Regulation and Initial Capital Structure: Evidence from the JOBS Act

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ABSTRACT

We examine capital structure implications of newly public firms' availing themselves of regulatory exemptions. Title I of the Jumpstart Our Business Startups (JOBS) Act provides newly public firms broad-scale regulatory relief but limits the benefits to a certain subset of firms named "Emerging Growth Companies (EGCs)." One of the EGC criteria is based on a $700 million public float threshold. We find evidence that firms appear to manipulate their public float at IPO issuance by bunching around the threshold to be eligible for the EGC status. Firms staying below the threshold are more likely to substitute public equity with debt. We further find the leverage effect persists over time, although public-float bunching attenuates.

Keywords: JOBS Act, IPOs, Public Float, Bunching, Capital Structure

1 Introduction

Understanding how firms choose their capital structures is one of the central questions in financial economics. Extensive literature attempts to answer the question and identifies a number of factors that explain variations in corporate capital structures both cross-sectionally and in time-series. Among the numerous important factors identified, the insight implying optimal capital structure has come through in the literature. In this paper, rather than attempt to test any theories of optimal capital structure, we ask how regulatory policies can lead to unintended influence on firms' moving toward the optimum capital structure. We use the Jumpstart Our Business Startups (JOBS) Act as a policy experiment to provide novel evidence that a regulatory action to benefit small firms seeking to raise public capital can affect their capital structure and that the effect can be persistent.

The JOBS Act (the Act) was signed into law in April 2012 to reduce the regulatory burdens of small firms in raising capital in United States. Title I of the Act particularly addressed concerns about the decline in small initial public offerings (IPOs) in United States and intended to reduce the imposed high compliance costs on small companies seeking to raise public equity. The Act provides broad-scale regulatory relief during the IPO process and the first five years after IPOs that includes reduced financial and executive compensation disclosures and delayed compliances with the Sarbanes-Oxley Act (SOX) and the Dodd-Frank Act requirements. IPO issuers under the Act can choose to take advantage of these provisions of the Act. Eligible issuers adopt nearly universally at least one of the Act's provisions suggesting that issuers believe the benefits of the Act to outweigh potential costs of reduced disclosure and compliance (Chaplinsky, Hanley, and Moon (2017)).

A unique empirical setting that the Act provides is that these regulatory benefits are permitted to the subset of firms that qualify as an "emerging growth companies" (EGCs). EGCs are firms with less than \$1 billion in revenues in the most recent fiscal year before their IPOs. An issuer can retain its EGC status for five years after its IPO unless its revenues exceed \$ 1 billion, it has issued more than \$1 billion non-convertible debt securities within three years, or it becomes a large accelerated filer–a firm with public float greater than \$700 million.[1] We especially utilize the last EGC-eligibility condition for public float and examine whether IPO issuers avail themselves to the regulatory benefits by purposefully setting their public float below the \$700 threshold and using the other form of external financing instead.

This empirical setting is similar to the previous studies that exploit the fact that firms manipulate their public float to be eligible for regulatory exemptions. Gao, Wu, and Zimmerman (2009), Iliev (2010), and Nondorf, Singer, and You (2012) provide evidence that firms actively avoid having their public float above \$75 million which is the cutoff point for the SOX Section 404 compliance. Chaplinsky, Hanley, and Moon (2017) find that the JOBS Act relaxed the \$75 million bright-line threshold by extending the exemptions to larger issuers. Consistent with Chaplinsky, Hanley, and Moon (2017), we also find that IPO issuers no longer purposefully set their public float below the \$75 million cutoff point. However, the Act newly introduced another bright-line public float threshold at \$700 million below which relatively larger issuers have incentives to remain. The \$700 million threshold exists before the Act as a cutoff point for a large accelerated filer, one of the Securities Exchange Commission (SEC) filer categories. Nonetheless, firms don't have enough incentives to manipulate their public float below the threshold before the Act, because the benefits of remaining below the threshold are limited to delaying their annual filing submissions to the SEC by 15 days (Alsabah (2018)). The Act

[1] Public float is the aggregate market value of the voting and non-voting common equity held by non-affiliates as of the last business day of its most recently completed second fiscal quarter.

significantly altered the expected benefits of lingering in the left side of the line by providing multiple exemption provisions and thus is attributable to financial implications that potentially differ around the threshold.

We begin our analysis by assessing whether the assignment of firms just above and below the threshold is random before and after the Act. We find robust evidence that firms seeking to raise public equity through IPOs after the Act purposefully set the value of public float below \$700 million at IPO issuance. In contrast, we don't find the similar bunching behavior of IPO issuers before the Act.

If there is a target or required amount of external financing for newly public firms, forcing themselves to raise public equity less than required will likely necessitate other forms of external financing eventually. To examine this point, we first predict the amount of public float for newly public firms after the Act using the pre-Act sample and compare the predicted value of public float with the actual public float raised. We find that the gap between the predicted and the actual is significant at approximately \$200 million on average, which translates into 25% less public float relative to what they would have raised before the Act. We next examine these firms that experience the deficit of public equity capital related to the EGC-eligibility threshold are more likely to have higher leverage. Consistent with this prediction, we find strong evidence that firms with public float deficit have significantly higher leverage. On average, a one standard deviation increase in the gap between the predicted value and the actual public float is associated with the 15 percentage-point higher leverage for newly public firms under the Act. We show that the result is not merely driven by the mechanical fact that firms raising less public equity at IPO issuance to qualify for EGC status have less total equity and thus fewer total assets in their book.

The analysis thus far is consistent with firms substituting equity with debt to quality for the regulatory benefits of the Act. We further examine whether this substitution effect is only temporary for the purpose of qualifying EGC status for up to five years after IPOs. Alti (2006) finds that IPO firms have significantly lower leverage ratios when they issue more equity in hot markets but the market-timing effect on leverage vanishes within two years. In contrast, Baker and Wurgler (2002) and Huang and Ritter (2009) find equity market timing has a long-run effect on capital structure. Consistent with the latter strand of the literature, our results show that the substitution effect is rather persistent. Firms that start with higher initial leverage for the EGC-eligibility reason at IPO issuance maintain their leverage for a longer term even after they lose the EGC eligibility. This finding is also consistent with Lemmon, Roberts, and Zender (2008) who document the importance of initial leverage for variation in capital structures.

Finally, we examine effects of the public float gap associated with EGC eligibility on corporate investment. Dambra and Gustafson (2018) show that de-burdening provisions of the Act contribute to an increase in investment and also more efficient investment by exploiting the relaxation of the $75 million cutoff point through the Act. We test for the similar prediction using the reinforcement of the $700 million cutoff point through the Act for relatively larger issuers. We find no evidence that firms raising less public equity to stay below the threshold invest less. The possible explanation for this result is that the reduction in compliance costs under the Act and the increased use of debt in substitution for public equity help avoiding possible investment distortion, particularly for larger firms with less severe financial constraints.

Our paper contributes to the extensive literature on corporate capital structure. The existing literature makes numerous important contributions to identifying determinants of capital structures. It is widely accepted that leverage increases with firm size, tangibility, growth opportunity and tax shield and decreases with profitability and probability of bankruptcy, among others. More recently identified determinants for capital structure include median industry leverage (Frank and Goyal (2009)), equity prices (Welch (2004)), equity market timing (Baker and Wurgler (2002), Alti (2006), and Huang and Ritter (2009)) and initial leverage (Lemmon, Roberts, and Zender (2008)). Our study identifies a unique mechanism in which firms in their early public years adjust their initial capital structure to be eligible to the benefits of regulatory exemptions. Furthermore, we find initial leverage ratios set initially by those regulatory-benefit incentives at IPO issuance persist for a relatively long term even after eligibility for the benefits is lost. Our findings are particularly informative for what determines the initial leverage and for the strand of literature on capital structure adjustment frequency.[2]

Lastly, our work is part of the growing literature on the JOBS Act. Among the literature that documents positive effects of the Act, Dambra, Field, and Gustafson (2015) find that the Act overall encourages more firms to go public. Dambra and Gustafson (2018) find that de-burdening provisions of the Act help reduce regulatory burdens to small public firms and make them to invest more efficiently. On the other side, focusing on changes in information environment by the Act, Chaplinsky, Hanley, and Moon (2017), Barth, Landsman, and Taylor (2017), and Agarwal, Gupta, and Israelsen (2017) find that the Act contributes to an increase in information asymmetry. Our findings provide new evidence on the tradeoff between regulatory benefits and optimal capital structure decisions and that the Act has an unintended consequence of encouraging firms in their early public years to use more debt.

