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Essays on IPO-Firm Earnings Management

Scott N. Bronson
University of Tennessee - Knoxville

To the Graduate Council:

I am submitting herewith a dissertation written by Scott N. Bronson entitled "Essays on IPO-Firm Earnings Management." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Business Administration.

Joseph V. Carcello, Major Professor

We have read this dissertation and recommend its acceptance:

Donald J. Bruce, Terry L. Neal, Tracie Woidtke

Accepted for the Council:

Carolyn R. Hodges

Vice Provost and Dean of the Graduate School

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Acceptance for the Council:

Anne Mayhew
Vice Chancellor and Dean of
Graduate Studies

(Original signatures are on file with official student records.)

ESSAYS ON IPO-FIRM EARNINGS MANAGEMENT

A Dissertation
Presented for the
Doctor of Philosophy Degree
The University of Tennessee, Knoxville

Scott N. Bronson
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DEDICATION

This dissertation is dedicated to the memory of my mother, Ilene Bronson, who inspired me to pursue my Ph.D.

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ABSTRACT

PART 1

This paper provides evidence on the timing of earnings management behavior for initial public offering (IPO) firms in the annual periods surrounding the offering. It also examines whether this behavior is related to CEO and CFO trading after the offering. Using discretionary accruals as my proxy for earnings management, I find that, for firms that file a new 10-K *before* the trading restrictions provided in underwriter lockup agreements end, average IPO-firm discretionary accruals are significantly positive in the first 10-K filed after the offering, and that these discretionary accruals are significantly larger than those in the offering prospectus. I also find a positive relation between CEO and CFO trading activity and discretionary accruals for the same group of companies. Taken together, the results suggest that earnings management behavior is more prevalent in the first 10-K filed than in the offering prospectus, that it is concentrated in the firms that file this 10-K before their lockup period expires, and that it is positively related to CEO and CFO trading after the offering.

PART 2

This paper examines whether earnings management behavior has decreased in the period following the passage of the Sarbanes-Oxley Act of 2002 (SOX) for IPO firms. It also explores how any changes I observe for IPOs relate to any changes that have occurred for the broader set of public companies. I find that IPO firms have experienced a significant decrease in earnings management after the passage of SOX. The results also provide evidence that this decrease is driven by the smallest public companies. While pre-SOX discretionary accruals for IPO firms are larger than those for non-IPO firms, I find that the post-SOX decrease in discretionary accruals results in the level of IPO-firm discretionary accruals becoming indistinguishable from that of non-IPO firms. Finally, the evidence suggests that the characteristics of post-SOX offerings are different from those of pre-SOX offerings, and that the decrease in discretionary accruals in the post-SOX period remains after controlling for these changes.

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GENERAL INTRODUCTION

GENERAL INTRODUCTION

Previous research documents that managers record income-increasing adjustments to earnings around the time that companies go public. Some papers document this earnings management behavior in the prospectus financial statements before the offering, while other studies document this behavior in the first financial statements filed after the offering. Managing earnings before the initial public offering (IPO) might lead to the company receiving higher proceeds from the offering than the company would have received without managing earnings.

Earnings management in the period following the IPO might occur for a number of reasons. One such motive is that insiders typically do not sell their shares in the offering. Instead, they enter into agreements with underwriters in which the insiders commit to holding their shares at least until a specified time following the offering. Increasing earnings in the financial statements publicly available when insiders begin to sell their interests could have a positive impact on the share price at the time they sell, thus increasing the proceeds they personally receive from these transactions.

In Part 1 of this dissertation, I examine IPO-firm earnings management behavior around the time of the offering. I hypothesize that one of the above motivations will override the other and, as a result, I will be able to detect significant differences in earnings management in one of the two periods surrounding the offering. I also test whether insider trading after the offering is related to income increasing earnings management in the annual financial statements available at the time the share sales occur.

In Part 2 of this dissertation, I examine whether recent regulations have affected IPO-firm earnings management. The Securities Act of 1933 (the ’33 Act) imposes stiff penalties for wrongdoing in a securities offering setting. In spite of these consequences, previous research suggests that earnings management occurs around these offerings. On July 30, 2002, the Sarbanes-Oxley Act of 2002 (SOX) was signed into law. While SOX does not directly affect the ’33 Act provisions, it does increase the penalties for certain corporate malfeasance for all companies. Because the specific motivations discussed above exist in the IPO setting, it is important that we understand the effects of increased potential liability on firms with these motivations. Thus, Part 2 explores whether I observe a change in IPO-firm earnings management in the post-SOX regulatory environment. The study also examines whether IPO companies respond the same as or differently from non-IPO companies.

PART 1: LOCKUPS, INSIDER SALES, AND THE TIMING OF IPO-FIRM EARNINGS MANAGEMENT

1. INTRODUCTION

An initial public offering (IPO) presents the management of a private company with, among other things, the opportunity to finance current operations and future growth. This process also provides the owners of these firms with a market in which they can sell their ownership interests. Obviously, it is in the best interests of these parties to maximize the value they receive for the shares sold in the offering. On the other hand, potential investors do not possess the same information set as the managers and owners, and these investors do not want to overpay for the shares they purchase. To help bridge this information gap, the Securities Act of 1933 (the ’33 Act) generally requires companies to prepare a registration statement that they file with the SEC and to disseminate a preliminary prospectus (commonly referred to as a “red herring”) to potential investors to aid the investors in making an investment decision with respect to the offering. These documents, however, do not eliminate the information advantage that company insiders possess. More importantly, “obvious entrepreneurial incentives for misrepresentation prevent outsiders from believing the unsupported claims of entrepreneurs” in the offering (Downes and Heinkel 1982, 1).

