abnormal returns for the treatment firms with abnormal returns for the control firms over various relevant event windows. The basic identifying assumption is that, conditional on a firm conducting an IPO over the July, 2011 to April, 2012 period, whether it did so before or after the December 8 cutoff can be considered to be quasi-random with respect to the factors that generate abnormal returns on the key event dates for the JOBS Act. This assumption appears reasonable, given the significant lead time involved in preparing and implementing an IPO.

We collect data on IPOs conducted on the US market over the period from July 2011 to April 5, 2012 from various sources, including the Securities and Exchange Commission's (SEC's) Electronic Data Gathering and Retrieval (EDGAR) system. We find a total of 87 firms that conducted IPOs over this period. For these firms, we also collect Compustat financial statement information and Center for Research in Security Prices (CRSP) data on firms' daily returns and on daily market returns. We use the data on IPO date, revenue in the most recently completed fiscal year, and other relevant variables to determine which of these firms satisfy the JOBS Act's criteria for EGC status. Taking account of missing data, our control group consists of 33 firms (with less than $1 billion in revenues that conducted IPOs prior to December 8, 2011). The treatment group of EGCs varies in size from 25 to 41, depending on the date; we have 27 treatment firms for our most important tests. While the sample size is relatively small, this serves primarily to create a bias against finding any significant results.

The bill that eventually became Title I of the JOBS Act (defining EGCs and relaxing their disclosure obligations) was introduced in the US House of Representatives on December 8, 2011. This initial bill did not backdate EGC status to December 8, 2011, although the cutoff date was later chosen to coincide with the date of its introduction. The bill was referred to the House Financial Services Committee, which produced an amended version on March 1, 2012 that included the December 8, 2011 cutoff date for EGC status. The House passed the bill on March 8, 2012 by an overwhelming margin. However, widespread opposition to the bill emerged immediately following the House vote, exemplified by an editorial in the influential *New York Times* describing the bill as "a terrible package . . . that would undo essential investor protections [and] undermine market transparency . . ."[2] This opposition created substantial uncertainty about whether the bill would be considered by the Senate. The uncertainty was largely resolved on

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[2] See: http://www.nytimes.com/2012/03/11/opinion/sunday/washington-has-a-very-short-memory.html?_r=0

March 15, when the Senate Majority Leader signaled the importance of the bill by scheduling a vote and describing it as "a measure the Senate should consider expeditiously and pass in short order."[3] The Senate passed the bill (with some amendments that did not pertain to the EGC provisions) on March 22. The House then passed the amended Senate version on March 27, and it was signed by the President on April 5, 2012.

We use both the market model and the Fama-French model (augmented by Carhart's momentum factor) to compute abnormal returns for the firms in our sample. Abnormal returns are calculated over a (-1, +1) event window that spans the period from the release of the House Financial Services Committee report on March 1, 2012 to the Presidential signature (this "full" event window spans February 29 to April 9, 2012). As many of the firms in our sample have only a limited pre-event returns history, our estimation window uses both the pre-event period and post-event returns data through December 31, 2012. We compute cumulative abnormal returns (CARs) for the full event window and for various shorter windows, in particular for the March 15 Senate event on which we focus.[4] We then use a regression framework to test whether the CARs for the treatment firms are significantly different from those for the control group of firms (controlling for various firm-level variables).

Our central result is that the March 15 Senate event was associated with positive and statistically significant abnormal returns for treatment firms (i.e. EGCs), relative to the control firms. A critical empirical challenge is that this sample consists of firms that are close to their IPO date, which may raise concerns related to the large literature in finance on IPO underpricing (e.g. Ljungqvist, 2008). However, this is a phenomenon that primarily affects the first trading day, which is excluded from all of our tests. Moreover, we find robust results when we control for the number of trading days since a firm's IPO and exclude firms that are one month or less from their IPO date. The result is also robust to controlling for revenue in the most recent fiscal year and a number of financial statement variables (such as assets, debt, earnings, and R&D expenditures). It is also robust to the inclusion of industry fixed effects (although the effective sample size becomes quite small) and to using as an alternative control group those larger firms (non-EGCs above the $1 billion threshold) that conducted IPOs after December 8, 2011.

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[3] See the Congressional Record, available at: http://thomas.loc.gov/cgi-bin/query/R?r112:FLD001:S51694
[4] In an alternative test, we aggregate the EGCs into a single portfolio and compute the portfolio CARs around March 15. This approach addresses concerns about the potential cross-correlation of returns among EGCs, and leads to similar results (as described in Section 5).

Reassuringly, two tests using firms that conducted IPOs after December 8, 2011 but were not subject to the JOBS Act - firms above the $1 billion threshold, and registered investment companies - as placebo "treatment" groups find no effects.

The baseline results imply a positive abnormal return of between 3% and 4%. The implied increase in firm value is at least $20 million for an EGC with the median market value in our sample. This is comparable in magnitude to, albeit larger than, estimates in the literature of the compliance costs associated with Section 404(b) of SOX (a provision relaxed for EGCs under the JOBS Act). Some evidence suggests that part of the effect is attributable to the relaxation of SOX requirements. Firms that are classified by the SEC as "nonaccelerated filers" (with a public float of less than $75 million) were exempt from compliance with SOX 404(b) prior to the JOBS Act. The effect for EGCs in our sample that are nonaccelerated filers is essentially zero, although any conclusions are tentative due to the small number of nonaccelerated filers.

We also address a number of potential alternative explanations and interpretations. If the partial retroactivity of the JOBS Act were attributable to lobbying by EGCs, this may potentially confound our results. Thus, we collect data on lobbying activity by EGCs and on campaign contributions by associated political action committees (PACs), and find that the results are unaffected by omitting the "politically active" EGCs. We also search for other news events (unrelated to the JOBS Act) about EGCs in the relevant window. Omitting EGCs that were the subject of unrelated news stories also does not affect the results. They are also unaffected by Winsorizing or omitting two firms that experienced particularly large positive abnormal returns.

A possible alternative interpretation of the result is that the relaxation of regulation may create greater opportunities for sophisticated incumbent shareholders to sell in the future to uninformed "noise traders" at inflated prices. To test this alternative interpretation, we collect data on analyst coverage of EGCs from the International Brokers Estimate System (I/B/E/S) dataset. Potential mispricing would presumably be more relevant for firms without analyst coverage, but we find that the EGC effect is virtually identical for firms with and without analyst coverage. This casts doubt on the alternative interpretation based on mispricing.

This paper addresses a central question in the analysis of securities regulation, and so it is related to a number of different strands of literature across law, economics and finance. The pioneering empirical literature on the effects of securities regulation used time-series

comparisons of various outcomes before and after the Securities Acts were enacted (e.g. Stigler, 1964; Friend and Herman, 1964).[5] More recently, a literature using cross-country empirical analysis has studied the impact of securities regulation and its (public and private) enforcement on the extent of stock market development (e.g. La Porta *et al*., 2006).

Our paper is most directly related to a literature using single-country quasi-experiments to analyze the effects of changes in securities law. For example, Greenstone *et al*. (2006) use as a quasi-experiment the 1964 amendments that extended the mandatory disclosure requirements of US securities law to certain firms trading over-the-counter (OTC).[6] They hand-collect price data for OTC firms, and compare abnormal returns for the firms that were subject to the amendments to those for a control group of otherwise similar exchange-traded firms that were already subject to these disclosure requirements and therefore unaffected by the amendments. This approach implies large positive abnormal returns for the affected firms of between 11.5% and 22.1% over the full event window, relative to the control group.

In contrast to Greenstone *et al*. (2006), our paper finds a negative effect of securities regulation on firm value in the US. However, this should not be viewed as in any way contradicting their findings, as we examine a much later time period and a very different regulatory environment. In particular, the 1964 amendments involved a much more extensive change in regulation for the affected firms than did the JOBS Act. In addition, the baseline level of regulation for OTC firms prior to the 1964 Amendments was very limited, whereas public firms were subject to very extensive regulation at the time of the JOBS Act. Rather, both our results and theirs can be encompassed within a simple conceptual framework outlined in Section 2 below, in which securities regulation initially increases firm value, but beyond a certain point may decrease value as compliance costs exceed the benefits of regulation to investors. Our results also point towards a less ambiguous interpretation in terms of social welfare than do theirs, a point that is developed in Section 2 below.

As the relaxation of the SOX internal control requirements is a significant component of the JOBS Act, our paper is also related to the empirical literature evaluating the effects of SOX

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[5] Benston (1973) uses an event study approach to analyze the effects of the Securities Exchange Act of 1934, using firms that were already disclosing the required information as a control group.

[6] Ferrell (2007) also analyzes the consequences of the 1964 amendments, finding positive abnormal returns and a reduction in volatility for OTC firms. Bushee and Leuz (2005) analyze the further extension of disclosure requirements in 1999 to the small firms that trade on the OTC Bulletin Board. They find significant benefits from this extension for certain firms, but also find that the increased compliance costs led some firms to exit the Bulletin Board.

(e.g. Chhaochharia and Grinstein, 2007; Litvak, 2007; Bartlett, 2009; Kamar, Talley and Karaca-Mandic, 2009; for a comprehensive recent review of this literature, see Coates and Srinivasan (2013)). Our paper is also related to single-country quasi-experimental studies of broader corporate governance reforms outside the US, which typically include some provisions relating to disclosure (e.g. Black, Jang and Kim, 2006; Dharmapala and Khanna, 2013). Finally, our paper is related to the large and growing legal literature on the JOBS Act (e.g. Langevoort and Thompson, 2013; Guttentag, 2013). However, this literature does not empirically analyze the consequences of the Act.[7]

This paper proceeds as follows. Section 2 develops a simple conceptual framework that is helpful in interpreting the results. Section 3 provides a brief overview and history of the JOBS Act. Section 4 describes the data and elaborates on the empirical strategy. Section 5 discusses the results, and Section 6 concludes.

2) A Simple Conceptual Framework

This section develops a simple conceptual framework that encapsulates many of the insights of the theoretical literature on securities disclosure, insider diversion and firm value (see e.g. Shleifer and Wolfenzon, 2002) and provides a simple framework within which to interpret the paper's results. Consider a firm that has (exogenously fixed) fundamental value V. Let r be a measure of the strength of securities regulation. Higher values of r entail higher compliance costs, but also reduce the expected diversion of private benefits by insiders. Suppose that insiders own a fraction $\alpha > 0$ of the firm, that $B(r)$ is a decreasing, convex function representing the private benefits diverted by insiders, and that $C(r)$ is an increasing, convex function representing the costs of compliance with securities regulation (which are borne *pro rata* by all shareholders). The diversion of private benefits is assumed to generate a deadweight loss, in the sense that \$1 of private benefits costs outside shareholders \$(1 + γ), where $\gamma > 0$.

Under these assumptions, the value placed on the firm by outside investors (V_M) and the value placed on it by insiders (V_I) can be expressed as:

$$V_M = (1 - \alpha)V - (1 + \gamma)B(r) - (1 - \alpha)C(r) \tag{1}$$

and:

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[7] A partial exception is Berdejo (2014), but its focus is on firms that went public after the enactment of the JOBS Act, rather than on the EGC sample analyzed here.

$$V_I = \alpha V + B(r) - \alpha C(r) \qquad (2)$$

It is immediately obvious from this simple framework that a decrease in r can either increase or decrease V_M, depending on the balance between private benefits and compliance costs (as illustrated in Figure 1). Moreover, the fact that the JOBS Act was widely supported by the business community does not render it a foregone conclusion that market reactions would be positive. It is entirely possible that a decrease in r could both decrease V_M and increase V_I, if the increase in $B(r)$ is sufficiently large.

Summing V_M and V_I, the aggregate value of the firm is:

$$V_M + V_I = V - \gamma B(r) - C(r) \qquad (3)$$

In the absence of externalities, this aggregate value can be interpreted as a measure of social welfare. Suppose that an exogenous legal reform (such as the JOBS Act) reduces r. In the absence of a sale of control, the observed market response reflects outside investors' value (V_M). Thus, if we observe a decline in V_M, it follows that the magnitude of the increase in private benefits borne by outside shareholders exceeds the magnitude of the decrease in outside shareholders' share of compliance costs. It does not necessarily follow, however, that the magnitude of the increase in the deadweight cost of private benefits exceeds the magnitude of the decrease in compliance costs.[8] Thus, it is unclear whether or not social welfare is decreased by the legal reform. While outsiders' value falls, the gains to insiders may be sufficient to offset this loss. This is essentially the situation implied by the findings of Greenstone *et al.* (2006), albeit in reverse. They find that an increase in r led to an increase in V_M; as they point out, however, this is not sufficient to establish that social welfare increases.