The remainder of the paper is organized as follows. Section 2 describes the data and variables used in our analyses. In Section 3, we discuss the main results relating public-float bunching

[2]Most recently, for example, Welch (2004) and Welch (2013) find that firms frequently adjust their capital structure, while Leary and Roberts (2005) and Strebulaev and Whited (2013) find that big active adjustments are infrequent.

around the threshold for EGC status and quantify the amount of public float deficit associated with the public float bunching. Section 4 examines capital structure implications of public float deficit. Section 5 examines the effects of public float deficit on investment. Section 6 concludes.

2 Data and Summary Statistics

We identify a list of all U.S. IPOs from Jay Ritter's IPO data website for our sample period from 1997 to 2017.[3] Our sample period starts from 1997, as 1997 is the first year of full coverage of electronic 10-K filings in the SEC EDGAR database. We obtain stock prices and other financial information for the issuer in the IPO list from the Center for Research in Security Prices (CRSP) and the Compustat databases. We further screen foreign firms, firms with a Standard Industrial Classification (SIC) code in the range of 6000 to 6999 and 4900 to 4949 to exclude firms in the finance and regulated utility sectors, respectively, and firms whose share code is not 10 or 11 in CRSP to exclude non-ordinary common shares. We finally merge the sample with our public float data explained below. Table 1 shows the details of our data collection procedure.

[Insert Table 1 Here]

We collect public float data by electronically processing 10-K (and 10-KSB) filings from the EDGAR. We first identify the SEC filer status for each 10-K filing. All reporting companies to the SEC under the Exchange Act are classified into three categories of filers including non-accelerated, accelerated, and large accelerated filers that have different disclosure and compliance requirements. We then extract the dollar amount of estimated public float and the estimation date for each filer in a given year. We calculate the public float ratio by dividing

[3]We thank Jay Ritter for providing the list of U.S. IPOs in his website. The IPO list excludes best efforts offers, American Depository Receipts (ADRs), closed-end funds, real estate investment trusts (REIT), banks and savings and loans (S&Ls), limited partnerships, special purpose acquisition companies (SPACs), and unit offers, similar to Loughran and Ritter (2004).

the dollar amount of estimated public float by the total market value of the filer on the same day as the public float is estimated.[4] Our final sample consists of 2,662 unique IPO firms and 17,808 firm years for the period of 1997-2017. Table 2 shows the numbers of IPOs and firm year observations in our sample by year.

[Insert Table 2 Here]

Table 3 provides summary statistics of the variables used in our analyses for our primary sample with firms that have actual public float between $75 million and $2 billion and predicted public float between $200 and $1,200 million and whose public float ratio does not exceed one.[5] The average public float amount of firms in our sample is approximately $500 million. When we adjust for inflation in 1997 purchasing power dollars using the consumer price index, the average of public float amounts is $378 million. Given the average total market value of equity of firms in our sample is $1.2 billon, approximately 75.1% of their equity is held by non-affiliates. In our analysis later, we predict expected amounts of public float and note that the means of the expected and actual public float amounts are nearly identical. However, the median of the expected public float amounts is greater than that of the actual public float amounts. We also calculate public float deficit, the difference between predicted and actual public float, and its mean is approximately $11 million.

[Insert Table 3 Here]

Firms in our sample are highly levered with the mean leverage ratio of 42%. As firms in our sample are newly public firms, their market-to-book ratio is high at 2.4. Asset tangibility and profitability are on average 20% and 4.5%, respectively. In our sample, 31% of firm years have

[4] After December 15th, 2002, the SEC requires companies excluding small business issuers that file 10-KSBs to calculate and report their public float amount for the last day of their second quarter. Previously, public float is calculated and reported within 60 days of their filing date in accordance with the SEC RIN 3235-AG82.

[5] The $75 million cutoff was another bright-line threshold associated with the SOX Section 404 compliance before the Act. We exclude firm years below $75 million in our analyses to avoid any commingled effects from the $75 million threshold. We also consider samples with different (narrower) public float bandwidths around the $700 million threshold for robustness.

no reported R&D expenses in annual filings. After missing values set to zero, the mean R&D intensity of the sample is 8.5% of total assets. The average firm age is 6.9 years. CAPX and R&D investments are 7.7% and 9.3% of total sales, respectively.[6] In our regression analyses, we control for equity market conditions with the average buy and hold return of all Nasdaq-traded stocks during the previous 90 days and IPO market conditions with the hot (cold) IPO market dummy that represents the highest (lowest) quartile in the number of IPOs per month. The average Nasdaq return for our sample period from 1997 to 2017 was 3.9%. The mean number of IPOs per month is 17.4. Based on the numbers of IPOs per month, we classify hot and cold IPO markets. 58.4% and 4.1% of firm years in our sample are classified as hot and cold market periods, respectively.

3 Public Float Bunching

3.1 Density Test for Continuity

We begin our analysis by examining whether IPO firms that have public float amounts around the $700 million threshold manage their public float to stay below the threshold. We assess this possibility using the McCrary (2008) test for discontinuity of the density of public float at the threshold. For this test, we restrict our data to firms whose filer status is not a large accelerated filer in the previous year and that are less than five years old since their IPO. This allows us to focus on the incentives of firms to stay below the threshold to be eligible for EGC status and accompanying exemption benefits.[7] Because the large accelerated filer category was

[6] We exclude observations with CAPX/sales and R&D/sales greater than 100% from the sample for the investment analysis later that uses those variables.

[7] An issuer can retain its EGC status for five years after its IPO unless its revenues exceeds $1 billion, it has issued more than $1 billion non-convertible debt securities within three years, and it becomes a large accelerated filer–a firm with public float greater than $700 million.

created on December 15, 2006, we only keep firm years after the date of IPO issuers from 2006.[8]

We divide the sample into two groups of pre- and post-Act IPO firms. We define firms with their IPO dates before December 9, 2011 as the pre-Act IPO firms, because firms that go public between 11/09/2011 and 04/05/2012 (the date of JOBS Act enactment) are retroactively qualified for EGC status. Table 4 provides the details on our samples for McCrary (2008) density tests.

[Insert Table 4 Here]

We consider two different bandwidths of public float: 1) between \$200 and \$1,200 million and 2) between 0 and \$2,000 million focusing on the \$700 million threshold. The pre-Act IPO firm sample has 777 firm years with 244 IPOs with the narrow bandwidth and 1,603 firm years and 410 IPOs with the wider bandwidth. The post-Act IPO firm sample has 877 firm years with 357 IPOs and 1,705 firm years and 575 IPOs, respectively. The two samples appear to be well balanced between pre- and post-Act periods with regards to the numbers of observations. In addition, we separately create a sample for a placebo test using IPO firm years before 2006. We restrict the sample to firms that have their public float below \$700 million in the previous year as there was no large accelerated filer category before 2006. The placebo sample has 1,258 firm years and 638 IPOs with the narrow bandwidth and 4,810 firm years and 1,485 IPOs.

[Insert Figure 1 Here]

Figure 1 graphically shows the McCrary density test results for the discontinuities around the \$700 million public float threshold for the narrow bandwidth. Figure 1(a) and (b) are for pre- and post-Act IPO firm samples, respectively. In Figure 1(a), we fail to reject the null of continuity in public float for the pre-Act IPO firm sample. Before the Act, benefits of remaining below the threshold are only the possibility to delay annual filing submissions to the

[8]Accelerated filer category was created on December 15, 2002. Before December 15, 2002, all firms, except small business issuers with less than \$25 million public float, are treated equally.