Empirical evidence suggests that IPO companies might act in a manner that is consistent with this incentive. Friedlan (1994); DuCharme, Malatesta, and Sefcik (2001); DuCharme, Malatesta, and Sefcik (2004); Teoh, Welch, and Wong (1998); and Teoh, Wong, and Rao (1998) document income-increasing discretionary accruals around the time of IPOs. These accruals have been positively associated with the offering proceeds (DuCharme, Malatesta, and Sefcik 2001), negatively associated with post-IPO market performance (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998), negatively associated with post-IPO operating performance (Teoh, Wong, and Rao 1998), and positively associated with the probability of shareholder lawsuits and the settlement of these lawsuits (DuCharme, Malatesta, and Sefcik 2004).

For IPOs before 1998, Standard & Poor's did not generally capture pre-IPO financial data from the prospectus in its Compustat database.[1] As a result, the authors of previous large-sample earnings management studies in the IPO context use financial statement data from the first 10-K filed after the IPO (referred to as the IPO-year financial statements) to compute discretionary accruals.[2,3] The argument for the reasonableness of using IPO-year discretionary accruals is that the incentive to manage earnings is likely to remain for several months following the offering. These incentives include pressure to meet verbal earnings projections communicated during road shows, pressure from underwriters to meet their analysts' earnings projections, and the expiration of underwriter lockup agreements (Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). While the incentives to meet earnings projections and analyst forecasts are likely to persist into the future, the lockup typically expires 180 days after the offering.

[1] Through discussions with Standard & Poor's representatives, I confirmed that they began to backfill pre-IPO data during 1998 as long as the pre-IPO period contains at least nine months of operations.

[2] One exception to this statement is Venkataraman, Weber, and Willenborg (2005) who examine IPO-firm discretionary accruals in both the pre- and post-IPO periods.

[3] DuCharme, Malatesta, and Sefcik (2004); Teoh, Welch, and Wong (1998); and Teoh, Wong, and Rao (1998) use IPO-year financial data because the use of the financial statements that were available at the time of the offering would have required them to hand collect data from prospectuses for sample sizes of more than 3,500, 1,600, and 1,600 companies, respectively. Contrast these sample sizes with those of DuCharme, Malatesta, and Sefcik (2001) and Friedlan (1994) who use pre-IPO financial data that they hand collect from prospectuses for 171 and 155 companies, respectively.

Underwriter lockups are agreements between the lead underwriter and company insiders. While they are not required by the securities laws, these agreements restrict insiders from selling their shares until a specified point in time. Upon lockup expiration, insiders are typically free to begin trading the shares they own in the company subject to certain volume restrictions. As a result, the earnings management effects of lockups (i.e., recording positive accruals in the period before insiders plan to sell their shares) are not likely to remain long after the lockups expire. To the extent that managers plan to sell stock after the lockup period ends, they can use accounting discretion to boost earnings until they liquidate their shares. Once the insiders sell the shares they wish to sell, this lockup-related, insider selling motivation for increasing earnings would disappear.

While managers have the incentive to increase earnings right before they sell shares, another possibility is that managers wish to increase the offering proceeds to the company, which would provide more cash to finance its business. Increased pre-IPO earnings might help the company support a higher offering price (DuCharme, Malatesta, and Sefcik 2001; Ritter 1984). However, the increased liability exposure for IPO firms and their auditors under the '33 Act might counteract these incentives (Venkataraman, Weber, and Willenborg 2005).

Thus, the timing of the incentive to manage earnings in the offering is somewhat ambiguous. On one hand, the insider selling argument suggests that managers would use accounting discretion in the financial statements that would be publicly available at the time they plan to sell their shares. These sales typically do not occur until after the lockup agreement expires sometime after the offering. On the other hand, increasing the proceeds to the company in the offering would require increasing earnings in the financial statements available in the prospectus, but the increased liability potential for these firms and their auditors might constrain this behavior in the pre-IPO period.

In this study, I examine earnings management behavior around the time of IPOs to explore whether, on average, IPO-firm managers use larger positive earnings adjustments (1) to boost offering proceeds or (2) to increase the amount they receive from selling shares soon after the offering. I examine this issue by comparing pre- and post-IPO discretionary accruals for different groups of companies and different time periods depending on whether the company's lockup period ends before or after the company files its first 10-K. The results suggest that the companies that file a 10-K *before* the lockup period expires exhibit more earnings management behavior in those 10-K financial statements as compared to the prospectus financial statements. This suggests that managers increase discretionary accruals in the 10-K financial statements publicly available when they anticipate selling their shares after the lockup expires.