On the other hand, suppose that an exogenous legal reform (such as the JOBS Act) reduces r, and we then observe an *increase* in V_M. This entails that the magnitude of the increase in private benefits borne by outside shareholders is smaller than the magnitude of the decrease in outside shareholders' share of compliance costs. From this, it necessarily follows that the magnitude of the decrease in compliance costs exceeds the magnitude of the increase in the deadweight loss from private benefits.[9] Therefore, social welfare necessarily increases in this scenario as a result of the decrease in r.[10]

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[8] More precisely, the decrease in V_M entails that $(1 - \alpha)\frac{\partial C}{\partial r} < -(1+\gamma)\frac{\partial B}{\partial r}$. However, this does not necessarily imply that $\frac{\partial C}{\partial r} < -\gamma\frac{\partial B}{\partial r}$, which is required for $(V_M + V_I)$ to increase.

[9] More precisely, the increase in V_M entails that $(1 - \alpha)\frac{\partial C}{\partial r} > -(1+\gamma)\frac{\partial B}{\partial r}$. This necessarily implies that

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3) The JOBS Act and its Legislative History

3.1) US Securities Law and the Context of the JOBS Act

The JOBS Act is the most recent in a series of statutes regulating the US securities markets. The key statutes in this area are the Securities Act 1933 (SA), Securities & Exchange Act 1934 (SEA), Sarbanes-Oxley Act 2002 (SOX), and now the JOBS Act.[11] The SA, and rules promulgated thereunder, are the primary means of regulating the capital raising process in the US. Thus, a substantial part of the regulations surrounding an IPO, a private placement of securities to large investors, or a debt issuance emanate from the SA. The SEA and associated rules cover a range of activities in the securities markets, ranging from the continuing disclosure obligations of firms to insider trading and a host of other items; the SEA also established the Securities and Exchange Commission (SEC). Together, the SA and SEA represent the bulk of Federal Securities Laws in the US.

Although there have been other significant enactments in this area (e.g., the Investment Advisors Act of 1940 and the 1964 Amendments), the next set of major reforms that were applicable across the securities markets came with the enactment of SOX in 2002. SOX was enacted as a response to the accounting scandals in the early 2000s, such as those involving Enron and Worldcom. It put in place a panoply of measures, including enhanced internal controls to provide more accurate financial disclosure. This was supplemented by requirements for top executives to certify financial statements (and the process for generating them) as well as requiring external auditors to certify/assess these internal controls. In addition to this, SOX required more disclosure of Off-Balance Sheet items as well as prohibiting the improper influence of an audit.

These enactments all increased disclosure, required more steps to be taken by firms and executives, amongst others, and enhanced penalties. The ratchet, so to speak, moved upward in

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$\frac{\partial c}{\partial r} > - \gamma \frac{\partial B}{\partial r}$, which implies that $(V_M + V_I)$ increases.

[10] Guttentag (2013, p. 186) argues that models emphasizing private benefits from suboptimal disclosure are not particularly relevant to the US context, where there exist robust private contracting mechanisms that can implement optimal solutions. If one adopts this view, then in the limit $B(r) = 0$ for all r, and the deadweight costs of private benefits are not a concern. The ambiguity in the social welfare implications of Greenstone *et al.* (2006) would disappear, but the interpretation of this paper's findings would not be substantially altered.

[11] For a comprehensive account and discussion, see e.g. Choi and Pritchard (2012).

each case.[12] However, the JOBS Act was arguably unique in the sense that the ratchet moved downwards – it took steps that were generally perceived to loosen some regulations, to allow for some firms to have fewer obligations, and to permit new ways to fund certain ventures. The key motivation for the JOBS Act appears to have been the decline in the number of IPOs since the technology boom of the 1990s and early mid 2000s (attributed by some to onerous regulation, including SOX)[13] combined with enthusiasm in Congress for legislation that could be presented as fostering employment creation after one of the greatest economic downturns in US history.

3.2) Provisions of the JOBS Act

The JOBS Act puts in place a number of provisions reflecting a variety of different amendments to the securities laws, ostensibly designed to enhance the ability of some firms – especially smaller firms – to raise capital. In particular, the Act begins by creating a new category of firm – the "emerging growth company" (EGC) for both the SA and SEA (and hence for SOX as well).[14] These are firms that in their most recent fiscal year had annual gross revenue of less than $1 Billion.[15] Firms remain EGCs until the earliest of the following events occurs:

(i)   Five (5) years have elapsed since the firm's IPO.[16]

(ii)   The Firm's annual gross revenue exceeds $1 Billion or more.[17]

(iii)   The Firm issues more than $1 Billion in non-convertible debt over three (3) years.[18]

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[12] The ratchet moved upwards with the Dodd-Frank Act (DFA) of 2010 as well. The DFA is important for a number of reasons – for instance, it introduced the "say-on-pay" votes on executive compensation that was one of the measures relaxed for certain firms by the JOBS Act. However, the DFA's changes to the regulatory structure and requirements of the SA and SEA are limited and hence we do not discuss it in detail.

[13] See e.g. the IPO Task Force report on "Rebuilding the IPO On-Ramp: Putting Emerging Companies and the Job Market Back on the Road to Growth" available at: http://www.sec.gov/info/smallbus/acsec/rebuilding_the_ipo_on-ramp.pdf

[14] In addition to the creation of this new category, the Act operates in at least four other large arenas. First, the Act relaxes some regulations and enacts new ones that are designed to facilitate the use of "crowdfunding" for certain businesses. This does not form the primary focus of our paper and hence we do not discuss it in any depth. Second, the Act eases restrictions for firms considering a private placement under Regulation D (and Rule 144A), which, in part, facilitates easier communication with some sets of potential investors. Third, the Act increases the amount that can be raised by firms using Regulation A (which is targeted to smaller issuers) from $5 Million to $50 Million. Fourth, the Act amends the registration requirements under the SEA such that now a firm is subject to parts of the SEA only when it has more than 2000 shareholders (as compared to the 500 shareholder threshold of the past) and more than $10 Million in assets (as compared to the $1 Million asset threshold of the past). All these measures appear designed to reduce or ease regulations on smaller or newer firms, especially those that might be designated as EGCs. We focus our discussion in the text on the regulation of EGCs and what the JOBS Act has done that makes their regulatory burdens lighter.

[15] See §§ 101(a) & (b), JOBS Act 2012.

[16] See id.

[17] See id.

(iv) The Firm meets the definition of a "large accelerated filer".[19]

To be considered an EGC, the firm's first sales of shares in its IPO must have occurred after December 8, 2011.[20] If a firm is an EGC then it is entitled to receive less onerous regulatory treatment in a number of spheres, as described below. It is noteworthy that an EGC can choose not to be treated as an EGC (and hence be treated as a "regular" issuer).[21]

If a firm is an EGC, and wishes to be treated as one, then it will receive more lenient compliance and disclosure obligations:

(i) The EGC will not be required to comply with the auditor attestation requirements of section 404(b) under SOX.[22]

(ii) The EGC will not be subject to audit firm rotation or auditor discussion and analysis requirements.[23]

(iii) The EGC is not subject to any future rules of the Public Company Accounting Oversight Board (PCAOB) unless the SEC explicitly decides that EGCs should be subject to the new rule.[24]

(iv) The EGC will receive a longer transition period to comply with new audit standards.[25]

(v) The EGC is not required to include more than two (2) years of financial statements in the filings that make up part of an IPO.[26]

(vi) The EGC is not required to comply with the "say on pay" and "pay versus performance" requirements.[27]

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[18] See id.

[19] See id. A large accelerated filer is a firm that:
"(i) [has] an aggregate worldwide market value of the voting and non-voting common equity held by its non-affiliates of $700 million or more;
(ii) [has] been subject to the requirements of section 13(a) or 15(d) of the Act for a period of at least twelve calendar months;
(iii) [has] filed at least one annual report pursuant to section 13(a) or 15(d) of the Act; and
(iv) … is not eligible to use the requirements for smaller reporting companies …for its annual and quarterly reports." (See 17 Code of Federal Regulations (CFR) § 240.12b-2).

[20] See § 101(d), JOBS Act 2012. The registration statement for the IPO must be "effective".

[21] See §107, JOBS Act 2012. At the time that we analyze market reactions, it would not have been known whether a particular EGC would elect to be treated as such. As discussed in Section 5.6 below, election into EGC status was common with respect to the SOX-related provisions of the JOBS Act - about 75% of the EGCs in our sample eventually chose to opt in to these reduced compliance obligations.

[22] See §103, JOBS Act 2012.

[23] See §104, JOBS Act 2012.

[24] See §104, JOBS Act 2012.

[25] See §102, JOBS Act 2012.

[26] See §102(b)(1), JOBS Act 2012.

(vii)   The EGC is not required to include certain financial data that relates to a time before the earliest audited statements included in its IPO filings.[28]

(viii)   The EGC can start the IPO process by confidentially submitting its draft registration to the SEC for non-public review (although if the firm decides to go forward with an IPO the registration statement must be publicly available at least 21 days prior to the start of the "roadshow" for the IPO.[29]

(ix)   The EGC can "test the waters" with large and sophisticated investors (e.g., Qualified Institutional Buyers, Accredited Investors) before and during the registration process.[30] This usually means the EGC can now have communications with these investors, whereas prior to the JOBS Act such communications may have triggered a host of disclosure requirements and penalties.

(x)   Investment Banks will now be allowed to both provide analyst research reports on the EGC as well as work as an underwriter for the EGC's public offering (in the past there were restrictions on communications made by such parties).[31]

The JOBS Act thus lessens the regulatory requirements for EGCs in a number of spheres. In particular, it allows the EGC to avoid being subject to some accounting, auditing and internal control requirements enacted under SOX as well as providing EGCs with a longer transition period to comply with some of these requirements. In addition, EGCs will have lesser disclosure burdens in their IPO filings and executive compensation disclosures as well as the ability to submit their filings confidentially (at least for some period of time). Finally, EGCs (and those associated with their offerings) will have fewer restrictions on their ability to communicate with potential investors compared to non-EGCs.

3.3) The Legislative History of the JOBS Act

The legislative history of the JOBS Act and the key event dates in its progress through Congress are summarized in Table 1. The bill that eventually became Title I of the JOBS Act

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[27] See §102(a)(1) – (3), JOBS Act 2012.
[28] See §102(b)(2), JOBS Act 2012.
[29] See §106(a), JOBS Act 2012. A "roadshow" (defined in 17 CFR §230.433(h)(4)) is a particular method of communicating the upcoming IPO to potential investors.
[30] See §105(c), JOBS Act 2012.
[31] See §105, JOBS Act 2012.

(H.R. 3606, defining EGCs and relaxing their disclosure and compliance obligations) was introduced in the US House of Representatives on December 8, 2011. This initial version did not backdate the effective date for EGC status to December 8, 2011, although the effective date was later chosen to coincide with the date of the bill's introduction. The bill was referred to the House Financial Services Committee, which produced an amended version on March 1, 2012 that included the December 8, 2011 cutoff date for EGC status.[32]

The House passed the bill on March 8, 2012 with overwhelming (and bipartisan) support. Moreover, President Obama had endorsed legislation of this type in his 2012 State of the Union address. Thus, one might ordinarily expect that there would subsequently be little uncertainty about eventual Senate passage and enactment (even though in an era of divided partisan control of the two chambers of Congress, it is common for the House to vote for a bill that is subsequently ignored by the Senate). However, widespread opposition to the JOBS bill began to emerge upon its passage in the House. Perhaps most notable is an editorial in the influential *New York Times* that described the various elements of the proposed reforms as: "A terrible package of bills that would undo essential investor protections, reduce market transparency, and distort the efficient allocation of capital."[33] There were also expressions of opposition from advocacy groups, former SEC officials, and some Democratic Senators. The JOBS bill also became embroiled in ongoing political disputes over the confirmation of Federal judicial nominees, with the perception that the Senate would not take up the JOBS bill until (or unless) these disputes were resolved.[34]

The emergence of widespread opposition after March 8 arguably created substantial uncertainty regarding whether the Senate would consider the bill (and hence about whether it would ever be enacted). The Senate Majority Leader Harry Reid (D-NV) had previously spoken in favor of the bill, but was perceived as being only lukewarm in his support; in particular, he was thought to favor alternative measures believed to promote "job creation" such as a transportation bill. Despite these uncertain expectations of a prompt Senate vote, the JOBS bill was taken up in the Senate on March 15, when Senator Reid signaled the importance of the bill by scheduling a vote. Perhaps most importantly, he described the legislation as follows:

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[32] This account is based on information in the Congressional Record, available at: http://thomas.loc.gov
[33] See "They Have Very Short Memories" *New York Times*, March 10, 2012, available at:
http://www.nytimes.com/2012/03/11/opinion/sunday/washington-has-a-very-short-memory.html?_r=0
[34] See e.g.
http://talkingpointsmemo.com/dc/reid-dares-gop-block-judicial-nominees-and-you-will-also-stall-the-jobs-act

"[L]et me take a moment to review what has transpired this morning. Last week the House passed the pending small business capital formation bill by a vote of 390 to 23 [This refers to the House vote on March 8 in favor of H.R. 3606]. President Obama has endorsed the bill very publicly; thus, this is a measure the Senate should consider expeditiously and pass in short order."[35]

A limited number of amendments were scheduled. The Senate passed the bill (with some amendments that pertained to the crowdfunding provisions but not to the provisions regarding EGCs) on March 22. The House then passed the amended Senate version on March 27, and the JOBS bill was signed into law by the President on April 5, 2012.