SEC by 15 days. We do not find evidence that the benefits provide enough incentives for firms to purposefully manage their public float below the threshold before the Act. By contrast, in Figure 1(b), we reject the null of continuity in public float at $700 million with 5% significance. It appears that IPO firms after the Act actively set their public float below the $700 million threshold most likely to receive the benefits of EGC status. Figure 1(c) considers the analogous test for the placebo sample in which the $700 million public float threshold has no meaning as the large accelerated filer status is nonexistent. We fail to reject the null of continuity in the placebo sample.[9] This indicates that there is no fundamental difference between firms with public float below and above $700 million except the regulatory benefits that the Act permits to firms qualified for the EGC status.

Collectively, these results are consistent with Gao, Wu, and Zimmerman (2009) who find that firms manage public float to stay small below $75 million to avoid compliance with the SOX Section 404. Gao, Wu, and Zimmerman (2009) suggest that the effects of the SOX are longer-lasting as on-going manipulation of public float. We also examine how long the effects of the JOBS Act benefits on public float management remain after IPOs. To do so, we create two subsamples based on firm age after IPOs, firm years with less than three years and more than or equal to three years after IPOs. We run the McCrary density test for each sample and report the results in Figure 2.

[**Insert Figure 2 Here**]

Figure 2(a) shows that IPO firms maintain their public float amount below the threshold up to three years since IPOs. We reject the null of continuity in public float at the $700 million threshold with 5% significance. In Figure 2(b), however, we fail to reject the null of continuity for firm years after 3 years since IPOs. This also supports for our conclusion that the public

[9]In unreported results, we consider another set of falsification tests with $600 or $800 million as a placebo threshold for our sample with post-Act IPO firms. We also fail to reject the null of continuity for these tests.

float management of newly public firms is attributable to the regulatory benefits that the Act provides for EGCs as the benefits are only permitted for five years after IPOs. The incentives of firms to be eligible for EGC benefits attenuate over time, and thus their public-float bunching appears to reduce after three years. Also, we can interpret that on-going manipulation of public float may not be feasible for public firms, and therefore firms are more likely to do so extensively once at IPO issuance.

3.2 Public Float Deficit

Next, we attempt to quantify the deficit in public equity financing associated with public float bunching after the Act by estimating the amounts of counterfactual public float assuming no effect from the Act. We estimate a regression model that predicts public float amounts using only observations that fall in the non-event period before the Act. Independent variables we consider in the predictive regression model include firm size, market to book ratio, R&D expenses scaled by total assets, book leverage, asset tangibility that is net property plant and equipment divided by total assets, profitability that is net income divided by total assets, firm age since an IPO, and the three variables we use in the previous regressions to control for market conditions (Nasdaq return and cold and hot market indicators). We also include industry fixed effects. The model specification is specifically the following:

$$\begin{aligned} Public\ Float_t = \alpha + \beta_1 Size_{t-1} + \beta_2 M/B_t + \beta_3 D/A_{t-1} + \beta_4 PPE/A_{t-1} + \beta_5 EBITDA/A_{t-1} \\ + \beta_6 RDD_{t-1} + \beta_7 R\&D/A_{t-1} + \beta_8 Age_t + \beta_9 Nasdaq90_t + \beta_{10} Hot_t + \beta_{11} Cold_t + \gamma_i + \epsilon_t, \end{aligned} \tag{1}$$

where t denotes a year, and i denotes an industry.[10] We predict the expected public float amount for each IPO firm year whose public float amount is just below the \$700 threshold after the Act and take the difference between the actual public float amount and the predicted

[10] We use contemporaneous market-to-book ratios in the model specification, because lagged market prices of equity prior to IPOs are not available at IPO years.

amount (henceforth public float deficit). The idea is that these firms staying near but below the threshold are more likely to be the firms that deviate from the required amount of public float and raise purposefully less than $700 million to qualify as an EGC.

[Insert Table 5 Here]

In Panel A of Table 5, we compare the predicted and actual public float amounts and test for the significance in public float deficit. We find that firms that actually raise public float in the range of $500 to $700 million and the range of $600 to $700 million after the Act have expected public float amounts that are greater than the threshold at $798 and $877 million in Rows 1 and 2, respectively. The economic magnitude of the effect is significant, as the estimated expected public float amount is greater than the actually raised amount by approximately $200 million in Row 1. This estimated average deficit is also statistically significant at the 1% level. For firms in the narrower range of $600 to $700 million in Row 2, the effect is stronger with the average public float deficit at $220 million.

We also estimate the deficit in public float associated with the effects from the Act in a difference-in-differences setting. The regression analysis with a difference-in-differences approach mitigates the potential concern that our predictive regression model for expected public float may have omitted variables and overestimate public float amounts for the post-Act period in a systematic way. To address this concern, we define treated and control firms based on whether their expected public float is above and below the $700 million threshold, respectively. We then run a regression of the difference between the actual and predicted public float amounts on the treated and post dummies and their interaction term.[11] We expect the interaction term to be significantly positive indicating that only firms that are expected to raise more than $700 million public float purposefully raise less after the Act and have greater public float deficit.

[11] We do not include a set of control variables as in Equation (1) for this analysis, because those control variables are used in the predictive regression for the expected public float.

Panel B of Table 5 presents the results that support this prediction.

The results in Panel B of the table overall show a significant difference in public float deficit between the treated and control firms that are both in the post-Act period. This supports for the conclusion that our results are not driven by systematic overestimation of public float for the post-Act period in our prediction model. We find that public float deficit is greater for the treated firms that are expected to have more than $700 million public float in a given year. The effects are robust across the difference bandwidths in Columns 1 to 3. The differential effect between the two groups is estimated at approximately $260 million in public float deficit, for example in Columns 1 and 2, which is comparable with the univariate estimate in Panel A of Table 5.

Taken together, we find that firms purposefully stay below the public float threshold to be eligible for EGC status after the Act. A back-of-the-envelope estimation of the public float deficit for those firms is approximately $200-$250 million. Considering that these firms are newly public firms that need extensive financing, the $200-$250 million shortage in public equity financing relative to what they optimally require will have important financial implications.

4 Leverage Effects

4.1 Substitution between Equity and Debt Financing

Thus far, our evidence strongly suggests that the JOBS Act, enacted with the unusual goal of reducing regulatory burdens of newly public firms for the capital formation purpose as opposed to the SOX, also has the unintended consequence that generates equity financing deficit for firms around the bright-line threshold. In this section, we further examine implications of the regulation-attributable equity financing deficit on capital structure. We specifically examine whether firms that deviate from their required public float amount to be eligible for the deburdening benefits of the Act cover the deficit by debt financing alternatively and whether this substitution has a long-lasting effect on capital structure. In the following regression model for book leverage, we use our variable of the public float deficit estimated in the previous section as the main variable of interest:

$$\begin{aligned} Leverage_t = \alpha + \beta_1 PFD_t + \beta_2 Treated + \beta_3 Post_t + \gamma_1 Treated \times Post_t + \gamma_2 PFD_t \times Treated \\ + \gamma_3 PFD_t \times Post_t + \delta_1 PFD_t \times Treated \times Post_t + \eta X_{t-1} + \gamma_i + \gamma_t + \epsilon_t, \end{aligned} \tag{2}$$

where t denotes a year, i denotes an industry, PFD is the difference between the predicted amount of public float and the actual amount, $Treated$ is an indicator variable that is one if the predicted public float amount is greater than \$700 million and zero otherwise, $Post$ is an indicator variable for firms that go public after the Act, and X is a set of control variables used in Equation (1) except leverage.[12] We also include year fixed effects and industry fixed effects at the two-digit SIC code level. Table 6 presents the results for the bandwidth of public float between \$400 and \$1,000 million.[13]

[Insert Table 6 Here]

In Columns 1 and 2, we first examine whether the public float deficit only can predict leverage. Column 1 has no other control variables, and Column 2 has control variables.[14] We find that leverage increases with public float deficit in both columns. The economic interpretation of the coefficient estimates in the two columns is that a \$1 million increase in the public float deficit translates into a 0.01-0.02 percentage point increase in leverage. As the average public float deficit is estimated at approximately \$200 million in the previous section, this indicates

[12] We also exclude D/A_{t-1} in the predictive regression for expected public float as leverage is the dependent variable for this analysis. Results are robust to including it in the predictive regression or this analysis as a control variable.