To test this assertion, I examine the relation between post-lockup IPO-firm insider trades and discretionary accruals in the annual financial statements from the period before the trading is allowed to occur. The trading analyses support this claim; that is, I observe a significantly positive relation between the proportion of CEO and CFO shares traded following the lockup period expiration and discretionary accruals, and this relation is limited to the firms that file a new 10-K *before* the lockup period expires. Taken together, the results suggest that earnings management behavior is more prevalent in the first 10-K filed than in the offering prospectus, that it is concentrated in the firms that file this 10-K *before* their lockup period expires, and that it is related to insider trading. The results of this study help to further develop our understanding of the incentives to manage earnings around the time of IPOs, and this evidence might be of interest to regulators and IPO investors as it can help these parties to identify the specific period around the offering in which IPO-firm managers' attempts to increase earnings are most likely to have affected the financial statements.

The rest of this paper is organized as follows. Section 2 provides background and develops the hypotheses. Section 3 discusses the research design. I provide the results of the main analyses in Section 4, and I test the sensitivity of these results in Section 5. Section 6 concludes with a summary.

2. BACKGROUND AND HYPOTHESES

Earnings Management to Increase the Offering Price

Typically, the primary motivation for going public is for the company to raise capital and to create a market in which its owners can liquidate their holdings at a later date (Ritter and Welch 2002). Given that no public market exists prior to the offering, an offering price must be determined before the company goes public. The price for the shares issued in the offering is determined through negotiations between the company and its underwriters. Empirical evidence suggests that earnings are a significant determinant of initial IPO value (DuCharme, Malatesta, and Sefcik 2001; Ritter 1984). Thus, the issuer selling securities has a strong financial incentive to increase pre-issuance earnings and, thereby, maximize its share price at the time of issuance.

The discretion available to managers under U.S. GAAP offers them the ability to decide between relatively conservative or aggressive applications of accounting principles. Furthermore, for fiscal years that began prior to December 15, 2005, APB 20 provided accountants for IPO firms with a unique opportunity to retroactively change accounting methods without the normal reporting requirements. Instead of requiring companies to report the cumulative effect of a change in accounting principles as a separate item in the calculation of net income, APB 20 allowed IPO-firm managers to change accounting principles by simply retroactively restating all pre-issuance financial statements (APB 1971, paragraph 29).[4] As a result, IPO-firm managers not only have the incentive to make positive adjustments to earnings in order to maximize the share issuance price, but they also had the ability to do so because of the unique IPO accounting rules that do not require the normal disclosure of a change in accounting methods.

Neill and Pourciau (1995) study the association between IPO proceeds and accounting choice and find evidence that firms receiving higher offering proceeds choose income-increasing inventory costing and depreciation methods. Note that these decisions are not necessarily problematic because the managers are choosing from among GAAP accounting methods.

Other studies examine the prevalence of earnings management behavior around the time of the offering by estimating discretionary accruals. Using hand-collected data from IPO prospectuses, Friedlan (1994) finds income-increasing discretionary accruals in the financial statements issued closest to but before the offering date. His results provide

[4] Statement of Financial Accounting Standards No. 154, Accounting Changes and Error Corrections, supersedes APB Opinion 20 for accounting changes and error corrections that occur in fiscal years beginning after December 15, 2005. This new standard does not include the exemption allowing offering firms to retroactively restate prior periods without the normal reporting requirements of a change in accounting principle.

evidence that managers exercise discretion in order to report a larger income number around the time of the IPO.

In order for inflated earnings to affect the offering price, however, investors must be unable to detect that manipulation has occurred. Identification of these positive adjustments would enable investors to reverse the excessive accruals and to more accurately value the shares. DuCharme, Malatesta, and Sefcik (2001) provide evidence that, after controlling for cash flows from operations, pre-IPO accruals (both discretionary and non-discretionary) are positively associated with the initial value of the IPO.[5] These results suggest that total accounting earnings have incremental explanatory power above cash flows in an IPO-valuation setting.

While incentives exist for management to increase the offering proceeds by recording positive accruals in the pre-IPO financial statements, the increased liability exposure for IPO firms and their auditors under the ’33 Act might counteract these incentives. Consistent with this argument, Venkataraman, Weber, and Willenborg (2005) document significantly negative pre-IPO discretionary accruals, on average, which they attribute to improved audit quality under the ’33 Act litigation regime.

Underwriter Lockup Agreements

While lockup agreements are not required by law, they are common in the IPO setting. These agreements, which typically have a 180-day term, are between lead underwriters and IPO insiders who agree to refrain from selling their shares until the lockup expires. Underwriters require lockup agreements to ensure that management

[5] Similar to Friedlan (1994), DuCharme, Malatesta, and Sefcik (2001) hand collect financial data from IPO prospectuses for their sample.

continues to have the same ownership interest in the company in the months immediately following the offering and also to help support the price of the IPO. Because the agreement is between underwriters and insiders, the underwriters have the freedom to release shares from the lockup at their discretion. Companies disclose the terms of the lockup provision in the registration statement and in the prospectus.

It is not uncommon for lockups to cover a substantial proportion of the post-IPO shares held by the public, and this proportion is frequently greater than 100 percent of the publicly traded shares (Bradley et al. 2001). As a result, the expiration of the lockup period followed by insider sales can have a large impact on the number of publicly traded shares for newly-public companies. After the unlock date, however, insiders are limited in the number of shares they can each individually trade. Rule 144 promulgated under the ’33 Act provides insiders with a mechanism to register the previously unregistered shares they hold. While the provisions of this rule are relatively straightforward, it limits the number of shares that an individual insider can sell in any three-month period to the greater of one percent of the shares outstanding or the average weekly trading volume during the four calendar weeks preceding the filing of his or her notification of intent to sell the shares on Form 144. Nevertheless, the expiration of the lockup enables insiders to liquidate some portion of their holdings publicly for the first time.