The March 15 developments and the speech by Senator Reid are likely to have resolved much of the uncertainty described above. In particular, given the overwhelming support in the House, the support of the President, and widespread support within the business community, any uncertainty surrounding the bill would have been likely to be about whether the bill would be sufficiently prioritized to reach a vote, rather than on whether it would pass, conditional on reaching the floor. In view of these circumstances, the March 15 consideration by the Senate and the strong endorsement by the Senate Majority Leader are likely to be of particular importance.[36] Consequently, our empirical tests (while examining a number of different event windows) focus in particular on the March 15 event date. In contrast, many of the other events (especially the Presidential signature on April 5, 2012, but perhaps also the initial passage in the House) may be expected to have conveyed little new information.

It is quite reasonable to ask why the effective date for EGC status was partially retroactive, especially as this is the cornerstone of our empirical strategy. This practice is not common in securities legislation, and the legislative record does not provide an explicit rationale. One possible explanation is that it was intended to prevent firms that were contemplating IPOs during the legislative process from delaying them to wait and see whether the bill would be enacted. Delaying IPOs would be a perverse consequence of legislation ostensibly intended to

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[35] See the Congressional Record, available at: http://thomas.loc.gov/cgi-bin/query/R?r112:FLD001:S51694

[36] It is important to note that we are not claiming that the Reid speech was necessarily the most important element in the enactment of the JOBS Act; for instance, the President's State of the Union speech in January 2012 may well have been more important. However, our empirical strategy (described more fully in Section 4 below) requires events that occurred after the retroactive application of the bill became known on March 1, 2012. Among these events, the March 15 consideration by the Senate and the strong endorsement by the Senate Majority Leader are likely to be the most important in affecting the perceived likelihood of eventual enactment.

promote them.[37] If the retroactivity provision was the result of lobbying by specific firms that had already conducted their IPOs after December 8 (or were about to do so), then it is possible that EGC status is correlated with firms' valuation of the JOBS Act. As this may confound our results, we undertake a robustness check that omits EGCs that lobbied for the Act or were otherwise politically active (see Section 5 below).

Another key question in terms of research design is whether the market anticipated the retroactive application of certain provisions of the JOBS Act and whether this may confound our interpretation of the findings. As noted earlier, we do not find the retroactivity provision in the public record prior to March 1, 2012 and it is not very common to see retroactivity in the securities law context. However, it may still be possible that the market anticipated the retroactivity provision, perhaps even from the beginning of the legislative process on December 8, 2011. If so, then the anticipated costs and benefits of the JOBS Act provisions would subsequently have been capitalized into the value of new IPO firms on their IPO date. It is thus important to our analysis that there was a subsequent (post-IPO) event that affected the likelihood of the bill's enactment. As argued above, the March 15 events in the Senate can be viewed as resolving much of the remaining uncertainty (as to the likely date, and likelihood, of enactment). Thus, even if there was some anticipation of the retroactivity provision, we would still expect a market reaction around March 15.[38]

4) Data and Empirical Strategy

4.1) Data

The dataset for this analysis is based on hand-collected data on firms that conducted IPOs in the months immediately before and after the December 8, 2011 cutoff for EGC status. In

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[37] Note, however, that firms that conducted IPOs after December 8 and before April 5 only obtained the post-IPO benefits (e.g. not being subject to certain SOX provisions), and not the reduced costs of conducting an IPO. Thus, firms that viewed the costs of conducting the IPO as being substantial may still have delayed their IPO beyond April 5 to take advantage of the cost reductions included in the JOBS Act. This may entail potential selection bias, as firms that delay would presumably be those that place the most value on the new IPO process. If firms' valuation of the post-IPO reductions in disclosure obligations is positively correlated with their valuation of the new IPO process (which seems to be a reasonable assumption), then this response by firms would merely create a bias against our findings. Essentially, the sample of firms that conduct IPOs prior to the enactment of the JOBS Act would consist of firms that place a lower value on the easing of regulatory burdens.

[38] Even if firms contemplating IPOs anticipated the retroactivity provision, it is unlikely that they would accelerate their IPOs as a result. As discussed in Section 4.2 below, the IPO process typically takes somewhere between 6 months and a year, leaving little scope for such a response. Moreover, firms that accelerated their IPOs would have had to conduct their IPOs under the (costlier) pre-JOBS Act regime.

particular, we collect data on IPOs conducted on the US market over the period from July 2011 to April 5, 2012, using the Securities Data Company (SDC) new issues database, the Securities and Exchange Commission's Electronic Data Gathering and Retrieval (EDGAR) system, and the IPO database maintained by Jay Ritter at the University of Florida.[39] Using these sources, we find a total of 87 firms that conducted IPOs over this period. For these firms, we also hand-collect data on revenue in the most recently completed fiscal year, the public float (the aggregate worldwide market value of the voting and non-voting common equity held by non-affiliated shareholders), accelerated filer status and other variables from the SEC's Electronic Data Gathering and Retrieval (EDGAR) system. A few of these IPOs are by publicly-traded investment companies (typically, closed-end funds). We identify these funds through their SEC filings (for instance, whether they report being subject to the Investment Company Act of 1940) and exclude them from the main analysis as they are largely unaffected by the JOBS Act (they are, however, used in a placebo test, as described in Section 5).

We merge this data with Compustat financial statement information (on assets, revenue, earnings, debt, R&D expenditures, market value, IPO date and other variables) and Center for Research in Security Prices (CRSP) data on firms' daily returns and market returns. We use the data on IPO date, revenue in the most recently completed fiscal year, and other relevant variables to determine which of these firms satisfy the JOBS Act's criteria for EGC status. To compute the number of trading days since a firm's IPO, we use as the IPO date the first date on which CRSP data is available for the firm. However, the results are similar when using instead a combination of SEC and Compustat data to define the number of trading days since a firm's IPO.[40]

The central variable determining whether a firm with a post-December 8 IPO is an EGC is its revenue in the most recently completed fiscal year. The revenue variable used in the analysis combines the Compustat variable REVT (Total Revenue) with hand-collected data on revenue from SEC filings for those firms with missing Compustat data. At the time that the key event dates occurred (March, 2012), the most recently completed fiscal year for a typical firm with a December fiscal year-end would have been fiscal year 2011. We use the Compustat

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[39] This dataset is available at: http://bear.warrington.ufl.edu/ritter/ipodata.htm, and is an updated version of the dataset described in Loughran and Ritter (2004).

[40] There are three distinct sources of data on IPO dates – the hand-collected data from the SEC filings that includes the date of the IPO, the Compustat variable IPODATE (defined as "Company Initial Public Offering Date"), and the first date on which CRSP data is available for the firm. There are some missing values of the Compustat variable IPODATE, and some minor discrepancies among the three data sources. These discrepancies do not, however, affect the classification of any firms as conducting IPOs before or after December 8, 2011.

variable "Fiscal Year-End" to determine the month in which each firm's fiscal year ends. For virtually all firms in the sample, the most recently completed fiscal year is fiscal year 2011. A few firms, however, have different fiscal year-ends, and this is taken into account in defining the appropriate fiscal year for measuring revenue.[41]

Certain other factors are also included in the JOBS Act as criteria for determining EGC status, but are of limited relevance for most firms in our sample. Firms classified by the SEC as large accelerated filers (with a public float exceeding $700 million) are not eligible for EGC status. We hand-collect data on each firm's public float from SEC filings, but only one firm that would otherwise be an EGC is sufficiently large in terms of public float to be above the $700 million threshold (and omitting this firm from our analysis does not affect the results). Similarly, very few firms in our sample report sufficient outstanding debt to potentially be above the debt issuance threshold (omitting these firms also does not affect the analysis).

Taking account of missing data, our control group consists of 33 firms (with less than $1 billion in revenues that conducted IPOs prior to December 8, 2011). The treatment group of EGCs varies in size from 25 to 41, depending on the date. We have 25 EGCs that conducted IPOs prior to the first major legislative event (on March 1). We have 27 treatment firms for our most important tests, which relate to the events in the Senate on March 15. There are 41 EGCs that conducted IPOs prior to the final event (the Presidential signature on April 5). Very few firms that went public in this period exceeded the $1 billion revenue threshold, with 5 such firms conducting IPOs after December 8, of which only 2 conducted IPOs prior to the events in the Senate on March 15.

4.2) Empirical Strategy

This paper's empirical strategy is based on using an event study approach to measure abnormal returns for EGCs around major legislative events in March 2012 that increased the probability of the JOBS bill's enactment. This provides a direct test of investors' expectations about whether or not the value of the mandatory disclosure obligations that the JOBS bill relaxed exceed the associated compliance costs. The partial retroactivity of the JOBS Act's definition of an EGC is thus crucial to this strategy. As described in Section 3 and depicted in Figure 2, the JOBS Act provides potential quasi-experimental variation along both a firm size dimension (the

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[41] For instance, a firm with a March fiscal year-end would have completed its most recent fiscal year (prior to the first major legislative event on March 1, 2012) on March 31, 2011, and its revenue in the most recently completed fiscal year would be revenue in fiscal year 2010.

$1 billion revenue threshold) and a temporal dimension (the December 8 cutoff). However, a regression discontinuity approach around the $1 billion revenue threshold, while attractive in principle, is precluded by the small number of firms that lie above the threshold, with 5 such firms conducting IPOs after December 8, of which only 2 conducted IPOs prior to the events in the Senate on March 15.

The firms subject to the "treatment" (i.e. EGC status) are all newly traded on public markets and within a few months at most of their IPO. Identifying a control group for these firms is a challenge, as the rest of the market may not necessarily provide an ideal baseline.[42] Moreover, the number of firms that conducted IPOs over the same period (after December 8, 2011 and before the key event dates in March 2012) and that did not satisfy EGC criteria (notably by having revenues greater than $1 billion) is very small, with only two firms having usable data. This effectively precludes using the "large" firms as the control group (though a supplementary analysis that uses them as the control group leads to similar results). Thus, for the primary control group, we use firms that conducted IPOs from July 2011 to December 8, 2011 and that satisfied the EGC criteria (apart from the IPO date). This yields a control group that is of comparable size to the treatment group, and that has very similar observable characteristics

Our empirical tests compare abnormal returns for the treatment firms with abnormal returns for the control firms over various relevant event windows. The basic identifying assumption is that, conditional on a firm conducting an IPO over the July, 2011 to April, 2012 period, whether it did so before or after December 8 can be considered to be quasi-random with respect to the factors that generate abnormal returns on the key event dates for the JOBS Act. This assumption appears reasonable, given the significant lead time involved in preparing and implementing an IPO (which is often considered to be at least 6 months).[43]

A critical empirical challenge is that this sample, especially the treatment firms, consists of firms that are close to their IPO date. This may raise concerns, given the large literature in finance on IPO underpricing (e.g. Loughran and Ritter, 2004; Ljungqvist, 2008). We address these concerns in a number of ways. In the regression analysis, we find robust results when we

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[42] A propensity score matching approach that matches the treatment firms with otherwise similar existing firms is possible in principle, but it would fail to address the critical issue of the treatment firms' youth as publicly-traded entities.