[13] Our results go through with a different bandwidth.

[14] We present results with no other control variables first, because those control variables are also used to predict expected public float amounts and thus may create a multicollinearity issue.

that the average effect of the leverage increase is 2-4 percentage points. This effect is also statistically significant at the 1% level. The positive association between public float deficit and leverage in Columns 1 and 2 is for any firms that raise less than the expected public float regardless of the JOBS Act effect. Therefore, in Columns 3 and 4, similar to the difference-in-differences setting in Panel B of Table 5, we consider the interaction between treated and control firms and also the triple differences with public float deficit. We find that the triple interaction term in Columns 3 and 4 is positive and significant at the 5% level. This indicates that the positive relation between public float deficit and leverage is stronger for the treated firms that are expected to have more than $700 million public float in a given year after the Act. The coefficient estimates for the triple interaction term translates into a 12 percentage point increase (=0.06×$200 million) in leverage for the average firm that is expected to raise more than $700 million in public float after the Act.

4.2 Potential Concerns

One possible concern is that our findings on the positive relation between public float deficit and leverage are driven merely by a mechanical effect. The leverage ratio is calculated as value of debt divided by the sum of value of debt and equity. Therefore, if the total amount of public float decreases, the leverage ratio mechanically drops with a decrease in total assets. To address this concern, we first use book leverage instead of market leverage as the book value of assets does not exactly follow the market value of assets that includes the value of public float. Second, we directly investigate whether the total amount of assets decreases after the Act for the firms that stay below the $700 million threshold.

[Insert Figure 3 Here]

Figure 3 presents box plots of the amount of total assets for both pre- and post-Act periods.

The box plots for both periods show that there is no overall linear relation between the amounts of public float and total assets.[15] In both figures, we find the average value of total assets in Boxes 5 to 7 for firms with public float below the threshold is comparable to that in Boxes 8 to 10 for firms above the threshold. Further, we compare Boxes 7 and 8 only for a narrower bandwidth and find the results are inconsistent with the above mechanical effect. The average value of total assets in Box 7 is greater (not smaller) than that in Box 8 for the post-Act period in Figure 3(b), while the result is opposite for the pre-Act period in Figure 3(a). This evidence effectively precludes the concern that our results are driven by a mechanical effect of reduced total assets.

4.3 Leverage Persistence

Our evidence in the previous section shows that firm incentives to avail themselves of regulatory exemptions under the Act have impacts on shaping initial capital structures of newly public firms. In this section, we examine how persistent these effects are. The effects that we focus on are different from the market timing effects as in Alti (2006) because regulatory benefits under the Act are only given during the initial five years at most of an EGC's life. Analyzing the persistent effects of the Act on capital structure is more in line with the literature on initial leverage and its persistence as in Lemmon, Roberts, and Zender (2008) for example.

For this analysis, we restrict our sample to firms that are likely to set their public float below \$700 million for the purpose of taking the regulatory exemptions for EGCs. More specifically, we compare initial leverage and its persistence between the two groups of firms with their public float at IPO issuance below \$700 million in the pre- and post-Act periods. We follow Alti (2006) who also examines the persistent leverage effects and use the cumulative change

[15] Box n represents the sample that contains firms with public float between $(n-1)\times\$100$ million and $n\times\$100$ million.

in leverage which is the difference between leverage in the initial five years after an IPO and leverage before the IPO. In this analysis, the main variable of interest is *Post* which is an indicator variable for firms that go public after the Act. We include a set of control variables used in Equation (1) except replacing leverage with leverage at IPO issuance. We include industry fixed effects at the two-digit SIC code level. If the leverage effects of the Act are not temporary, we expect to find the cumulative change in leverage from the pre-IPO leverage will continue to be positive in the years after IPOs. Table 7 presents the results.

[Insert Table 7 Here]

In Columns 1 to 3, we consider three different bandwidths between \$200 and \$1200, \$400 and \$1000, and \$500 and \$900 million in public float. We find in all three columns that newly public firms with public float below the \$700 threshold after the Act do not reduce leverage relative to the pre-IPO level as much as similar firms before the Act, as the coefficient estimates for *Post* dummy are positive and statistically significant. This indicates that the substitution between public float and debt financing is not reversed shortly after IPOs and can persistently affect firm capital structure. In Columns 4 and 5, we consider falsification tests. In Column 4, we use a different sample of firms with public float in the range of \$100 to \$200 million for the same sample period in the previous three columns. In Column 5, we consider a different sample period from 1997 to 2011 before the Act with a placebo event in 2006. Firms with public float amounts far below the \$700 threshold or that go public far earlier than the enactment of the Act are less likely to have the leverage effects from the Act. In Columns 4 and 5, we do not find evidence for the effect on the cumulative change in leverage. These falsification tests alleviate potential concerns that *Post* dummy may simply capture the change in market conditions before and after the Act as we are unable to include year fixed effects together with *Post* dummy in our regressions.

In Table 8, we consider a more stringent test for leverage persistence where we investigate firms that lose their EGC status and accompanying regulatory benefits. Those firms have public float less than $700 million at IPO issuance but become to fall in the large accelerated filer status in the subsequent years. Changing the filer status from non-large accelerated filer to large accelerated filer can happen for multiple reasons such as public float and revenues exceeding $700 million and $1 billion thresholds, respectively. If the leverage effects of the Act are manifested during a few initial years since IPOs when regulatory exemptions are permitted with EGC status, we expect to find no difference in leverage between firms newly becoming large accelerated filers in the pre- and post-Act periods.

[**Insert Table 8 Here**]

In Table 8, we find that leverage ratios between the two groups are indeed different significantly. From Column 1 with a wider bandwidth to Column 3 with a narrower bandwidth, we find that firms after being newly categorized as a large accelerated filer (losing their EGC status) after the Act maintain their leverage ratio higher than firms with similar size in public float and also being newly categorized as a large accelerated filer before the Act. The estimated difference in leverage between the two groups is approximately 15-17 percentage points, which is economically significant. Considering the limitation that our sample does not include firm years long after IPOs as the Act is enacted recently in 2012, the difference is likely to decrease over time. However, the evidence in Table 8 strongly supports for the conclusion that the initial substitution effect between public float and debt financing on capital structure is persistent.

Collectively, our results are consistent with the interpretation that firms are more likely to use debt financing instead of equity financing that includes public float for the purpose of staying below the bright-line threshold for EGC status. The initial leverage set higher for this reason appears to have a long-term effect of capital structure.

5 Investment Effects

Focusing on the relaxation of the $75 million public float threshold of the Act, Dambra and Gustafson (2018) show that the Act contributes to an increase in investment for smaller issuers. In this section, we consider a similar test to Dambra and Gustafson (2018) for investment around the newly introduced $700 million threshold for relatively large issuers. If firms deviate from their optimal level of equity financing to stay below the threshold, it can create financing friction and thus inefficient investment. On the other hand, if the reduction in disclosure and compliance costs with EGC status outweighs the costs of friction in equity financing, we expect to find no effect on investment. We conduct an analogous test to Columns 2 and 4 of Table 6 by replacing the dependent variable with investment measured by capital expenditures and/or R&D expenditures scaled by sales.[16] Table 9 reports the results.