Bradley et al. (2001) document average unlock-day abnormal returns of -0.74 percent and a -1.61 percent abnormal return during the five-day window surrounding the lockup expiration date, which appear to be concentrated in firms with venture capital backing. Similarly, Field and Hanka (2001) find a -1.5 percent abnormal return during the three-day period surrounding the lockup expiration date, which they partially attribute to downward sloping demand curves and larger-than-expected insider selling. They also provide evidence of a permanent 40 percent increase in trading volume. Brav and Gompers (2003) report similar evidence with respect to returns and volume. Thus, the expiration of the lockup agreement represents a shift in the nature of trading soon after the offering.

Brav and Gompers (2003) test three potential explanations for the presence of lockups in the IPO setting. The first possible explanation is that insiders agree to lockup their shares for a longer period of time as a signal of firm quality. The second explanation is that lockups serve as a commitment device to help overcome information asymmetry, and firms with a larger moral hazard problem are more likely to agree to a longer lockup period. The final possibility they examine is whether underwriters use lockups to extract additional compensation from the company by requiring that the lead underwriter be the market maker for pre-lockup sales or by receiving additional fees for underwriting a formal seasoned equity offering (SEO). Brav and Gompers' (2003) results support the explanation that lockups serve as commitment devices; that is, longer lockups are associated with companies that are more likely to suffer from information asymmetry as a result of moral hazard. Furthermore, they find that underwriters of firms with lower incentives for moral hazard are more likely to let insiders at these firms sell shares before the lockup expires.[6]

[6] Fifteen percent of the IPOs in the Brav and Gompers (2003) sample report share sales prior to the end of the lockup period. The average (median) number of insider transactions for this subset of companies was six (two), and these sales represented an average (median) proportion of 5.2 percent (0.8 percent) of shares subject to the lockup agreement.

Earnings Management Subsequent to the IPO

The association between the information environment and lockups documented by Brav and Gompers (2003) suggests that insiders might have the opportunity to manage earnings without outsiders knowing about it. In order for earnings management to occur, however, the incentive to do so must be present. As Brav and Gompers (2003, 26 footnote 21) argue “because insiders sell little of their holdings at the IPO and are restricted from selling until after the lockup expires, engaging in earnings management prior to the release is clearly in their self interest.” Successfully increasing the price of the shares sold, however, would require the stock price to reflect the information contained in accruals.

Subramanyam (1996) demonstrates that the market prices discretionary accruals, and Sloan (1996) reports that stock prices fail to account for the lower persistence of the accrual component of earnings relative to the cash flow component of earnings. Xie (2001) extends these studies and concludes that the market overprices discretionary accruals. Finally, Beneish and Vargus (2002) find that income-increasing accruals drive accrual mispricing. Thus, it appears as though insiders planning to sell shares after the lockup expires can affect the stock price by engaging in earnings management during the period before the lockup expires.

A number of studies document significantly positive discretionary accruals in IPO-year financial statements. DuCharme, Malatesta, and Sefcik (2004); Teoh, Welch, and Wong (1998); and Teoh, Wong, and Rao (1998) document that, on average, IPO-year discretionary accruals are significantly positive. Teoh, Welch, and Wong (1998) and Teoh, Wong, and Rao (1998) also illustrate that, along with earnings, abnormal accruals decrease over the years subsequent to the IPO suggesting that managers increase IPO-year earnings to a level that is not sustainable in the future. Teoh, Welch, and Wong (1998) find that firms with higher levels of IPO-year discretionary accruals experience worse post-issuance stock market performance than IPO firms with lower levels of discretionary accruals. Finally, DuCharme, Malatesta, and Sefcik (2004) document a negative relation between discretionary accruals and post-issue returns, a positive relation between the lower post-issue returns and an increased probability of shareholder lawsuits, and a positive association between discretionary accruals (in absolute dollars) and lawsuit settlements (in absolute dollars). DuCharme, Malatesta, and Sefcik (2004, 47) conclude that “this evidence strongly supports the opportunism hypothesis.” Furthermore, these results also provide evidence that, in the context of IPOs, discretionary accrual models detect behavior that can result in an increased probability of litigation against the firm.

IPO Earnings Management Timing

For years prior to 1998, Standard & Poor’s did not capture pre-IPO financial data in its Compustat database. As a result, the studies with larger samples of IPOs (i.e., DuCharme, Malatesta, and Sefcik 2004; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998) measure discretionary accruals using IPO-year financial statement data. The argument in favor of using IPO-year accruals is that the incentive to manage earnings is likely to remain into the months following the offering. These incentives include pressure to meet verbal earnings projections communicated during road shows, pressure from underwriters to meet their analysts’ earnings projections, and the expiration of underwriter lockup agreements (Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998).