[43] For instance, one guide prepared by a financial consulting firm specifies the timeframe as 6-9 months - see http://www.publicfinancial.com/articles/timeframe-to-go-public.html. Pwc's guide for 2011 "Roadmap for an IPO: A Guide to Going Public" (available at: http://www.pwc.com/us/en/transaction-services/publications/roadmap-for-an-ipo-a-guide-to-going-public.jhtml) envisages a timeframe of 6-12 months (p. 35).

control for the number of trading days since a firm's IPO and exclude firms that are one month or less from their IPO date. It should also be borne in mind that IPO underpricing in the US market appears to be primarily a phenomenon that affects the first trading day. Indeed, a standard practice in the IPO underpricing literature is to measure underpricing using first-day returns; using first-week returns leads to very similar underpricing measures (e.g. Ljungqvist, 2008). We exclude firms' first trading day from all of our tests. Firms may also experience greater volatility during the earlier phases of public trading, but this would tend to create a bias against any significant findings.

4.3) The Market Model and the Computation of Abnormal Returns

Event studies in the scholarly literature use a variety of approaches to estimate firms' normal or predicted returns. We use the market model and the Fama-French model (described in Section 4.4 below), both of which are widely used in the literature. The market model does not rely on a specific set of economic assumptions, and is thus in some respects less restrictive. We use a market model to compute abnormal returns for the firms in our sample over a (-1, +1) event window that spans the period from the release of the House Financial Services Committee report on March 1, 2012 to the Presidential signature. This period from February 29 to April 9, 2012 is referred to as the "full event window" in the discussion below. A (-1, +1) window, which starts one trading day before the event and ends one trading day afterwards, is frequently used in the event study literature, as it accommodates some degree of anticipation or leakage of information immediately prior to the event, and allows some scope for delayed reaction. However, it does not unduly dilute the impact of the event by extending the window beyond a day on either side of the event.

The market model for firm i uses daily returns for firm i and for the market, and can be represented as follows (see e.g. Bhagat and Romano, 2002, p. 146):

$$R_{it} = a_i + b_i M_t + e_{it} \tag{4}$$

where R_{it} is firm i's return on day t, M is the market return on day t, and e is the error term. We run this regression separately for each firm over an estimation window that begins on the first day that returns data is reported for that firm in CRSP (if that date is prior to February 29) and ends on December 31, 2012, excluding the full event window defined above (February 29 to April 9, 2012). For example, for a firm that first appears in CRSP on August 15, 2011, we use as the estimation window the period from August 15, 2011 to February 28, 2012 and the period

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from April 10, 2012 to December 31, 2012. For a firm that first appears in CRSP in March 2012, we use the period from April 10, 2012 to December 31, 2012 as the estimation window. Using a post-event period as part of the estimation window is fairly common in event studies, although the more standard practice is to use the pre-event period. In our situation, many of the firms in our sample have only a limited pre-event returns history (and some have no pre-event return history), so the use of an estimation window that includes the post-event period through December 31, 2012 is indispensable to our analysis.

We use the results of running Equation (4) separately for each firm to compute (for each firm i) a predicted return on each day of the full event window (February 29 to April 9, 2012). We then subtract this predicted return from the actual return on each day of the full event window to obtain the abnormal return (AR_{it}) for each firm i on each of these days:

$$AR_{it} = R_{it} - \widehat{R_{it}} \tag{5}$$

where $\widehat{R_{it}}$ is the predicted return for firm i (i.e. $\widehat{R_{it}} = \hat{a} + \hat{b}M_t$, where $\hat{a}$ and $\hat{b}$ are the estimated coefficients from the regression in Equation (4) for firm i). These abnormal returns are then used to compute cumulative abnormal returns (CARs) for each firm for the full event window and for various relevant shorter windows. For firm i:

$$CAR_i = \sum_t AR_{it} \tag{6}$$

where the abnormal returns (AR_{it}) for firm i are summed over each of the relevant intervals.

4.4) The Fama-French and Carhart Four-Factor Model

A widely used set of alternatives to the market model is based on the Capital Asset Pricing Model (CAPM), which posits that R_{it} depends on the difference between the market return (M_t) and the risk-free rate of return (denoted F_t) on day t. To improve the ability of the model to predict returns, Fama and French (1993) added two factors to the CAPM – a "small minus big" factor (SMB_t) that represents the difference between returns on day t of stocks with a small market capitalization and those of stocks with a large market capitalization, and a "high minus low" (HML_t) factor that represents the difference between returns on day t of stocks with a high book-to-market ratio and those of stocks with a low book-to-market ratio. Carhart (1997) further augmented the model by introducing an "up minus down" momentum factor (UMD_t) that represents the difference between returns on day t of stocks that have increased in value over the past year and those of stocks that have decreased in value over the past year.

This four-factor model, which is now widely used in the literature, can be represented as follows (see e.g. Kothari and Warner (2007, p. 25)), using the notation introduced above:

$$R_{it} = a_i + b_{1i}(M_t - F_t) + b_{2i}SMB_t + b_{3i}HML_t + b_{4i}UMD_t + e_{it} \qquad (7)$$

We use the results of running Equation (7) separately for each firm to compute (for each firm i) a predicted return on each day of the full event window. We then subtract this predicted return from the actual return to obtain Fama-French abnormal returns and CARs, in a manner analogous to that shown in Equations (5) and (6) above.

4.5) Regression Analysis

The central empirical hypothesis of this paper concerns whether the CARs for the treatment firms differ from those for the control firms during the windows defined by crucial legislative events in the history of the JOBS Act. To formally test this hypothesis, we use a regression framework to test whether the CARs for the treatment firms are significantly different from those for the control group of firms. The basic regression model is:

$$CAR_i = \alpha + \beta EGC_i + \epsilon_i \qquad (8)$$

where EGC_i is an indicator variable that is equal to 1 if firm i conducted its IPO after December 8, 2011, and had less than $1 billion of revenue in its most recently completed fiscal year (the primary criteria for EGC status), and is equal to zero otherwise.

Augmented with various control variables, the regression model is:

$$CAR_i = \alpha + \beta EGC_i + \zeta REV_i + \vartheta DAYS_i + \mathbf{X}_i \xi + \epsilon_i \qquad (9)$$

where:

REV_i is firm i's revenue in its most recently completed fiscal year (typically fiscal year 2011, but defined taking into account firm i's own fiscal year end-date, as described above)

$DAYS_i$ is the number of trading days since firm i's IPO, calculated at the beginning of the event window to which CAR_i pertains.[44]

$\mathbf{X}_i$ is a vector of additional control variables from Compustat. These include total assets (Compustat variable AT), long-term debt (Compustat variable DLTT), earnings before interest, taxes, depreciation and amortization (Compustat variable EBITDA), and

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[44] For example, for the full event window, this would be the number of trading days from firm i's IPO date to February 29; for the March 14-16 event window, this would be the number of trading days from firm i's IPO date to March 14). The IPO date is based on the date the firm first appears in the CRSP data, but the results are robust to using the IPODATE variable from Compustat and hand-collected IPO dates from the SEC website.

research and development (R&D) expenditures (Compustat variable XRD) for fiscal year 2011. R&D expenditures are defined such that missing values are set to zero.

We also use a number of other variables for additional robustness checks. These include the Compustat variables listed above for fiscal year 2012 (although there is a significant number of missing values for these), and the Compustat variable reporting market value (MKVALT) for fiscal year 2012. We also use a similar set of Compustat quarterly variables for the first quarter of 2012. Firms' public float (which is important in defining accelerated filers) is hand-collected from SEC filings for fiscal year 2012.[45]

Before proceeding with the analysis, it is important to check whether the treatment and control groups appear to be comparable in terms of the various firm characteristics represented by the control variables. Table 2 reports descriptive statistics for the control variables used in the regression analysis and in robustness checks, separately for the treatment firms and the control firms. The set of treatment firms here consists of those that had completed IPOs before March 14, 2012, to correspond to the sample used in the regression analysis. On the whole, the two groups look very similar along these dimensions. In particular, the crucial variable for determining EGC status (revenues in the most recently completed fiscal year) is very similar across the two groups. Many of the variables, such as earnings, are remarkably similar across treatment and control firms. While there are some differences, there is nothing to indicate that the treatment and control firms are of substantially different size, or have other substantially divergent characteristics.[46] The exception, of course, is the number of trading days from a firm's IPO to March 14: this is approximately 31 days on average for the treatment firms and approximately 122 days on average for the control firms. This difference, however, is unavoidable given the construction of these groups, and the limitations of the quasi-experiment that Congress has provided.

5) Results

5.1) Comparing Abnormal Returns for Treatment and Control Firms

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[45] Market value and public float are not meaningful for many of the treatment firms in 2011, as they were not publicly traded for most or all of that year.
[46] Formal t-tests show that the differences in the means of these variables across the treatment and control groups are statistically insignificant, except for the difference in the number of trading days since a firm's IPO.

Having obtained the daily abnormal returns for each firm, a first step in the analysis is to compare the CARs over this period for the treatment and control firms. Table 3 reports the average CARs for the treatment and control firms for the full event window and for six potentially relevant shorter windows. The first of these shorter windows is around the House Committee report of March 1 and spans February 29 to March 2. The second window extends the first one to encompass the entire period of House deliberation and the March 8 vote (February 29 to March 9). The third is around the March 15 event that signaled prioritization of the bill in the Senate (March 14-16). The fourth window extends this to the March 22 Senate vote (March 14-23). The fifth window is around the March 27 House vote on the amended Senate bill (March 26-28). The final window is around the President's signature (April 4-9).

The third column of Table 3 reports the mean CAR among treatment firms, the standard error, and the number of firms in the group for each of these windows.[47] The CARs reported in Table 3 are obtained using the market model, but the patterns are very similar for the Fama-French CARs (with the partial exception of the March 1 event, as discussed below). The fourth column of Table 3 reports corresponding values for the control firms. The final column reports whether the differences between the CARs for the treatment and control firms are statistically significant. This is determined using a regression similar to that in Equation (8), in which the CARs for both groups of firms are regressed on an indicator variable for EGC status. However, a series of t-tests with unequal variances gives qualitatively similar results.

If we were to take the event study results over the full event window at face value, it would appear that there was a large positive and statistically significant CAR for the treatment firms. However, the control firms also experienced a large CAR over this period (albeit one that is not statistically significant). The difference between the CARs for the treatment and control groups is not statistically significant. This may be due to the length of the window (especially given the relatively small number of affected firms), and because the full event window potentially dilutes the effect by including many events that may not have conveyed any information to market participants. Thus, we focus on the shorter windows defined above.

The central result that emerges from Table 3 is the importance of the March 15 event, when the Senate Majority Leader signaled the importance of the bill and its high priority. As

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[47] Mechanically, the mean CAR and standard error are obtained by regressing the CARs for the treatment firms on a constant.

may be expected *a priori*, there is a substantial abnormal return for the treatment firms (of about 3.5%). This is statistically significant, and is also significantly higher than the abnormal return experienced by control firms. This is the only event to give rise to a statistically significant difference in abnormal returns between the treatment and control groups (and, as discussed below, March 15 is the only date anywhere within the full event window on which there is a statistically significant difference between the treatment and control firms). The March 1 event represents a partial exception, in that the treatment firms experienced an abnormal return that is of borderline statistical significance. The difference between the treatment and control firm market model CARs is statistically significant. However, this difference is insignificant using Fama-French CARs (and is not robust to the inclusion of even a minimal set of controls in a regression framework). Thus, we treat the March 1 outcome as being statistically insignificant (see Section 5.6 below for further discussion).

When the March 15 window is extended to encompass the Senate deliberations and vote (March 14-23), the CAR for the treatment firms remains significant. However, it is no longer significantly different from the CAR for the control firms. This suggests that the impact of the Senate deliberations was concentrated immediately around the March 15 event. The period of House deliberation (February 29 to March 9) gave rise to a higher CAR for the treatment firms, but this CAR is not statistically significantly different from zero, and is not statistically significantly different from the CAR experienced by the control firms over that period. The House vote on the amended Senate bill (March 26-28) gave rise to a higher CAR for the treatment firms. However, this CAR is not statistically significantly different from zero, and is not statistically significantly different from the CAR experienced by the control firms over that period. Finally, the President was widely viewed as being favorable to the bill, and so it is not surprising that the abnormal returns for the treatment firms around the Presidential signature are essentially zero, and statistically insignificant.

While this is not shown in Table 3, we also conduct the same analysis for all other dates within the full window (February 29 to April 9). For the "nonevent" dates (on which no new information about the JOBS bill appeared), this serves as a placebo test to determine whether there were significant differences between the treatment and control firms for reasons unrelated to the JOBS Act. This analysis reinforces the basic conclusion that the only statistically significant difference between these two groups of firms occurs around March 15. The two

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groups of firms both experience essentially zero abnormal returns on most nonevent days (as well as on many "event" days), and the difference between their abnormal returns is not statistically significant on any nonevent day. In particular, there is no preexisting trend or pattern indicating higher abnormal returns for EGCs in the days immediately prior to the March 14-16 window. Around March 12, there is a quantitatively large negative CAR for EGCs. While there was widespread expression of opposition to the JOBS bill around this time, there were no legislative events. Thus, we are cautious about interpreting this negative CAR as being related to the JOBS bill; in any case, the difference between the CARs for the treatment and control firms is not statistically significant.