[Insert Table 9 Here]

In Columns 1 and 2, we first find that the public float deficit has negative effects on investment measured by capital expenditures. Approximately $200 million deficit in public float is associated with 2 percentage point decrease in CAPX investment. However, the coefficient estimates for the public float deficit include its effects on all firms that raise less than the expected public float amount irrespective of the effects from the Act. We thus investigate more in depth by estimating a difference-in-differences regression with the triple interaction term between public float deficit and treated and post indicators. We use the previously defined variable for the treated indicator which is one if the predicted public float amount is greater than the $700 million and zero otherwise. The post indicator is one for firms that go public after the Act and zero otherwise. We find that the triple interaction term in Column 2 is positive

[16] We exclude RDD and $R\&D/A$ in the predictive regression for expected public float as R&D investment is the dependent variable for this analysis. Results are robust to including them in the predictive regression.

but insignificant. This indicates that the reduction in CAPX investment is not associated with public float bunching around the $700 million threshold. In Columns 3 and 4, we examine R&D investment and find that the results are similar. The coefficient estimates for the public float deficit in both columns are negative and significant, indicating that approximately $200 million deficit in public float is related to a 0.6% decrease in R&D investment. However, the coefficient estimate for the triple interaction term is also insignificant in Column 4. Similarly, in Columns 5 and 6, we find that the sum of CAPX and R&D investments appears to be affected by public float deficit in general, but is not particularly associated with public float bunching around the $700 million threshold under the Act.

These results collectively support for the conclusion that firms raising less public equity to stay below the threshold are not subject to underinvestment problems. We interpret this finding that the increased use of debt in substitution for public equity in combination with the reduction in compliance costs under the Act help avoiding potential underinvestment problems. We also note that the issuers around the $700 million public float threshold are relatively large-size firms with less severe financial constraints than issuers around the $75 million threshold as in Dambra and Gustafson (2018).

6 Conclusion

The JOBS Act was widely supported by firms, entrepreneurs, investors, and policy makers as a remedy to the regulatory burdens to smaller firms that seek to raise capital in the United States. Nearly all firms that go public after the Act and are eligible for EGC status choose to adopt the status and avail themselves of the permitted regulatory exemptions under the Act. This suggests that the benefits create strong incentives for firms that go public to raise public float only up to the bright-line threshold at \$700 million regardless of their required amount of public float. In this paper, we document novel evidence of an unintended consequence of the Act that such firms raise approximately 25% (\$200 million) less public float compared to what they would have required otherwise and cover the deficit with additional debt financing.

We show that the substitution away from public float toward debt capital increases newly public firms' initial leverage by approximately 12 percentage points. We further show that the increase in leverage ratios is not just a temporary result associated with EGC status but has a long-lasting effect on capital structure consistent with the persistent initial leverage in Lemmon, Roberts, and Zender (2008). In addition to providing new evidence for persistent initial capital structure, our findings are particularly informative for what determines initial leverage. Our paper identifies a unique mechanism in which firms in their early public years adjust their initial capital structure for possible benefits of regulatory exemptions.

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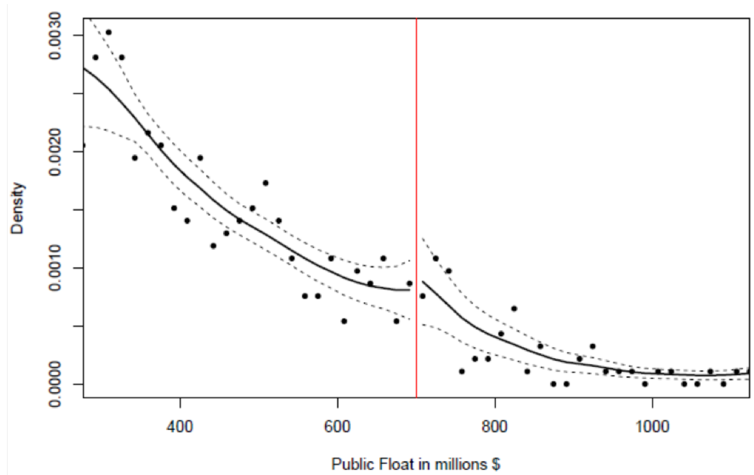
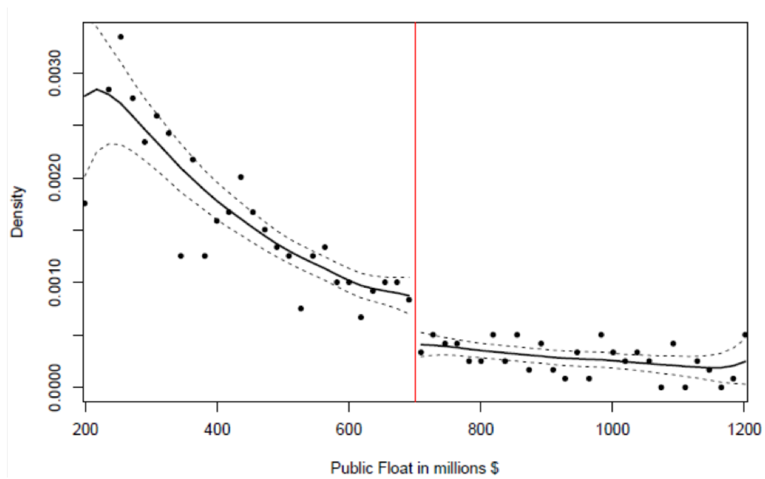
Figure 1: McCrary Density Tests

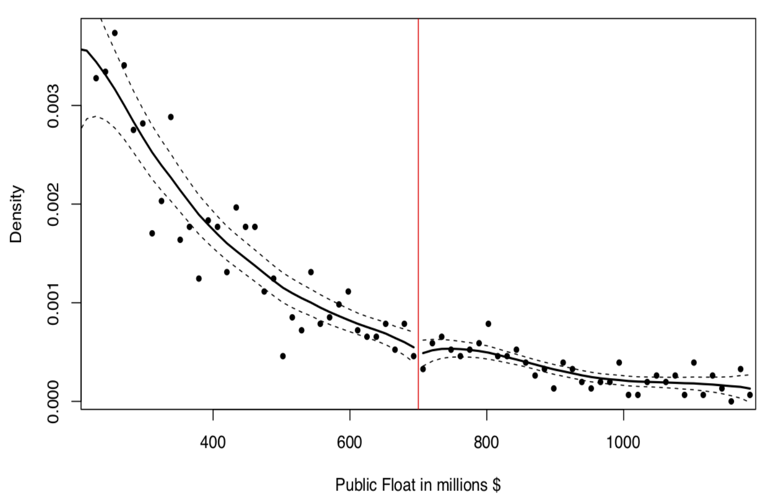
The table displays graphical representation of the McCrary (2008) test of discontinuity in public float. The pre-Act sample in (a) contains firms that go public from December 2005 and to December 2011. The post-Act sample in (b) contains firms that go public from December 2011 to December 2017. The placebo sample in (c) contains firms that go public from January 1997 to January 2005. Firms in all three samples are required to have public float between \$200 million and \$1,200 million and less than 6 years old since their IPOs.



(a) Pre-Act Sample: 2006-2011 (p-value=0.65)



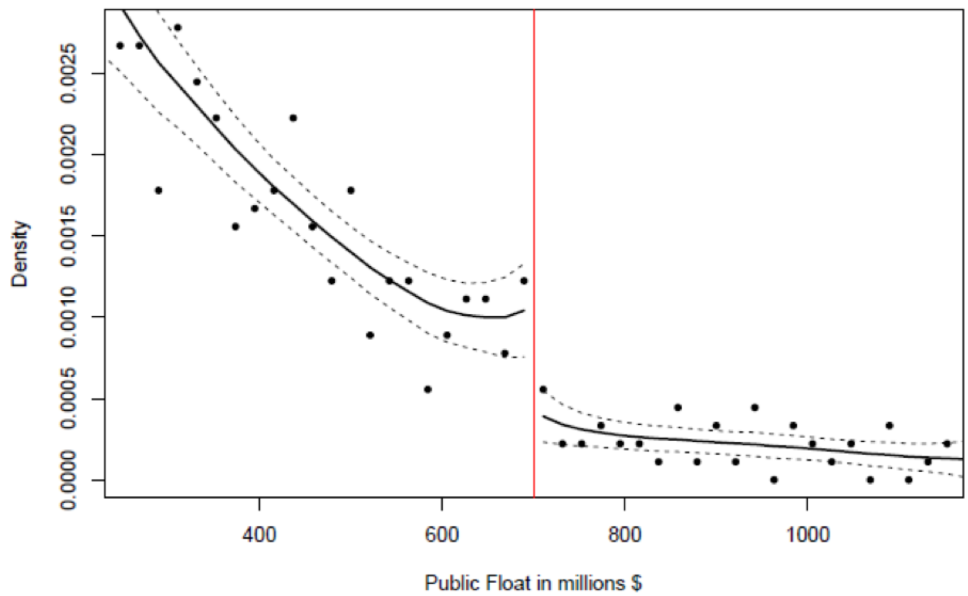
(b) Post-Act Sample: 2012-2017 (p-value=0.05)