In terms of the potential explanations above, the lockup-related motivation seems like the most logical reason that the incentive to manage earnings extends into the periods following the IPO. While it is not possible to measure the verbal earnings projections communicated during road shows, it is also conceivable that these companies will issue earnings projections into the future and that the motivation to meet these projections is not unique to the post-IPO period. In addition, the pressure to meet analysts' forecasts is likely to persist into the future. The lockup expiration, however, typically occurs within 180 days of the offering. The lockup expiration presents the first opportunity for IPO insiders to publicly sell their shares. If these insiders wish to increase the share price at the time of their sales, one way to do so would be through income-increasing discretionary accruals because the market misprices discretionary accruals (Xie 2001) and accrual mispricing is concentrated in income-increasing accruals (Beneish and Vargus 2002).

On the other hand, Friedlan (1994) and DuCharme, Malatesta, and Sefcik (2001) document positive discretionary accruals in the period preceding the IPO. More recently, however, Venkataraman, Weber, and Willenborg (2005) find negative discretionary accruals during the pre-IPO period, which they argue is consistent with auditor conservatism. They also document that these pre-IPO discretionary accruals are significantly less than those from the post-IPO period, and that post-IPO performance-adjusted discretionary accruals are significantly positive. I build on their analyses by proposing that the expiration of the lockup results in higher discretionary accruals following the offering.

Not all companies go public during the same part of their fiscal year. As a result, Rule 3-12 of Regulation S-X governs the period of the financial statements required to be included in the prospectus depending on the expected IPO date. Because the lockup expiration date depends on the offering date, the period of the financial statements publicly available at the time the lockup period expires also depend on the offering date. For companies that file a 10-K after the offering but before the lockup expires, the publicly available financial statements before the IPO are different from the publicly available financial statements before the lockup expires. Therefore, it is possible to test whether, on average, IPO companies act on the incentive to manage earnings to increase the offering proceeds or the incentive to manage earnings to increase the share price around the time of the lockup expiration. With respect to the difference in discretionary accruals between the pre-IPO period and the pre-lockup-expiration period, my formal hypothesis stated in its alternative form is as follows:

> **H1a**: For the companies that file a 10-K *before* the lockup period expires, the level of discretionary accruals included in the last annual financial statements before the IPO is significantly different from the level of discretionary accruals included in the annual financial statements publicly available at the time that the lockup expires.

Underwriters do not require all companies to have lockup arrangements in connection with an IPO. Furthermore, not all companies will file annual financial statements between the time of their IPO and the expiration of a lockup agreement. For example, Hotels.com went public on February 20, 2000, and the company's insiders agreed to a 180-day lockup agreement with its lead underwriter, Donaldson, Lufkin & Jenrette. The company's final prospectus included financial statements as of and for the period ended December 31, 1999, and the company did not file its 10-K with its

December 31, 2000 financial statements until April 2, 2001, well after the August 23, 2000 lockup period expired. As a result, the pre-IPO and the pre-lockup-expiration financial statements for Hotels.com are the same. See Figure 1 for a timeline of these events.[7]

Previous IPO research that measures discretionary accruals in the IPO year treats the following companies the same way: (1) companies without a lockup, (2) companies similar to Hotels.com that do not file a 10-K until *after* the lockup expiration, and (3) companies with lockup agreements that file a 10-K *before* the lockup period expires. That is, they measure discretionary accruals for these IPO-firm groups at the same point in time relative to the IPO – the IPO year. If the lockup expiration is the reason in favor of measuring discretionary accruals in the IPO year, however, measuring the accruals for groups (1) and (2) above during that period is problematic because there is no lockup for group (1) and the IPO-year financial statements are not public until *after* the lockup expires for group (2). As a result, the relative strength of the motivation to manage earnings in each of the different periods becomes irrelevant because the pre-IPO financial statements are the same as those publicly available before the lockup expires. This line of reasoning would suggest that discretionary accruals would be significantly higher in the pre-IPO period than those in the IPO year for both of these groups of companies.

On the other hand, the '33 Act applies to the pre-IPO financial statements, and the increased potential costs of misstating financial statements in this period might outweigh the benefits of managing earnings before the lockup expires. Furthermore, Venkataram, Weber, and Willenborg (2005) find that IPO firms record income-decreasing

[7] All Tables and Figures for Part 1 are included under the heading Part 1: Appendix.

discretionary accruals in the pre-IPO period due to conservatism. Thus, I am unable to predict a sign for the difference in discretionary accruals between the pre-IPO year and the IPO year for this group of companies. My formal hypothesis stated in the alternative form for the firms with IPO-year financial statements that are not public until *after* the lockup expires is as follows:

> **H1b**: For the companies that file their first 10-K *after* the lockup period expires, the level of discretionary accruals included in the last annual financial statements before the IPO is significantly different from the level of discretionary accruals included in the first annual financial statements filed after the IPO.[8]

Post-Lockup Trading and Earnings Management

The argument that managers have the incentive to manage earnings before the expiration of the lockup period assumes that the amount of insider selling is significant enough to entice managers to manage earnings prior to the lockup expiration. As a result, I examine whether the level of discretionary accruals depends on the amount of insider trading that occurs subsequent to the lockup expiration.

Previous research documents a relation between earnings management and insider trading. Beneish (1999) finds that managers in firms subject to SEC enforcement actions for earnings overstatements are more likely than control firms to have sold shares or to have exercised stock appreciation rights during the period in which earnings were overstated. He also provides evidence that these overstatements mislead investors and delay drops in share prices and, therefore, that overstatement-firm managers profit from their sales. Beneish does note, however, that the results of his study apply to companies

[8] All of the sample firms have a lockup agreement. As a result, I do not include a formal hypothesis for the firms without a lockup agreement.

that the SEC has prosecuted, which hampers the ability to generalize the results to all insider sales that follow earnings manipulation.