As all EGCs experience a given legislative event on the same day, a potential problem for inference is the possible cross-correlation of returns across EGCs on the event dates. A common approach to addressing this potential problem is to aggregate the sample firms into a single portfolio and to estimate the portfolio CARs around the event dates (see e.g. Kothari and Warner, 2007). This procedure renders moot any cross-correlation among the returns of different firms. We thus aggregate all of the EGCs in our sample into an "EGC portfolio" and compute its CAR around March 15. This portfolio experiences a 4.2% CAR over March 14-16, and this CAR is statistically significant (the test statistic is 2.22). Another approach to addressing cross-correlation and other potential problems with conventional standard errors is to use bootstrapping (Kothari and Warner, 2007). Inferences using bootstrapped standard errors are very similar to those using the conventional standard errors reported in Table 3.

Overall, the results in Table 3 confirm the *a priori* expectation of the importance of the March 15 event, and reflect the comparative lack of importance of the various other events (and of the nonevent days within the full window). Thus, the regression analysis focuses on the CARs over the March 14-16 window, as described in the next subsection.

5.2) Basic Regression Results

The results from the regression in Equation (8), for the market model CAR over the March 14-16 window, are reported in Column 1 of Table 4. The indicator for EGC status is positive and significant, confirming that the treatment firms experienced a significantly higher CAR (of close to 4%) over this window than did the control firms. The results are very similar when using the Fama-French CARs, as reported in Column 2 of Table 4 (where the use of CARs

based on Equation (7) implicitly controls for differential returns over the event window by size, book-to-market ratio and momentum).

It is possible that the shorter period since the IPO date for the treatment firms may bias the results, as might differences in firm size. Column 3 of Table 4 reports the results when two control variables – revenue in the most recently completed fiscal year and trading days since the firm's IPO – are added to the regression model. To further mitigate any bias that may be due to differential post-IPO returns behavior, Column 3 of Table 4 excludes firms with an IPO date one month or less prior to the event window (i.e. all firms with IPOs on February 15 or later are excluded). This entails omitting 6 firms, but the results shown in Column 3 are very similar to the baseline results.

Column 4 of Table 4 reports the results of a regression corresponding to Equation (9). This includes a wider set of controls, including the Compustat variables total assets, long-term debt, earnings (EBITDA) and R&D expenditures for fiscal year 2011 (as well as revenues and trading days since IPO). Once again, the results are very similar to the baseline results. They are also very similar when similar variables from the Compustat quarterly data for the first quarter of 2012 are used instead (these results are not reported for reasons of space). Another specification involves adding the Compustat variables total assets, long-term debt, earnings (EBITDA) and R&D expenditures for fiscal year 2012, in addition to the same variables for fiscal year 2011 (and revenues and trading days since IPO). The fiscal year 2012 variables would not have been known to market participants at the time of the legislative events we examine. However, including both the 2011 and 2012 variables provides a flexible specification of changes in these variables that may have been anticipated by market participants and thus could potentially affect the abnormal returns. Missing values in Compustat for the 2012 variables leads to a substantial reduction in sample size, but the EGC variable remains significant (these results are also not reported for reasons of space).

As previously discussed, all EGCs experience a given legislative event (such as the March 15 developments in the Senate) on the same day. Thus, a potential problem with inference using regression specifications such as Equations (8) and (9) is that the standard errors may be contemporaneously correlated across firms (e.g. Salinger, 1992). Assuming that such correlation is stronger within industries, one possible approach to addressing this issue is to cluster the standard errors at the industry level. We use 2-digit Standard Industrial Classification (SIC)

industries, obtained from Compustat and augmented with hand-collected SIC codes from the SEC's EDGAR website. The results in Table 4 are robust to clustering standard errors at the 2-digit level (these results are also not reported for reasons of space). Unfortunately, due to the small sample size, it is not possible to use a finer degree of disaggregation of industries than the 2-digit level.[48]

It is also possible that abnormal returns over the event window differ across industries for reasons unrelated to the JOBS Act. Thus, we use these 2-digit SIC codes to create industry fixed effects to take account of this possibility. Column (1) of Table 5 reports the results of a regression corresponding to Equation (8), augmented with industry effects at the 2-digit level. As this specification restricts the estimation to within-industry variation, the effective sample size is substantially reduced (there are 23 industry clusters among the 60 firms). Nonetheless, the basic result is robust to the inclusion of industry effects. When industry effects are combined with an extensive set of control variables, however, the EGC coefficient's significance drops away. We attribute this not to the absence of an effect, but to the very limited effective sample size in specifications of this type.

5.3) An Alternative Test

The main analysis uses firms with pre-December 8 IPOs as the control group. An alternative control group consists of the large firms that conducted IPOs after December 8. Using this control group potentially controls better for immediate post-IPO effects, since the control firms have very similar IPO dates to the treatment firms. However, it may control less well for size and associated characteristics, if the returns experienced by firms depend on size. As foreshadowed earlier, the problem with this control group is the small number of non-EGCs that conducted IPOs over the relevant period. Five such firms conducted IPOs after December 8, only 2 of which conducted IPOs prior to the events in the Senate on March 15.

Nonetheless, if we use these 2 large firms as the control group, the basic result is robust. Column 2 of Table 5 reports the results of a regression analogous to that in Equation (8), but with the sample consisting of treatment firms and the 2 large firms in the alternative control

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[48] The small sample size also limits the scope for implementing other cross-sectional tests. For instance, if regulatory burdens are more severe for smaller firms, we might expect that the EGC effect would be larger for smaller firms. However, interactions between the EGC dummy and various size variables are statistically insignificant. Whether the EGC effect is larger for firms with stronger governance may help shed light on whether disclosure and governance are substitutes or complements. However, interactions between the EGC dummy and proxies for governance (such as institutional ownership) are statistically insignificant.

group (with the pre-December 8 control group omitted). The coefficient on the EGC variable is significant and very similar in magnitude to that in the baseline results. Of course, this result should be treated with great caution, given the small size of the control group. Nonetheless, it provides some evidence that the higher CARs for EGCs over March 14-16 are not due to confounding post-IPO returns behavior.

5.4) Placebo Tests

A potential concern with the baseline results is that differences in abnormal returns across the treatment and control firms are driven by their (slightly) different IPO dates, rather than by investors' reactions to the JOBS Act. A general approach to addressing these types of concerns is to use placebo tests - in particular, false experiments in which the ostensible treatment group conducted IPOs over the same (post-December 8) period as the EGCs, but were not subject to the JOBS Act provisions. If these firms also experience higher abnormal returns over March 14-16 than do the control firms, then the baseline results cannot be attributed to the JOBS Act.

There are two potential placebo groups in our data, but unfortunately both are quite small in size. The first is the set of large firms (with revenues exceeding $1 billion) that conducted IPOs after December 8. As discussed above, there are only two of these firms with usable data. Column 3 of Table 5 reports the results from a regression similar to Equation (8) in which the "treatment" group consists of the 2 large post-December 8 IPO firms and the control group is the standard one used in the baseline results (i.e. firms with pre-December 8 IPOs and less than $1 billion in revenue). The coefficient on the indicator variable for the "treatment" firms is not only statistically insignificant (which may simply reflect the small sample size) and negative in sign, but also small in magnitude. The 95% confidence interval is [-0.0240, 0.01578], implying that we can rule out a positive CAR of more than about 1.6%. This is substantially smaller than the effect found in the baseline results.[49]

A second potential placebo group consists of investment companies (typically, closed-end funds) that conducted IPOs over the post-December 8 period. These funds are subject to the Investment Company Act of 1940, and this different regulatory regime implies that they were

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[49] It is possible that the small firms in our control group form a poor control for these large post-December 8 firms, for instance, if abnormal returns are driven by firm size or associated characteristics. An alternative placebo test is thus to use as the control group the large firms (with revenue above $1 billion) that conducted IPOs prior to December 8. There are only 2 such firms in our dataset, however, so regression analysis would not be meaningful. Instead, we examine the mean CARs for these two groups of firms. The large post-December 8 firms (the placebo "treatment" group) experienced negative and statistically insignificant abnormal returns around March 15. There is no indication that this placebo treatment group experienced CARs comparable to those of the true treatment group.

largely unaffected by the JOBS Act. However, they may be subject to some of the same effects associated with "newness" (such as investor sentiment) as the EGCs. Unfortunately, there are only 2 such funds that conducted IPOs over the relevant period. Column 4 of Table 5 reports the results of a regression similar to Equation (8) in which the "treatment" group consists of the 2 post-December 8 IPO funds and the control group is the standard one used in the baseline results (i.e. firms with pre-December 8 IPOs and less than $1 billion in revenue). Again, the coefficient on the indicator variable for the "treatment" firms is not only statistically insignificant (which may simply reflect the small sample size) and negative in sign, but also small in magnitude. The 95% confidence interval is [-0.0259, 0.01521], implying that we can rule out a positive CAR of more than about 1.5%. This is substantially smaller than the effect found in the baseline results. Taken together, these placebo tests suggest that the baseline results are not driven simply by differences in IPO dates.

5.5) Interpreting the Magnitude of the Effect

In combination with the CAR for treatment firms reported in Table 3, the coefficients on the EGC indicators in Columns 1 and 2 of Table 4 entail that the treatment firms experienced a positive abnormal return of between 3% and 4% as a result of the March 15 event that increased the likelihood of the enactment of the JOBS Act. The mean market value for EGCs in our sample is $760 million (as reported in Table 2), while the median market value is about $600 million. Thus, for the median firm, this result implies an increase in market value of over $20 million around March 15.

To quantify the total change in value associated with the relaxed disclosure and compliance obligations of the JOBS Act, we need to know the change around March 15 in investors' perception of the probability of the enactment of the JOBS bill. While this is obviously impossible to observe directly, the nature of the events surrounding the JOBS bill provides a means of inferring this change in probability, under certain additional assumptions. Suppose that investors' estimate of the total treatment effect associated with the JOBS Act remained fixed over the full event window (February 29 to April 9). As a first step, note that events subsequent to March 15 did not give rise to any statistically significant abnormal returns for EGCs relative to control firms (see Table 3 and the discussion in Section 5.1 above). Thus, the perceived probability of enactment after March 15 can be presumed to be 1, as otherwise there would have been some further subsequent updating of beliefs.

The probability of enactment combines two conceptually separate notions – the probability of the bill's passage, and the probability that its provisions would be retroactively applied to our treatment firms. The latter probability can reasonably be assumed to have been zero prior to March 1 (as there was no public announcement of the December 8 cutoff before March 1) and to have increased to 1 on March 1 (as all subsequent versions of the bill contained the partial retroactivity provision). Prior to March 1, investors held some belief about the probability of enactment, but this would not have been reflected in their valuation of our treatment firms, as there was no indication at that time that these firms would become subject to the new legal regime. The market reaction around March 1, however, would have capitalized this preexisting probability of enactment (along with any increase in that probability due to the House Committee report) into the value of our treatment firms. Thus, this market reaction allows us to infer investors' perceived probability of enactment.

There is a 2% abnormal return for treatment firms around March 1 (see Table 3). However, as discussed in Section 5.1, this is only of borderline statistical significance, and is not robustly significantly different from the returns for control firms. If we thus view the March 1 CAR for EGCs as indistinguishable from zero, then the aggregate increase in EGCs' value over the full period is simply the March 15 effect (about 3.5% in Table 3). Moreover, a zero March 1 CAR implies that the perceived probability of the JOBS bill's enactment was zero at that time.[50] Therefore, this probability can be inferred to have increased from zero to 1 on March 15, with the concomitant implication that the total change in value associated with the relaxed disclosure and compliance obligations of the JOBS Act is equal to the March 15 effect (i.e. around $20 million for the median EGC). Although there may be reason to view the March 1 CARs as being effectively zero,[51] if we were to adopt the somewhat less conservative position that the March 1

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[50] Let p_E be the probability of enactment, p_R be the probability of retroactivity, and X be the aggregate treatment effect of the JOBS Act. On March 1, $p_E \Delta p_R \, X = 0$ (note that this is p_E, rather than the change in p_E, because the entire prior probability of enactment is reflected in treatment firms' value upon the announcement that they will become subject to the JOBS bill provisions). Then, assuming that $\Delta p_R = 1$, and for any nonzero X, it follows that $p_E = 0$.