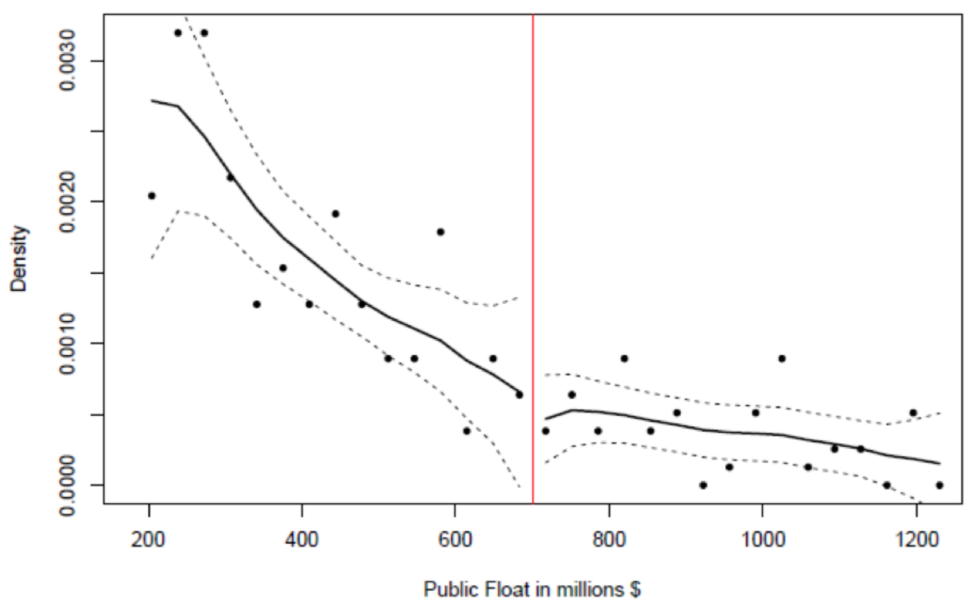
(c) Placebo Sample: 1997-2005 (p-value=0.76)

Figure 2: McCrary Density Tests by Firm Age

The table displays graphical representation of the McCrary (2008) test of discontinuity in public float for the post-Act sample that contains firms that go public from December 2011 to December 2017. Firms in the sample are required to have public float between $200 million and $1,200 million. Firms are less than 3 years old since their IPOs in (a) and with age between 3 and 5 in (b).



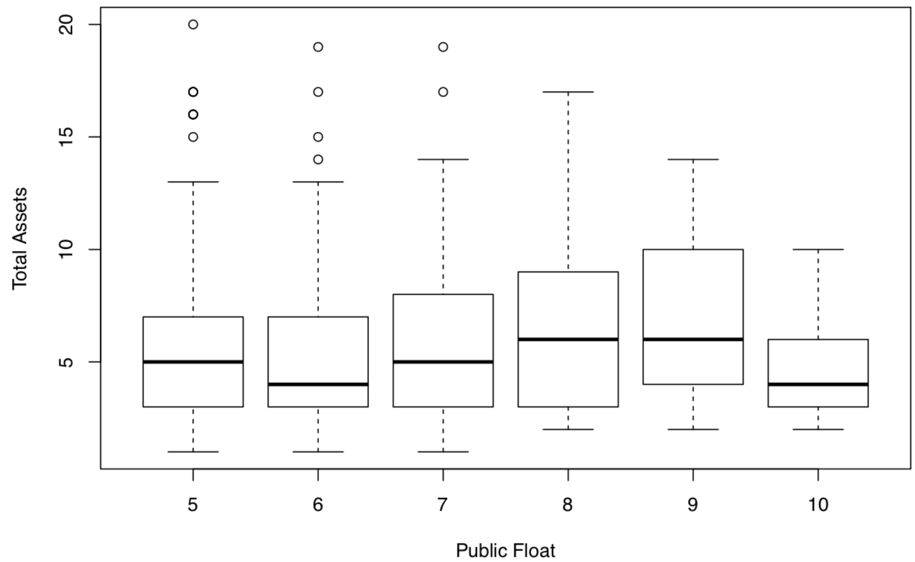
(a) Post-Act Sample with Firm Age Less than 3 Years (p-value=0.03)



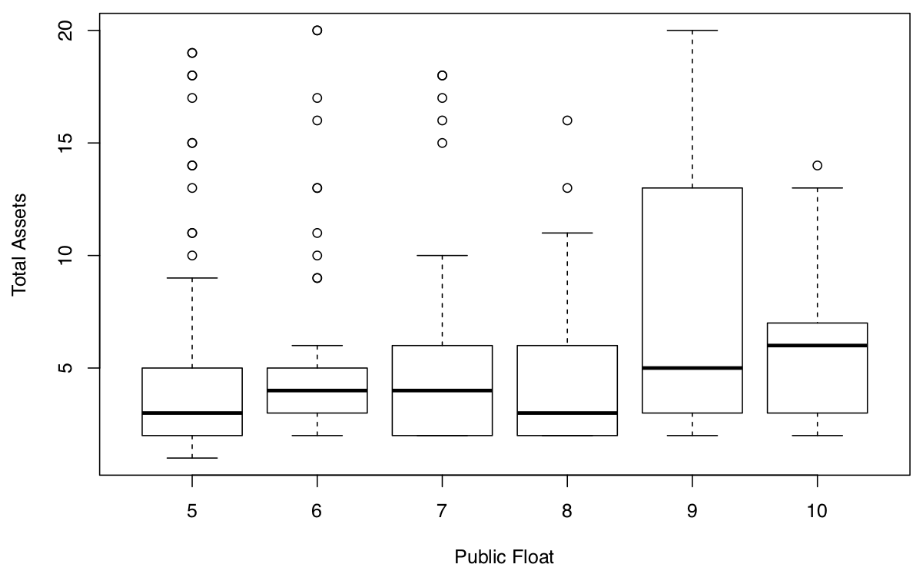
(b) Post-Act Sample with Firm Age Greater than or Equal to 3 Years (p-value=0.66)

Figure 3: Box Plots for Book Assets

The figures present box plots of the distribution of total assets for (a) the pre-Act sample and (b) the post-Act sample. Box n represents the sample that contains firms with public float between $(n-1)\times\$100$ million and $n\times\$100$ million.



(a) Pre-Act Total Assets by Public Float Bin



(b) Post-Act Total Assets by Public Float Bin

Table 1: Sample Collection Procedures

The table shows our sample collection procedure. We first identify a list of all U.S. IPOs from Jay Ritter's IPO data website for our sample period from 1997 to 2017. We then merge the IPO list with the CRSP and the Compustat databases to obtain stock prices and other financial information. We exclude foreign firms, firms with a SIC code in the range of 6000 to 6999 (financials) and 4900 to 4949 (regulated utilities), and firms whose share code is not 10 or 11 (ordinary common shares) in CRSP. Finally, we merge the sample with our public float data.

Procedure	Observations
Ritter's IPO Data (1997-2017)	4,200
Merge with the CRSP/Compustat Merged database	3,940
Exclude foreign firms	3,550
exclude firms in the financial and utility industries	2,930
Keep only ordinary common shares	2,850
Merge with the public float data from the EDGAR	2,662

Table 2: Numbers of IPOs and Firm Years over Time

The table reports the numbers of IPOs and firm year observations in our sample by year. The sample consists of firm year observations of all IPO firms for the sample period from 1997 and 2017. We discuss our IPO sample collection procedure in Table 1 in detail.