Other studies examine the relation between insider trading and accruals using a broader sample of companies. Beneish and Vargus (2002) examine whether insider trading contains information about earnings quality. Their results suggest that, when insider selling accompanies income-increasing accruals, the persistence of these accruals is significantly lower than when insider buying accompanies income-increasing accruals. Based on further testing to explore whether opportunistic earnings management or changes in the economic environment in which these companies operate drive their results, Beneish and Vargus (2002) conclude that their findings can be at least partially explained by opportunistic earnings management. Park and Park (2004) find similar results with respect to the association between insider sales and earnings management, and they also provide evidence that this result is robust to controlling for the possibility that managers decide to sell *after* their companies report abnormally high accruals.

The results in the IPO setting are similar to those for other public companies. Darrough and Rangan (2005) document a negative relation between managerial selling and the change in R&D expenditures for a sample of R&D intensive IPO firms. They interpret this result as suggesting that managers believe that investors fixate on earnings and, therefore, borrow from the future benefits of R&D to increase IPO-year earnings. In addition, they document a positive relation between IPO-year discretionary accruals and managerial selling for their sample.

Darrough and Rangan’s (2005) sample includes 243 firms that went public between 1986 and 1990. Because their research question focuses on earnings management through a specific expense, R&D, rather than overall earnings, the authors eliminate any companies that had zero R&D expenditures. This requirement results in the loss of 375 IPOs from their sample. As a result, the ability to generalize their results for the association between managerial trading and discretionary accruals to a broader set of IPO firms is unclear. To build on Darrough and Rangan's (2005) results, I test whether the level of discretionary accruals before the lockup expires is positively associated with the proportion of shares that managers sell subsequent to the expiration of the lockup agreement. My formal hypothesis that addresses this question is stated in the alternative form as follows:

> **H2**: There is a positive relation between managerial selling after the lockup expires and the level of discretionary accruals included in the annual financial statements publicly available at the time that the lockup expires.

3. RESEARCH DESIGN

The Discretionary Accruals Model, Heteroskedasticity, and Scaling

I use a modified version of the Jones (1991) model to estimate discretionary accruals, my proxy for earnings management. This model estimates non-discretionary accruals using the cross-sectional method outlined in DeFond and Jiambalvo (1994) and an accounts receivable adjustment as outlined in DeFond and Park (1997). The model suggests that the expected level of accruals is a function of cash-basis revenues (i.e., the change in revenues adjusted for the change in accounts receivable) and the level of property, plant, and equipment:

$$TACC_i = \alpha_0 + \alpha_1(\Delta Sales_i - \Delta AR_i) + \alpha_2 PPE_i + \varepsilon_i, \quad (1)$$

where i is a firm subscript, $TACC$ equals total accruals (i.e., the difference between some measure of income and cash flows), $\Delta Sales_i - \Delta AR_i$ equals the year-over-year change in cash-basis revenues, and PPE equals the level of gross property, plant, and equipment. The model includes the change in cash-basis sales to measure the expected level of working capital accruals, which is expected to change as revenues change, and the level of property, plant, and equipment to measure the expected depreciation component of accruals.

Accruals result from timing differences between when companies recognize revenue and when the cash actually changes hands. As companies grow, transaction volume increases. The number and magnitude of potential timing differences increase as transaction volume increases. Therefore, the variation in total accruals is likely to increase with company growth and size. As a result, the Jones (1991) model suffers from heteroskedasticity, a violation of the OLS assumption that the variance of the residuals is constant. This violation leads to inefficient, but unbiased OLS coefficient estimates.

The widely accepted form of the model used in prior literature weights each variable by lagged total assets (see e.g., Dechow, Sloan, and Sweeney 1995; DeFond and Jiambalvo 1994; Jones 1991; Kasznik 1999; Klein 2002; Kothari, Leone, and Wasley 2005) as a control for heteroskedasticity. Furthermore, this heteroskedasticity is assumed to be proportional to lagged total assets squared, A_{t-1}^2 (see Jones 1991, footnote 33). Thus, $Var(\varepsilon_i) = E(\varepsilon_i^2) = \sigma^2 * A_{t-1}^2$, and the standard deviation of $\varepsilon_i = \sigma * A_{t-1}$. Weighting each variable in equation (1) by the inverse of A_{t-1} corrects for heteroskedasticity of this specific functional form. One can then estimate the following transformed equation via OLS:

$$\frac{TACC}{A_{t-1}} = \alpha_0 \frac{1}{A_{t-1}} + \alpha_1 \frac{\Delta Sales - \Delta AR}{A_{t-1}} + \alpha_2 \frac{PPE}{A_{t-1}} + \varepsilon \frac{1}{A_{t-1}}. \qquad (2)$$

Note that this is equivalent to running WLS and defining the weight such that each observation is weighted by the inverse of lagged assets. In equation (2),

$$Var(\varepsilon_i) = Var\left(\frac{\varepsilon_i}{A_{t-1}}\right) = \frac{E(\varepsilon_i^2)}{A_{t-1}^2} = \frac{\sigma^2 * A_{t-1}^2}{A_{t-1}^2} = \sigma^2. \qquad (3)$$

Thus WLS fixes the heteroskedasticity problem when the heteroskedasticity is proportional to lagged assets.