[51] Given the President's support for legislation of this type, and the overwhelming popularity of the JOBS bill in the House, it may seem surprising that investors would have perceived a very low or zero likelihood of enactment prior to March 1. This may not be unreasonable, however, given the prospect of opposition in the Senate, as well as general (and perhaps - at least in *ex ante* terms - well-founded) skepticism about the possibility of any legislative action, however popular the cause, in an era of divided partisan control of Congress.

effect was nonzero, then the total impact of the JOBS Act would be about $33 million for the median EGC.[52]

Another important issue that bears on the magnitude of the total change in value associated with the relaxed disclosure and compliance obligations of the JOBS Act is the elective nature of EGC status. At the time that we measure market reactions, there was no information about which EGC-eligible firms would choose to opt in to some or all of the JOBS Act provisions. However, it can be presumed that investors held some belief about the average probability of a firm choosing to take advantage of the new regime. To address this issue, we hand-collect data from firms' SEC filings about their SOX compliance status (as the SOX provisions were arguably the most important among the JOBS Act provisions). Of the 27 treatment firms in our primary empirical tests, we are able to classify 26 using the firms' disclosures about their SOX compliance status. Of these, 19 are not fully SOX-compliant, implying that they have elected to make use of the relevant JOBS Act exemptions, and 7 are fully SOX-compliant (indicating that they have opted out of EGC status for the SOX provisions). Thus, about three quarters of the treatment firms in our sample opt in to EGC status for the SOX provisions. If this is representative of a wider pattern of firm choices over other JOBS Act provisions, and if investors correctly anticipated this fraction, the baseline magnitude derived above would increase from about $20 million to about $27 million for the median EGC, discounting for the probability of opting out.[53]

5.6) The Role of SOX Compliance Costs

One of the potentially most important provisions of the JOBS Act involves the relaxation of SOX 404(b) compliance obligations. There is a large literature in accounting that analyzes the compliance costs associated with SOX 404. This literature has found the compliance costs to be substantial, especially (in relative terms) for smaller firms. Alexander *et al*. (2013) use survey responses of firms to estimate compliance costs (including additional audit fees and the cost of

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[52] Using the 2% abnormal return for EGCs around March 1 in Table 3, the total treatment effect would be about 5.5% (the sum of the March 1 and March 15 effects). Investors' prior perception of the probability of enactment would be inferred to be about 0.36, with that probability rising to 1 on March 15.

[53] If investors could predict which firms would opt in, then we might expect the market reaction to be concentrated among those firms. It does not appear, however, that the firms that ultimately chose to opt in enjoyed higher CARs than those that did not. It is possible that this may be because the firms that opted out of EGC status were substantially smaller than average – if it is the case that compliance costs are more burdensome for smaller firms, then this is the opposite of the pattern that investors may have anticipated. Thus, investors may not have been able to predict that these firms would opt out, and the observed market reaction would be averaged across all EGC-eligible firms.

employees' time). They find that on average the cost of compliance is $2.3 million per year. This would amount to about $12 million over the 5-year horizon of the JOBS Act exemption.

However, SOX compliance is likely to involve both fixed costs (for instance, of initially establishing internal control mechanisms) and variable costs (that are incurred each year that the firm is in compliance, such as audit fees). The EGCs in our sample went public prior to the enactment of the JOBS Act, and so would have expected to have to comply with SOX immediately. Thus, they are likely to have incurred the initial fixed costs of SOX at the time they went public. Once the JOBS Act was enacted, they could potentially save the variable costs for a five-year period. Thus, it is the variable rather than fixed costs of SOX compliance that are of greatest relevance to the effect we find. Grundfest and Bocher (2007) report evidence that the first-year cost of implementing SOX 404 was approximately $1.5 million for firms with market capitalization in the same range as that of the median EGC in our sample. This seems to be a reasonable proxy for the initial setup costs. Subtracting this fixed cost from the approximately $12 million cost over 5 years implies a variable cost of over $10 million over the 5-year horizon of the JOBS Act exemption.[54]

Hence, it appears that there is a substantial potential cost saving from the JOBS Act exemption with respect to SOX (of course, the JOBS Act does not exempt firms from all SOX Section 404 requirements, but the internal control requirements and auditor attestation are often thought to be particularly burdensome). The size of the effect we find on March 15 is thus of the same order of magnitude as (albeit larger than) the compliance cost savings from SOX 404(b) exemption.

To test empirically whether SOX compliance costs play a role in the effect we find, we use the fact that firms that are classified by the SEC as "nonaccelerated filers" (with a public float of less than $75 million) were exempt from compliance with the Sarbanes-Oxley internal control disclosures prior to the JOBS Act. These firms would thus be expected to derive smaller benefits from EGC status. We use the public float variable (hand-collected from SEC filings) to

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[54] Our conversations with senior practitioners in corporate and securities laws suggest that the costs of SOX compliance in the early years after its enactment (to which the Grundfest and Bocher (2007) estimate refers) would largely have been centered on the setup cost for the first year. This fixed cost component in those early years would have included a large "learning curve" element. Over time, however, firms and their attorneys became more familiar with SOX compliance. As a result, the fraction of compliance costs that were incurred at the beginning (e.g. at the IPO stage) declined. Thus, by the time of the JOBS Act, initial fixed costs are likely to have represented a smaller fraction of total SOX compliance costs than in earlier years; variable costs would have represented a corresponding larger fraction of the total cost of SOX.

classify firms as nonaccelerated filers; 4 of the EGCs in our sample have a public float of less than $75 million. Column 5 of Table 5 reports the results of a regression of the form:

$$CAR_i = \alpha + \beta EGC_i + \gamma(EGC_i * NAF_i) + \zeta NAF_i + \epsilon_i \qquad (10)$$

where NAF_i is an indicator variable that is equal to 1 if firm i has a public float of less than $75 million.

The effect for EGCs in our sample that are nonaccelerated filers is indeed smaller than that for other EGCs. The magnitude of the coefficient indicates that the positive effect of the JOBS Act largely does not apply to nonaccelerated filers. However, the interaction term is not statistically significant, perhaps because of the small number of nonaccelerated filers in the sample. Running the basic specification (Equation 8) on a sample that consists only of the control firms and EGCs that are nonaccelerated filers yields a coefficient on the EGC variable that is very close to zero (a point estimate of 0.0049) and statistically insignificant (this is not reported for reasons of space). This suggests that the JOBS Act effect exists only for those EGCs that were subject to SOX internal control disclosures, although conclusions are necessarily tentative given the small sample.[55]

5.7) Tests for Potential Alternative Explanations

5.7.1) Lobbying for the JOBS Act

If the partial retroactivity provision of the JOBS Act was the result of lobbying by specific firms that had already conducted their IPOs after December 8 (or were about to do so), then it is possible that EGC status is correlated with firms' benefits from the JOBS Act. In particular, under the lobbying assumption, firms in the control group (those that conducted IPOs from July 2011 to December 8, 2011) failed to obtain retroactivity to July 2011, and so might be presumed to value the JOBS Act less than do the treatment firms (which were successful in obtaining retroactivity back to December 8). Thus, it is important to test for the possibility that the retroactivity provision was the result of lobbying. To do so, we collect data on lobbying activity by EGCs and on political contributions by political action committees (PACs) associated

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[55] Iliev (2010) exploits the discontinuity in the application of SOX Section 404 at the threshold of a $75 million public float to analyze the impact of this SOX provision on market value when implementation began in 2004. Using a regression discontinuity design that compares firms around the $75 million threshold, Iliev (2010) finds that SOX Section 404 reduced firm value. This suggests that the compliance costs exceed the benefits of this provision, at least for small firms. This result is quite consistent with our findings regarding the broader set of disclosure and compliance provisions in the JOBS Act (including the relaxation of SOX Section 404(b)).

with EGCs.[56] Only one EGC reported lobbying for the JOBS Act. A broader group of 6 EGCs were "politically active" at any time for which data exists – i.e. they either lobbied Congress on any issue (not necessarily the JOBS Act specifically), or campaign contributions were reported from associated PACs. Column 1 of Table 6 reports the results of a regression that excludes these 6 EGCs from the sample. This specification is similar to that in Equation (9), and includes the set of controls from Column 4 of Table 4. The basic result is robust, suggesting that the findings are not confounded by lobbying or other political activity by EGCs.

5.7.2) Other Confounding Events Involving EGCs, and the Role of Outliers

While the EGCs in our sample are chosen based on the partially retroactive application of the JOBS Act, it is possible that the firms within this treatment group experienced other events during the window around March 15. To ensure that the results are not due to other potentially confounding events, we search for news stories mentioning any of the EGCs in our sample over the March 14-16 period that could potentially affect their share price. These include, for instance, stories about earnings announcements, press releases about firms' plans or operations, and the release of analysts' forecasts. In all, we find 12 EGCs that were mentioned in news stories in the relevant period. Column 2 of Table 6 reports the results of a regression that excludes these 12 EGCs. The basic result is robust, suggesting that the findings are not confounded by news stories reporting information about the EGCs unrelated to the JOBS Act.

The subset of firms mentioned in news stories includes two that are potential outliers, with particularly large positive abnormal returns. Of course, the robustness check reported above automatically excludes these firms. In addition, we exclude these two firms alone, and Winsorize the CARs to address potential outliers. The results are very similar in these additional robustness checks.

5.7.3) An Alternative Interpretation Involving Future Mispricing

The basic framework we use to interpret our results, developed in Section 2, emphasizes the tradeoff between the compliance costs associated with securities regulation and the value to outside investors of compliance. While this is a very standard conceptual framework, an alternative approach from the behavioral finance tradition emphasizes instead the possibility of

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[56] This information is from the Federal Election Commission website and the website opensecrets.org. Note that it is also possible that firms may exert political influence through their membership of trade associations or industry lobby groups. However, we focus on independent lobbying by EGCs, as it is unlikely that an industry-wide group would differentially advance the interests of the EGCs relative to the control firms.

mispricing. In particular, in a framework such as that of Bolton, Scheinkman and Xiong (2006), incumbent (sophisticated) shareholders value the opportunity to sell in the future to uninformed noise traders who overvalue the stock. In theory, it is possible that a legal reform that relaxes mandatory disclosure obligations may increase the likelihood of future mispricing (including overvaluation) – essentially, it would become easier to generate positive investor sentiment through selective or misleading disclosures. This would increase incumbent shareholders' option value of selling to noise traders in the future.

Observationally, the mispricing theory sketched above is substantially equivalent to our basic result, in that it would predict an increase in value for EGCs relative to control firms (which did not experience any change in disclosure obligations). To test whether the evidence is more consistent with our interpretation or with the mispricing interpretation, we collect data on analyst coverage from the International Brokers Estimate System (I/B/E/S) database. This database provides extensive information about analyst estimates. We focus in particular on the number of analysts following a given firm, and assume that there is no analyst coverage of firms that do not appear in the I/B/E/S data. The basic idea underlying this test is that mispricing is more likely to occur among firms with more limited analyst coverage (or none). Thus, the mispricing story should imply that the EGC effect would be concentrated among firms with less analyst coverage. This approach is consistent with a substantial literature in finance premised on the notion that greater analyst coverage is associated with less information asymmetry and mispricing (e.g. Chang, Dasgupta and Hilary, 2006).

Of the 27 EGCs, we classify 11 as having analyst coverage and 16 as having no analyst coverage. Column 3 of Table 6 reports the results of a regression where the treatment group consists only of EGCs without analyst coverage, while Column 4 of Table 6 reports the results of a regression where the treatment group consists only of EGCs with analyst coverage. The EGC coefficient is positive and statistically significant for both treatment groups, and moreover is virtually identical in magnitude. Using the full sample of EGCs and including an interaction between EGC status and analyst coverage results in the interaction term being statistically insignificant (this is not reported for reasons of space). Thus, this evidence does not suggest that the JOBS Act effect is concentrated among EGCs for which mispricing is more likely. Instead, it appears more consistent with the interpretation we have adopted (based on the framework in Section 2) rather than with the alternative mispricing interpretation.

5.7.4) Other Robustness Checks

As part of the IPO process, firm insiders generally agree not to sell more than a specified number of their shares for a specified period of time (typically, 180 days) following the IPO. These agreements are known as "lockups." The empirical literature has found that the end of the lockup period is associated with an increase in the supply of shares and with a significant decrease in the share price (e.g. Field and Hanka, 2001). It is possible that our results may be confounded by the expiration of lockups for the control firms (which may depress their price and make it appear that the treatment firms' relative value increases). We thus identify those control firms with IPO dates approximately 180 days prior to the March 15 window (i.e. an IPO date in September, 2011). Only one control firm has a September 2011 IPO date; excluding this firm from the analysis does not affect the results. Thus, it does not appear that our results are confounded by the expiration of lockups.