Year	Number of IPOs	Number of Firm Years
1997	365	287
1998	215	506
1999	363	780
2000	294	983
2001	56	903
2002	48	861
2003	44	799
2004	131	847
2005	114	881
2006	117	907
2007	142	961
2008	16	859
2009	36	839
2010	69	834
2011	66	771
2012	81	839
2013	129	916
2014	162	997
2015	97	1,029
2016	67	1,019
2017	50	990
Total	2,662	17,808

Table 3: Summary Statistics

The table reports summary statistics of the variables used in our analyses for the primary sample that consists of firms with actual public float between \$75 million and \$2 billion and predicted public float between \$400 million and \$1 billion. Firms in the primary sample are also required to have public float ratio that does not exceed one. The sample period is from 1997 to 2017. Public Float is the aggregate market value of the voting and non-voting common equity held by non-affiliates as of the last business day of its most recently completed second fiscal quarter. Public Float (in 1997 \$) is the public float amount adjusted for inflation in 1997 purchasing power dollars using the consumer price index. Market Value of Total Equity (in 1997 \$) is the fiscal year end stock price multiplied by the number of shares outstanding, adjusted for inflation in 1997 purchasing power dollars using the consumer price index. Public float Ratio is Public Float (in 1997 \$) divided by Market Value of Total Equity (in 1997 \$). Predicted Public Float (in 1997 \$) is estimated using the following regression model in 1997 purchasing power dollars: $Public\ Float_t = \alpha + \beta_1 Size_{t-1} + \beta_2 M/B_t + \beta_3 D/A_{t-1} + \beta_4 PPE/A_{t-1} + \beta_5 EBITDA/A_{t-1} + \beta_6 RDD_{t-1} + \beta_7 R\&D/A_{t-1} + \beta_8 Age_t + \beta_9 Nasdaq90_t + \beta_{10} Hot_t + \beta_{11} Cold_t + \epsilon_t$. Public Float Deficit is the difference between the actual public float and the predicted public float in 1997 purchasing power dollars. Leverage is book debt divided by total assets in percentage. M/B is book debt plus market value of equity divided by total assets. PPE/A is net property, plant and equipment divided by total assets in percentage. EBITDA/A is earnings before interest, taxes and amortization divided by total assets in percentage. RDD is an indicator variable that is one if R&D expenses are missing. R&D/A is R&D expenses divided by total assets in percentage. Age is the difference in years between the data date and the IPO date. CAPX/Sales and R&D/Sales are capital expenditures and R&D expenditures divided by total sales in percentage, respectively. Nasdaq90 is the average buy and hold return of all Nasdaq-traded stocks during the previous 90 days. IPOs per Month is number of IPOs in a given month using all U.S. IPOs from Jay Ritter's IPO data website for our sample period from 1997 to 2017. Hot (Cold) is an indicator variable for the highest (lowest) quartile in the number of IPOs per Month. Leverage, M/B, R&D, PPE/A, EBITDA/A, CAPX/Sales, and R&D/Sales are winsorized at the 1% level.

	Mean	Std. Dev	Min	Median	Max	Obs.
Public Float	499.789	426.770	75.067	348.448	1999.059	6599
Public Float (in 1997 \$)	377.852	319.746	49.457	267.680	1793.029	6599
Market Value of Total Equity (in 1997 \$)	1162.014	1339.461	26.366	727.773	22741.711	6453
Public Float Ratio	0.751	0.223	0.066	0.796	1	6599
Predicted Public Float (in 1997 \$)	388.954	123.697	130.929	375.384	925.399	6599
Public Float Deficit	11.102	276.509	-1554.038	84.843	818.019	6599
Leverage	41.862	26.064	4.034	37.196	123.402	6599
M/B	2.410	1.688	0.612	1.800	8.574	6599
PPE/A	20.011	21.685	0.114	10.903	89.243	6599
EBITDA/A	4.466	21.946	-119.014	9.528	43.108	6599
RDD	0.309	0.462	0	0	1	6599
R&D/A	8.527	14.033	0	2.020	86.238	6599
Age	6.899	4.714	1	6	21	6599
CAPX/Sales	7.704	12.053	0.223	3.777	99.284	6374
R&D/Sales	9.287	14.867	0	1.269	99.913	6144
Nasdaq90	3.922	16.124	-41.427	3.029	68.453	6599
IPOs per Month	17.425	13.700	0	14	70	240
Hot	0.584	0.493	0	1	1	6599
Cold	0.041	0.198	0	0	1	6599

Table 4: McCrary Density Test Samples

The table shows (1) the numbers of IPOs and firm year observations in the samples used for the McCrary (2008) test of discontinuity in public float in Figure 1. We also consider alternative samples in (2) for robustness.

	(1) 200≤Public Float≤1200	
	Number of IPOs	Number of Firm Years
Pre-Act Sample: 2006-2011	244	777
Post-Act Sample: 2012-2017	357	877
Placebo Sample: 1997-2005	638	1,258
	(2) 0≤Public Float≤2000	
	Number of IPOs	Number of Firm Years
Pre-Act Sample: 2006-2011	410	1,603
Post-Act Sample: 2012-2017	575	1,705
Placebo Sample: 1997-2005	1,485	4,810

Table 5: Public Float Deficit

In Panel A, we estimate the following predictive regression model for expected public float using the pre-Act sample: $Public\ Float_t = \alpha + \beta_1 Size_{t-1} + \beta_2 M/B_t + \beta_3 D/A_{t-1} + \beta_4 PPE/A_{t-1} + \beta_5 EBITDA/A_{t-1} + \beta_6 RDD_{t-1} + \beta_7 R\&D/A_{t-1} + \beta_8 Age_t + \beta_9 Nasdaq90_t + \beta_{10} Hot_t + \beta_{11} Cold_t + \epsilon_t$. Variable definitions are in Table 3. The means of predicted and actual public float amounts are reported for firms with actual public float (1) between \$500 and \$700 and (2) between \$600 and \$700 after the Act. Panel B reports the difference-in-differences regressions for Public Float Deficit, the difference between the predicted and actual public float amounts. Treated is an indicator variable that is one if the predicted public float amount is greater than \$700 million and zero otherwise. Post is an indicator variable for firms that go public after the Act. Year fixed effects and industry fixed effects at the two-digit SIC code level are included. Standard errors in parenthesis are clustered by firm.

Panel A: Mean Difference

	Public Float				
Bandwidth	Predicted Mean	Actual Mean	Mean Difference	t-stat	Observations
(1) 500≤Public Float≤700	798.06	600.35	197.71	20.55	102
(2) 600≤Public Float≤700	876.6	655.3	221.30	18.23	47

Panel B: Difference-In-Differences

	Public Float Deficit		
	(1)	(2)	(3)
	200≤Public Float≤1200	400≤Public Float≤1000	500≤Public Float≤900
Treated × Post	265.68***	262.38***	114.27*
	(55.69)	(61.30)	(68.78)
Treated	-162.71***	-143.12***	-100.89*
	(46.31)	(49.07)	(53.31)
Post	42.44	21.42	63.65
	(28.32)	(35.56)	(48.21)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	3,676	2,063	1,039
Adjusted R-squared	0.06	0.07	0.12

Table 6: Public Float Deficit and Leverage

The table reports results from the regressions of leverage on public float deficit for the public float bandwidth between \$400 to \$1,000 million. Public Float Deficit is the difference between the predicted and actual public float amounts. The predicted public float amount is estimated from the following predictive regression model using the pre-Act sample: $Public\ Float_t = \alpha + \beta_1 Size_{t-1} + \beta_2 M/B_t + \beta_4 PPE/A_{t-1} + \beta_5 EBITDA/A_{t-1} + \beta_6 RDD_{t-1} + \beta_7 R\&D/A_{t-1} + \beta_8 Age_t + \beta_9 Nasdaq90_t + \beta_{10} Hot_t + \beta_{11} Cold_t + \epsilon_t$. Treated is an indicator variable that is one if the predicted public float amount is greater than \$700 million and zero otherwise. Post is an indicator variable for firms that go public after the Act. Control variables in (2) and (4) include M/B, RDD, R&D/A, PPE/A, EBITDA/A, Nasdaq90, Hot, and Cold and are lagged for one year. Variable definitions are in Table 3. Year fixed effects and industry fixed effects at the two-digit SIC code level are included. Standard errors are clustered by firm.