To assess how well WLS addresses the heteroskedasticity problem, I estimate discretionary accruals using both OLS and WLS. After each industry regression for each year, I perform White’s (1980) heteroskedasticity test. I then compare the results from White’s (1980) test for the OLS models with those from the WLS model.

Another potential issue with the discretionary accruals model is scaling. Barth and Kallapur (1996, 530) argue that running a cross-sectional model using financial data can be problematic if the research question must be answered “*after controlling for scale differences*” and the model does not control for these scaling differences. The problem in the context of the discretionary accruals model is that the observed variables (*TACC*, $\Delta Sales_i - \Delta AR_i$, and *PPE*) actually include a scale factor, S_i, which I assume to have a multiplicative effect for purposes of illustration. The equivalent version of equation (1) that reflects this scaling effect is as follows:

$$TACC_i * S_i = a * S_i + b(\Delta Sales_i - \Delta AR_i) * S_i + cPPE_i * S_i + e_i. \qquad (4)$$

This equation parallels equation (2) from Barth and Kallapur (1996, 531). Note that the original constant term in this equation, *a*, is multiplied by the scale variable, S_i. As Barth and Kallapur (1996) highlight, however, the scale variable, S_i, is frequently not observable. Thus, the following equation remains:

$$TACC_i * S_i = a' + b'\Delta Sales_i * S_i + c'PPE_i * S_i + u_i. \quad (5)$$

Notice that in equation (5) a' represents the intercept term, while in equation (4) the intercept term, *a*, is multiplied by the scaling factor, S_i. As a result, equation (5) omits a relevant variable that is correlated with the other variables in the model. This correlated omitted variables problem results in biased coefficients.

If the scaling factor were known and observable, researchers could simply multiply each of the variables in equation (4) by the inverse of the scaling factor. As discussed above, however, this scaling factor is frequently not observable. This problem presents researchers with the need to select a proxy variable to measure S_i that can be used (1) to deflate the observed variables or (2) as a scale-related control variable in the model. Barth and Kallapur (1996) use simulations to test the effects of using proxy variables along with these two alternative solutions.[9] In terms of reducing coefficient bias, Barth and Kallapur (1996) find that including a scale proxy as a control variable is more effective than deflation using the same scale proxy.

The modified-Jones model deflates all the variables in the equation by total assets at the beginning of the period to control for heteroskedasticity because she finds the

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[9] Barth and Kallapur (1996) examine the scaling effects of estimating the relation between the market value of equity and earnings, not the scaling effects of estimating the Jones (1991) model. Given that researchers use financial data that is not purged of the actual scale effect to estimate the Jones (1991) model, however, it is reasonable to generalize the Barth and Kallapur (1996) results to the discretionary accrual estimation process.

squared residuals from the unscaled model [equation (1) above] to be highly correlated with squared lagged assets. Although the reason for scaling is to address a heteroskedasticity problem, this method also addresses the scale issue. While scaling by lagged total assets controls for heteroskedasticity when the variance of the residual is proportional to squared lagged assets, it is not clear whether lagged assets is the true scale factor. If lagged assets do not represent the true scaling factor, the Barth and Kallapur (1996) results suggest that including lagged assets as a scale proxy in equation (1) and not scaling any of the variables has the potential to reduce bias and increase the efficiency of the results.

In my main analyses, I estimate discretionary accruals in a manner consistent with the prior literature (i.e., using WLS). I also estimate discretionary accruals using the OLS estimator on a model that includes lagged assets as a scale proxy in the regression without dividing through by lagged assets, and I discuss these results in Section 5. The next section discusses the discretionary accrual estimation process in more detail.

Discretionary Accruals Estimation

I calculate total accruals for all Compustat firms using equation (2) from Hribar and Collins (2002, 109) as follows:

$$TACC_{j,t} = EBXI_{j,t} - CFOPS_{j,t}, \tag{6}$$

where *j* and *t* are firm and time subscripts, respectively, and *TACC* equals total accruals, *EBXI* equals income before extraordinary items from the statement of cash flows (DATA123), and *CFOPS* equals net cash flows from operating activities adjusted for extraordinary items and discontinued operations (DATA308 – DATA124).

As discussed above, I estimate non-discretionary accruals using the cross-sectional method outlined in DeFond and Jiambalvo (1994) and an accounts receivable adjustment as outlined in DeFond and Park (1997). This method groups all companies with sufficient Compustat data with other companies from the same two-digit SIC code in the years in which earnings management is hypothesized and estimates non-discretionary accruals using the following equation:[10,11]

$$\frac{TACC_{j,t}}{A_{j,t-1}} = \beta_0 + \beta_1 \frac{1}{A_{j,t-1}} + \beta_2 \frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{A_{j,t-1}} + \beta_3 \frac{PPE_{j,t}}{A_{j,t-1}} + \varepsilon_{j,t}, \qquad (7)$$

where j and t are firm and time subscripts, respectively, and $TACC$ equals total accruals from equation (6), A equals total assets (DATA6), $\Delta Sales$ equals the change in revenues (DATA12) from the prior period, ΔAR equals the change in receivables (DATA2) from the prior period, and PPE equals the level of gross property, plant, and equipment (DATA7).[12] I estimate the model for industries with at least 20 observations.