The definition of EGCs in the JOBS Act excludes firms that are classified by the SEC as "large accelerated filers" (with a public float of over $700 million), and also excludes firms that issue more than $1 billion of nonconvertible debt over a three-year period. One of the EGCs in our sample has a public float that exceeds $700 million (though it should be borne in mind that such a firm may still derive benefits from EGC status for a year or so, as large accelerated filer status is not attained until the firm files reports with the SEC for a year). Omitting the small number of firms in our sample that are large accelerated filers, or that have high debt levels, does not affect the results. EGCs may be subject to alternative forms of monitoring (e.g. by creditors) that make disclosure and SOX compliance less relevant; the exclusion of firms with high debt levels (and the use of a debt control in Table 4, Column 4) helps to address this possibility. Foreign private issuers are eligible for EGC status, but may benefit less from it than other firms. However, excluding the small number of foreign private issuers in our sample does not affect the basic results.

6) Discussion and Conclusion

In this paper, we use an unusual quasi-experimental setting created by the JOBS Act of 2012 to find what is, to the best of our knowledge, the first empirical evidence that "ratcheting" down securities regulation is associated with a positive market response. However, great care must be exercised in interpreting these results. First, although market responses may be treated as

indicative of the value that investors place on the reforms, it is not clear that the reforms only have value to investors of the particular firms subject to the regulatory changes. Reforms could have effects on other parties who are not accounted for in our tests.[57] A related point is that our empirical strategy requires measuring these market responses for firms that went public prior to the enactment of the JOBS Act (and which presumably originally expected to be subject to the old legal regime). It is possible that the relaxation of disclosure and compliance obligations may encourage fraudulent issuers to issue securities in the period after enactment. Such an effect, if it exists, would not be captured in our empirical analysis.

Second, even if we use the market response as the best first approximation of the value of the reforms, we caution that this should not be interpreted as evidence that mandatory disclosure is value reducing for investors as a general matter. Moreover, our findings, properly construed, should not be viewed as being in tension with prior studies finding large, significant and positive market responses to increases in regulation. These prior studies examine different types of reforms and have very different baselines. For example, Greenstone *et al.* (2006) find large positive effects when looking at the extensive reforms enacted in the OTC market in 1964. The OTC market was fairly lightly regulated prior to the reforms. The 1964 Amendments involved almost the entire corpus of the SEA being applied to many (but not all) OTC firms. Thus, their study addressed a situation where a lightly regulated market became much more heavily regulated. Our study, in contrast, looks at a situation where a particularly heavily regulated market becomes somewhat less heavily regulated for a subset of firms. For similar reasons, our results do not call into question the extensive body of cross-country evidence (e.g. La Porta *et al.*, 2006) finding that stronger securities laws foster stock market development, nor the single-country studies (e.g. Dharmapala and Khanna, 2013) finding positive effects of corporate governance reforms on firm value.

Assuming that regulation (like most other things) is subject to diminishing and ultimately negative returns, it is entirely consistent to find that large increases in regulation (relative to a low baseline) generate large increases in market value, while small reductions in regulation (relative to a high baseline) also generate an increase. This simple idea is depicted in Figure 1 (which represents the simple conceptual framework developed in Section 2). Note also that,

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[57] For instance, Langevoort and Thompson (2013) argue that a persistent theme in the history of securities regulation is a desire to hold large business enterprises accountable to the general public, in a way that is only tenuously related to standard notions of investor protection.

while Figure 1 assumes a single dimension of the "strength of regulation," in reality regulation is multidimensional. It is entirely possible that different dimensions of regulation (for instance, financial statement disclosure versus internal control requirements) may have differing impacts on shareholder value, and this may also help reconcile our findings with those of the previous literature. Within this context, we interpret our findings as providing quasi-experimental empirical evidence of the impact of regulation being relaxed when it may have gone beyond the optimal point for a specific set of firms (EGCs). Against the backdrop of the existing literature, this is an important and novel result regarding securities regulation in general, as well as being an important finding about the specific effects of the JOBS Act.

However, there are a number of important limitations to this analysis that should be emphasized. In general, these stem from the nature of the (presumably unintended) quasi-experiment that Congress has provided. First, the number of firms affected by the JOBS Act's partial retroactivity is small. In itself, this primarily creates a bias against finding any significant results. While we find a quite robust positive effect notwithstanding this limitation, the small sample makes it difficult to analyze how the effect varies across subsets of firms. The events that transpired during the legislative process, while providing some variation in the apparent probability of enactment, are also less than ideal. For instance, there are also no clearly negative events that reduce the probability of enactment (such as votes against the bill in committee or on the floor).

As a result of these limitations, we do not have conclusive evidence on which aspect of the reforms applicable to EGCs might have the greatest impact in generating the positive market response. The treatment firms in our study do not benefit from the provisions reducing IPO costs (because their IPOs occurred prior to April 5, 2012), but do benefit from the post-IPO provisions, including the SOX and accounting-related changes and a few changes in disclosure on executive compensation. Given that EGC firms that have just completed an IPO often have managers and owners whose interests are closely aligned, we would not expect that the disclosure costs of executive compensation would be very great (especially as they would have borne some of them in the IPO process). This suggests that, on an *a priori* basis, most of the post-IPO benefits are likely to center on the SOX and accounting-related changes.

One piece of evidence regarding the importance of the SOX-related provisions comes from the response of nonaccelerated filers (small firms that were not subject to the relevant SOX

provisions even prior to the JOBS Act). As discussed in Section 5.6, the magnitude of the market response for nonaccelerated filers is essentially zero, suggesting that they derived little benefit from the JOBS Act. However, caution must be exercised in interpreting this result, as there are few nonaccelerated filers in the EGC sample, and the difference between nonaccelerated filers and other EGCs is not statistically significant.

The magnitude of the positive reaction that we find for EGCs around the March 15 event is of the same order of magnitude, albeit larger than, the estimated savings in Section 404 SOX compliance costs (attributable to the internal control requirements). It is not necessarily surprising that the magnitude would be larger than can be directly attributed to SOX 404, as EGCs also benefited from other accounting-related changes, such as not being subject to audit firm rotation or auditor discussion and analysis requirements,[58] not being subject to any future rules of the PCAOB (unless the SEC explicitly subjects EGCs to them),[59] and receiving a longer transition period to comply with new audit standards.[60] There are also many aspects of the internal control requirements, such as their effects on risk-taking, employee time and effort, and litigation risk, that are difficult to quantify and may not be fully captured in existing estimates of compliance costs.

This paper represents a first attempt at the empirical analysis of the JOBS Act. There are many potential avenues for further research that may clarify some of these unresolved issues. For example, EGC status is elective for firms meeting the revenue and other criteria. It may be possible to analyze the market reactions to firms electing to be treated as EGCs to shed more light on the impact of the relaxation of disclosure and compliance obligations, as more data becomes available over time.

The effect of mandatory securities regulation on firm value has been a longstanding concern across law, economics and finance. However, it has proved challenging to find quasi-

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[58] See §104, JOBS Act 2012. The JOBS Act also relaxed compensation disclosure and analysis (CD&A) requirements by permitting an EGC to be considered a "smaller reporting company" for purposes of satisfying the executive compensation disclosure requirements of Item 402 of Regulation S-K (see §102(c), JOBS Act 2012). This in essence means EGCs will (i) not have to file a CD&A, (ii) disclose compensation only for the CEO and two other named officers, (iii) disclose compensation information for the current fiscal year only, and (iv) not have to include certain tables. This may arguably have disproportionately benefited technology firms. See COMPENSIA, *Executive Pay Disclosure Trends of Emerging Growth Companies*, THOUGHTFUL PAY ALERT, May 3, 2013. Available at: http://www.compensia.com/tpa_050313_emerging_growth.html. However, while the interaction between the EGC dummy and an indicator for technology firms is positive (suggesting a larger benefit for technology firms), it is not statistically significant.
[59] See §102, JOBS Act 2012.
[60] See §104, JOBS Act 2012.

experimental variation in the application of securities regulation, for example because securities law typically applies to all firms listed in a given jurisdiction. The JOBS Act of 2012 involved a limited degree of retroactivity that provides a rare quasi-experimental setting in which to address this question. Although this limited retroactivity applies to a relatively small number of firms, it provides an important source of evidence on the impact of the JOBS Act not just for these firms, but for all those firms that will be subject to the new regime in the future. Our results also shed light on the costs and benefits of mandatory securities regulation more generally.

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Figure 1: Conceptual Framework

V

$V - B(0)$

Outsiders' value
$= (1 - \alpha)V - (1 + \gamma)B(r) - (1 - \alpha)C(r)$

r^*

Strength of regulation (r)
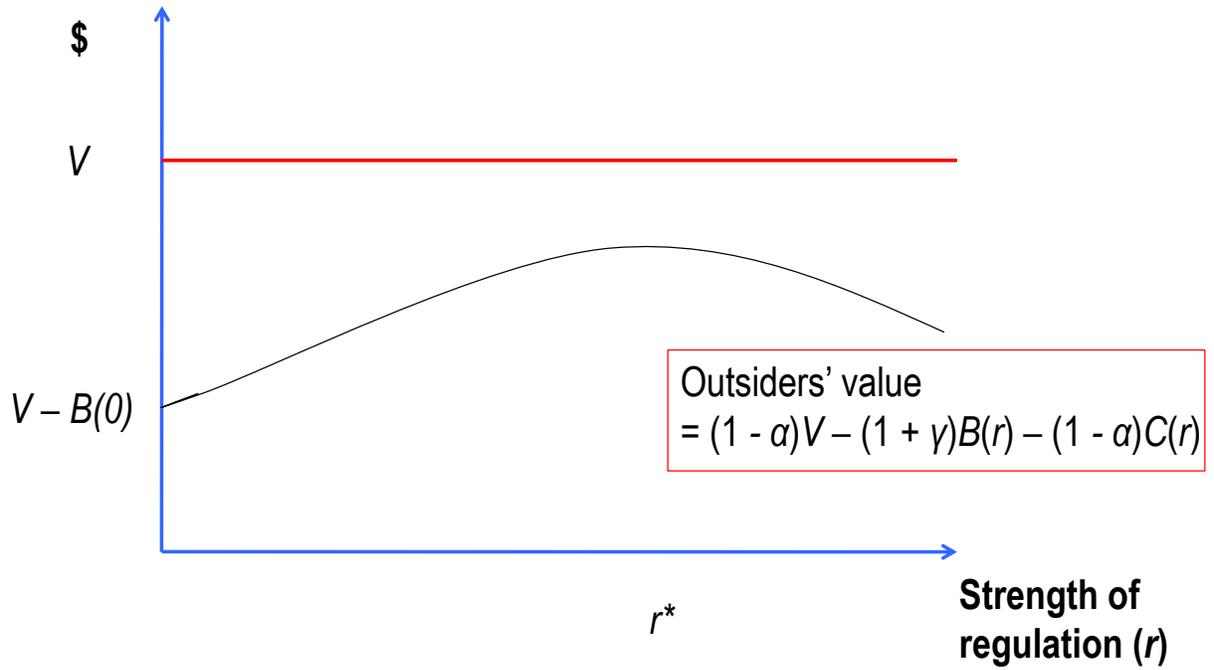
$\$$

Figure 2: Empirical Strategy

$\$$

Revenue = $1 billion

2 firms

2 firms

Control Group (33 firms)

Treatment Group (25 to 41 firms)