	Leverage			
	(1)	(2)	(3)	(4)
PFD	0.01***	0.02***	0.01***	0.02***
	(0.002)	(0.002)	(0.002)	(0.002)
PFD × Treated × Post			0.06**	0.05**
			(0.03)	(0.02)
Treated			10.65***	-4.41***
			(1.75)	(1.71)
Post	-0.51	3.78	0.55	2.78
	(2.98)	(2.73)	(3.09)	(2.89)
Treated × Post			-10.10*	-3.61
			(5.20)	(4.97)
PFD × Treated			0.003	-0.001
			(0.004)	(0.003)
PFD × Post			0.004	-0.003
			(0.01)	(0.01)
Controls		Yes		Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	4,618	4,618	4,618	4,618
Adjusted R-squared	0.18	0.35	0.20	0.35

Table 7: Persistent Effect on Leverage

The table reports results from the regressions of the cumulative change in leverage, the difference between leverage in the initial five years after an IPO and leverage before the IPO. Post is an indicator variable for firms that go public after the Act. We restrict the sample for this analysis to IPO firms that have public float in the range of \$200 to \$700 at the first year since their IPO. Three different bandwidths are considered: (1) 200≤Public Float≤1200, (2) 400≤Public Float≤1000, and (3) 500≤Public Float≤900. We consider falsification tests with the bandwidth of (4) 100≤Public Float≤200 which is far below the \$700 million threshold and the bandwidth of (5) 500≤Public Float≤900 for the alternative sample period from 1997 to 2011 before the Act with a placebo event in 2006. Variable definitions are in Table 3. Industry fixed effects at the two-digit SIC code level are included. Standard errors in parenthesis are clustered by firm.

	Cumulative Difference in Leverage				
	(1)	(2)	(3)	(4)	(5)
				Placebo Tests	
	200≤PF≤1200	400≤PF≤1000	500≤PF≤900	100≤PF≤200	500≤PF≤900
Post	9.84***	6.67**	11.75***	4.23	0.59
	(2.04)	(3.23)	(3.70)	(4.43)	(3.30)
M/B	-0.50	-0.73	-1.25	-0.21	-1.94***
	(0.37)	(0.52)	(0.77)	(0.56)	(0.48)
D/A at IPO	-0.99***	-0.99***	-1.00***	-1.01***	-0.98***
	(0.01)	(0.01)	(0.01)	(0.004)	(0.01)
PPE/A	0.16***	0.08	0.15	0.17**	0.33***
	(0.06)	(0.10)	(0.11)	(0.08)	(0.11)
EBITDA/A	0.04***	0.05***	0.05***	-0.06	0.07***
	(0.01)	(0.02)	(0.02)	(0.05)	(0.01)
RDD	9.64***	14.46***	20.48***	2.86	14.95***
	(2.53)	(3.71)	(4.71)	(2.96)	(4.83)
R&D/A	-0.07	0.07	-0.04	-0.05	-0.33**
	(0.08)	(0.17)	(0.21)	(0.10)	(0.16)
Age	0.78	-0.16	0.94	1.05***	0.04
	(0.58)	(0.93)	(1.25)	(0.27)	(1.22)
Nasdaq90	-0.04	-0.10*	-0.02	-0.01	0.02
	(0.03)	(0.05)	(0.07)	(0.04)	(0.07)
Hot	-4.23***	-1.70	-5.04	-3.46	-4.17
	(1.61)	(2.38)	(3.16)	(2.44)	(3.34)
Cold	9.80***	9.30**	7.74*	1.98	10.20**
	(3.06)	(3.68)	(4.68)	(4.77)	(5.08)
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	1,160	579	350	657	254
Adjusted R-squared	0.98	0.98	0.98	0.98	0.97

Table 8: Losing EGC Status and Leverage Persistence

The table reports results from the regressions of leverage on Post for firms that lose their EGC status and accompanying regulatory benefits. Those firms have public float less than $700 million at IPO issuance but become to fall in the large accelerated filer status in the subsequent years. Post is an indicator variable for firms that go public after the Act. Three different bandwidths are considered: (1) 200≤Public Float≤1200, (2) 400≤Public Float≤1000, and (3) 500≤Public Float≤900. Variable definitions are in Table 3. Industry fixed effects at the two-digit SIC code level are included. Standard errors in parenthesis are clustered by firm.

	Leverage		
	(1)	(2)	(3)
	200≤PF≤1200	400≤PF≤1000	500≤PF≤900
Post	15.06***	16.90***	17.84***
	(4.43)	(5.58)	(6.43)
M/B	0.11	0.19	0.38
	(0.57)	(0.61)	(0.69)
PPE/A	0.14	0.21	0.14
	(0.14)	(0.17)	(0.20)
EBITDA/A	0.15	0.19	0.31**
	(0.12)	(0.13)	(0.15)
RDD	16.00***	14.41**	18.41***
	(5.23)	(5.65)	(6.42)
R&D/A	-0.06	-0.09	-0.05
	(0.26)	(0.29)	(0.39)
Age	-0.27	0.57	1.29
	(1.46)	(1.74)	(2.19)
Nasdaq90	0.09	0.13	0.12
	(0.08)	(0.09)	(0.11)
Hot	-0.44	-4.08	-6.54
	(3.87)	(4.44)	(5.12)
Cold	15.56***	12.07**	10.43
	(5.32)	(6.06)	(6.68)
Industry FE	Yes	Yes	Yes
Observations	248	202	162
Adjusted R2-squared	0.24	0.25	0.28

Table 9: Public Float Deficit and Investment

The table reports results from the regressions of measures of investment on public float deficit. We consider capital expenditures, R&D expenditures, or the sum of capital and R&D expenditures scaled by sales as a measure of investment. We exclude observations with CAPX/sales and R&D/sales greater than 100% from the sample for this analysis. Public Float Deficit is the difference between the predicted and actual public float amounts. The predicted public float amount is estimated from the following predictive regression model using the pre-Act sample: $Public\ Float_t = \alpha + \beta_1 Size_{t-1} + \beta_2 M/B_t + \beta_3 D/A_{t-1} + \beta_4 PPE/A_{t-1} + \beta_5 EBITDA/A_{t-1} + \beta_8 Age_t + \beta_9 Nasdaq90_t + \beta_{10} Hot_t + \beta_{11} Cold_t + \epsilon_t$. Treated is an indicator variable that is one if the predicted public float amount is greater than \$700 million and zero otherwise. Post is an indicator variable for firms that go public after the Act. Control variables in (2), (4), and (6) include M/B, RDD, R&D/A, PPE/A, EBITDA/A, Nasdaq90, Hot, and Cold and are lagged for one year. Variable definitions are in Table 3. Year fixed effects and industry fixed effects at the two-digit SIC code level are included. Standard errors are clustered by firm.

	CAPX/Sales		R&D/Sales		(CAPX+R&D)/Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
PFD	-0.01***	-0.01***	-0.003***	-0.004***	-0.01***	-0.01***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
PFD × Treated × Post		0.004		0.002		-0.003
		(0.01)		(0.01)		(0.01)
Treated		-0.13		-0.75		-0.08
		(0.66)		(0.72)		(0.82)
Post	2.66***	3.30***	1.69	2.14	4.13***	5.20***
	(0.91)	(1.10)	(1.12)	(1.33)	(1.34)	(1.56)
PFD × Treated		-0.001		0.002		0.002
		(0.002)		(0.001)		(0.002)
PFD × Post		0.003		0.01		0.01
		(0.01)		(0.01)		(0.01)
Treated × Post		-4.06***		-4.71***		-7.65***
		(1.55)		(1.63)		(1.97)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,438	4,438	4,362	4,362	4,272	4,272
Adjusted R-squared	0.33	0.33	0.52	0.52	0.46	0.46