IPO Date

July 2011   Dec. 8, 2011   March 2012 April 5, 2012
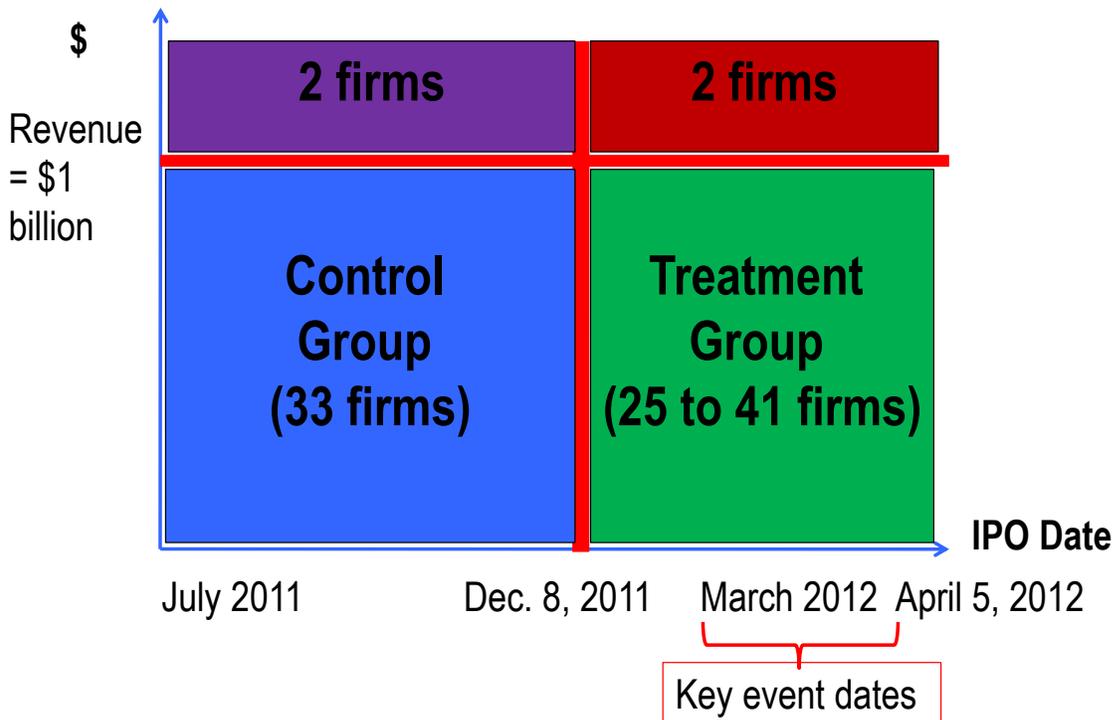
Key event dates

Table 1: Important Event Dates for the JOBS Act

Date	Event
December 8, 2011	The bill (H.R. 3606) is introduced in the House, and referred to the House Financial Services Committee.
February 16, 2012	The bill is ordered to be reported by the House Financial Services Committee (by a vote of 54-1).
March 1, 2012	The bill is reported (amended) by the House Committee on Financial Services (H. Rept. 112-406). This report includes the December 8, 2011 cutoff date for eligibility for EGC status (this appears to be the first public appearance of this cutoff date).
March 8, 2012	The bill is passed by the House by a vote of 390-23.
March 15, 2012	The measure is laid before the Senate by unanimous consent, and committed to the Senate Committee on Banking, Housing and Urban Affairs. Speech by Senate Majority Leader describing the bill as "a measure the Senate should consider expeditiously and pass in short order."
March 21, 2012	Cloture on the bill is invoked in the Senate (by a 76 – 22 vote).
March 22, 2012	The (amended) bill is passed by the Senate (by a 73-26 vote). The Senate amendment relates to the "crowdfunding" provisions of the bill, not to the EGC provisions.
March 27, 2012	The amended Senate bill is passed by the House (by a 380-41 vote).
April 5, 2012	Presidential signature; the JOBS Act becomes law.

Note: These legislative events are based on information reported on the Library of Congress THOMAS system, available at http://thomas.loc.gov, supplemented by various media reports.

Table 2: Descriptive Statistics for Control Variables

Variable	Treatment Firms Mean (Standard deviation) (Number of firms)	Control Firms Mean (Standard deviation) (Number of firms)
Revenue in the most recently completed fiscal year (typically 2011)	194.13 (231.72) (27)	182.96 (217.19) (33)
Revenue (fiscal year 2012)	278.72 (296.55) (22)	299.89 (326.31) (21)
Trading days since IPO	30.59 (17.10) (27)	121.52 (36.32) (33)
Total assets (fiscal year 2011)	413.33 (575.67) (27)	364.80 (605.00) (30)
Total assets (fiscal year 2012)	946.90 (1630.24) (23)	512.65 (723.58) (21)
Long-term debt (fiscal year 2011)	107.88 (275.55) (27)	98.75 (278.80) (30)
Long-term debt (fiscal year 2012)	364.48 (1180.00) (23)	179.74 (405.80) (21)
Earnings (fiscal year 2011)	45.51 (93.94) (27)	41.91 (86.86) (30)
Earnings (fiscal year 2012)	74.52 (134.86) (22)	72.42 (102.70) (20)
R&D (fiscal year 2011)	9.53 (12.19) (27)	5.90 (10.31) (33)
R&D (fiscal year 2012)	10.83 (16.26) (27)	4.49 (8.20) (33)
Market value (fiscal year 2012)	760.14 (701.91) (23)	832.09 (776.09) (21)
Public float (fiscal year 2012)	541.03 (1526.79) (27)	381.59 (534.09) (31)

Note: This table reports descriptive statistics for the control variables used in the regression analysis and in various robustness checks. Revenue in the most recently completed fiscal year is hand-collected from the SEC's EDGAR database, taking account of each firm's fiscal year. The number of trading days from each firm's IPO date to March 14, 2012 is calculated using CRSP data. "Public float" is the aggregate worldwide market value of the voting and non-voting common equity held by its non-affiliates), which is hand-collected from 10-K filings in the SEC's EDGAR database. Note that this is shown only for 2012, as the public float is not defined for 2011 for firms that went public in 2012. All other variables are from Compustat. Earnings represents EBITDA; R&D is defined such that missing values are set to zero. All variables (apart from the number of trading days) are reported in millions of dollars.

Table 3: Cumulative Abnormal Returns (CARs) for Key Event Windows

Event	Window (-1, +1)	Treatment Firms Mean CAR (Standard error) (Number of firms)	Control Firms Mean CAR (Standard error) (Number of firms)	Statistically significant difference?
Entire window	February 29-April 9, 2012	0.1211*** (0.0354) (25)	0.0646 (0.0495) (33)	No
House Committee report	February 29-March 2, 2012	0.0200* (0.0104) (25)	-0.0114 (0.0077) (33)	No (not robust)
House deliberation and vote	February 29-March 9, 2012	0.0181 (0.0138) (25)	-0.0027 (0.0162) (33)	No
Beginning of Senate consideration	March 14-March 16, 2012	0.0358** (0.0167) (27)	-0.0035 (0.0084) (33)	Yes
Senate deliberation and vote	March 14-March 23, 2012	0.0629*** (0.0223) (27)	0.0215 (0.0178) (33)	No
House vote on amended Senate bill	March 26-March 28, 2012	0.0216 (0.0154) (33)	-0.0092 (0.0170) (33)	No
Presidential signature	April 4-April 9, 2012	0.0043 (0.0059) (41)	-0.0056 (0.0087) (33)	No

Note: This table reports mean cumulative abnormal returns (CARs) for the various windows specified, separately for the treatment firms (which conducted IPOs after December 8, 2011, and meet the basic criterion for eligibility for emerging growth company (EGC) status of having less than $1 billion of revenues in the most recently completed fiscal year) and the control firms (which conducted IPOs from July, 2011 to December 8, 2011, and had less than $1 billion of revenues in the most recently completed fiscal year). Conventional standard errors are reported in the table, but the results are essentially identical using bootstrapped standard errors. The test of statistical significance in Column 4 uses a regression of the CAR on an indicator variable for the treatment firms.

*: significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Basic Regression Results

	Dependent variable: Cumulative Abnormal Return (CAR), March 14-16, 2012			
	Full sample	Full sample (using Fama-French CARs)	Excluding Recent IPOs	Full sample
EGC	**0.03929**	**0.03813**	**0.04946**	**0.06057**
	(0.01865)**	**(0.01841)****	**(0.02289)****	**(0.02497)****
Revenue in most recent fiscal year			-0.00001	0.00003
			(0.00003)	(0.00003)
Number of trading days since IPO			0.00017	0.00029
			(0.00024)	(0.00025)
Total assets				-0.00003
				(0.00002)
Long-term debt				0.00006
				(0.00003)*
Earnings				-0.00010
				(0.00014)
R&D expenditure				0.00127
				(0.00104)
Constant	-0.00351	0.00538	-0.02230	-0.04262
	(0.00841)	(0.00846)	(0.02416)	(0.03023)
Number of Observations	60	60	54	57
R^2	0.08	0.08	0.08	0.14

Note: This table reports the results of a series of regressions for the CAR for the March 14-16 interval (during which Senate consideration of the bill commenced). The primary variable of interest (EGC) is an indicator = 1 for firms satisfying the JOBS Act's criteria for an "emerging growth company" (notably, having revenue of less than $1 billion in the most recently completed fiscal year). Revenue in the most recently completed fiscal year is hand-collected from the SEC's EDGAR database, taking account of each firm's fiscal year. The number of trading days from each firm's IPO date to March 14, 2012 is calculated using CRSP data. All other variables are from Compustat (for 2011). Earnings represents EBITDA; R&D is defined such that missing values are set to zero. Robust standard errors are reported in parentheses.
*: significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Additional Regression Results

	Dependent variable: Cumulative Abnormal Return (CAR), March 14-16, 2012				
	Including Industry Effects (using Fama-French CARs)	Alternative Test (using "large" non-EGCs with post-Dec 8 IPOs as the control group)	Placebo Test (using "large" non-EGCs as the "treatment" group)	Placebo Test (using investment companies as the "treatment" group)	Test of differential effect for firms not subject to SOX 404
EGC	**0.05412**	**0.04340**			0.04503
	(0.02540)**	**(0.01764)****			(0.02162)**
"Large" firm with post-Dec 8 IPO			**-0.00412**		
			(0.00978)		
Investment co. with post-Dec 8 IPO				**-0.00533**	
				(0.01009)	
EGC*NAF					**-0.03779**
					(0.02736)
NAF					-0.00257
					(0.01520)
Industry effects?	Yes	No	No	No	No
Constant	0.00033	-0.00763	-0.00351	-0.00351	-0.00328
	(0.00936)	(0.00484)	(0.00851)	(0.00851)	(0.00934)
Number of Observations	59	29	35	35	60
R^2	0.45	0.02	0.0004	0.0007	0.10

Note: This table reports the results of a series of regressions for the CAR for the March 14-16 interval (during which Senate consideration of the bill commenced). In Columns 1, the primary variable of interest (EGC) is an indicator = 1 for firms satisfying the JOBS Act's criteria for an "emerging growth company" (notably, having revenue of less than $1 billion in the most recently completed fiscal year). "Large firm with post-December 8 IPO" is an indicator variable = 1 for firms with revenue exceeding the $1 billion threshold that conducted IPOs after December 8, 2011. "Investment company with post-December 8 IPO" is an indicator variable = 1 for registered investment companies (typically closed-end funds) that conducted IPOs after December 8, 2011. NAF is an indicator variable =1 for nonaccelerated filers. Robust standard errors are reported in parentheses.
*: significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Tests for Potential Alternative Explanations

	Dependent variable: Cumulative Abnormal Return (CAR), March 14-16, 2012			
	Excluding "Politically Active" EGCs	Excluding EGCs with Other Events	Including only EGCs without Analyst Coverage	Including only EGCs with Analyst Coverage
EGC	0.06864	0.05809	0.07151	0.06967
	(0.03002)**	(0.02824)**	(0.03522)**	(0.02926)**
Revenue in most	0.00002	0.00001	0.00003	0.00001
recent fiscal year	(0.00005)	(0.00003)	(0.00004)	(0.00003)
Number of trading	0.00039	0.00033	0.00036	0.00038
days since IPO	(0.00026)	(0.00025)	(0.00026)	(0.00025)
Total assets	-0.00002	-0.00003	-0.00002	-0.00010
	(0.00002)	(0.00002)	(0.00001)	(0.00006)
Long-term debt	0.00005	0.00006	0.00004	0.00020
	(0.00003)*	(0.00003)*	(0.00002)*	(0.00011)*
Earnings	-0.00010	-0.00007	-0.00011	0.00002
	(0.00015)	(0.00014)	(0.00020)	(0.00013)
R&D expenditure	0.00153	0.00064	0.00055	0.00194
	(0.00117)	(0.00091)	(0.00088)	(0.00115)
Constant	-0.05515	-0.04136	-0.04465	-0.04771
	(0.03442)	(0.02803)	(0.02875)	(0.02992)
Number of Observations	51	45	46	41
R^2	0.16	0.13	0.11	0.31

Note: This table reports the results of a series of regressions for the CAR for the March 14-16 interval, testing various potential alternative explanations. The primary variable of interest (EGC) is an indicator = 1 for firms satisfying the JOBS Act's criteria for an "emerging growth company" (notably, having revenue of less than $1 billion in the most recently completed fiscal year). Control variables are identical to those in Table 4. Robust standard errors are reported in parentheses.
*: significant at 10%; ** significant at 5%; *** significant at 1%.

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