Discretionary accruals for the sample firms are then calculated using the estimates from equation (7):

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[10] Two-digit SIC codes group old- and new-economy firms together. To subdivide companies into old- and new-economy groups, I use the new-economy definitions from Murphy (2003). This affects the two-digit SIC code groups listed below. Companies in the four-digit SIC code groups included in parentheses are considered new-economy companies, while all other four-digit SIC code groups are not. The affected groups are as follows: 35 (3570, 3571, 3572, 3576, and 3577), 36 (3661 and 3674), 48 (4812 and 4813), 50 (5045), 59 (5961), and 73 (7370, 7371, 7372, and 7373).

[11] I follow Kothari, Leone, and Wasley (2005) and add an additional constant term to this equation. Kennedy (2003) argues that including an additional constant in a WLS model can help to control for potential omitted variables bias and that inclusion of this additional constant does not create additional bias. Note that this is equivalent to including lagged assets in the base model before dividing through by lagged assets. Thus, this model is actually somewhat of a hybrid between a WLS model and a model that controls for scale by using a scale proxy.

[12] Following Teoh, Welch, and Wong (1998) and Teoh, Wong, and Rao (1998), I estimate equation (2) by excluding all sample firms and all firms that conducted an SEO in the respective year from the estimation sample. Given that Compustat now includes pre-IPO data, I also exclude IPO companies during the pre-IPO period.

$$DA_{j,t} = \frac{TACC_{j,t}}{A_{j,t-1}} - \hat{\beta}_0 - \hat{\beta}_1 \frac{1}{A_{j,t-1}} - \hat{\beta}_2 \frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{A_{j,t-1}} - \hat{\beta}_3 \frac{PPE_{j,t}}{A_{j,t-1}}, \quad (8)$$

where j and t are firm and time subscripts, respectively, and DA equals discretionary accruals and each $\hat{\beta}$ represents the industry-specific coefficient estimated from equation (7). Thus, $DA_{j,t}$ is basically the residual from equation (7).

Finally, I adjust the discretionary accruals calculated in equation (8) using a portfolio approach employed in prior literature (e.g., Ashbaugh, LaFond, and Mayhew 2003; Kasznik 1999; and Klein 2002). Kothari, Leone, and Wasley (2005) demonstrate that adjusting for the discretionary accruals of similarly performing firms can correct for the misspecification of discretionary accrual models previously documented for companies with extreme performance (see e.g., Dechow, Sloan, and Sweeney 1995). I adjust for performance by first assigning the sample companies to a return on sales (ROS) decile. Each decile contains firms from the same industry with sufficient Compustat data to calculate discretionary accruals and ROS. I create the deciles by sorting the firms in each industry based on their ROS value in a given year and assigning them each to one of 10 groups based on their ROS ranking. Finally, I calculate the median discretionary accruals for each decile (excluding the sample companies), and I subtract this value from the sample firms' discretionary accrual estimate. While Kothari, Leone, and Wasley (2005) use return on assets (ROA) to measure performance, I use ROS because, as Teoh, Wong, and Rao (1998) highlight, ROA in the IPO year should be lower because the IPO proceeds would immediately increase assets, whereas IPO proceeds are less likely to have an immediate impact on sales. The performance adjusted discretionary accrual is calculated as follows:

$$PADA_{j,t} = DA_{j,t} - MedianDA_{ROS\ Decile,t}, \tag{9}$$

where *j* and *t* are firm and time subscripts, respectively, and *PADA* equals performance-adjusted discretionary accruals, $DA_{j,t}$ equals discretionary accruals calculated using equation (8) for the sample firm, and $MedianDA_{ROS\ Decile,t}$ equals the median discretionary accrual for the given ROS decile.

Sample Selection

Table 1 provides an outline of the sample selection process. The sample of IPO firms comes from the Securities Data Corporation (SDC) New Issues Database, which includes firm-commitment IPOs. I include companies that completed common stock IPOs during the period between January 1, 2000 and the signing of SOX on July 30, 2002. The sample excludes ADRs, REITs (SIC code 6798), partnerships, closed-end funds, unit offers, financial institutions (SIC codes 6000-6199), and insurance companies (SIC codes 6300-6411). I exclude financial institutions and insurance companies because the accrual composition for these firms is unique to these industries. These restrictions result in 459 IPOs. I further restrict the sample to include firms with sufficient financial data available on the 2004 Compustat Industrial, Full Coverage, and Research files. I then merge this sample with the CRSP Daily Stock File. Ninety-five companies are missing data necessary to estimate discretionary accruals, 21 companies report zero revenues (which results in these companies having an undefined ROS measure), 20 are missing other financial data needed to calculate the control variables, four companies come from two-digit SIC codes with fewer than 20 companies, and four companies are missing from Compustat.

Using the SEC's EDGAR database, I hand collect the date of the last annual financial statements included in the prospectus, the date that each sample company files its first 10-K, whether or not a lockup agreement exists, and the length of the lockup agreement.[13] EDGAR is missing one or both of the filings for nine companies. Finally, I exclude eight firms because they have discretionary accrual values greater than (less than) the 99^{th} (1^{st}) percentile and were not excluded for other reasons. The final sample used to test H1 includes 298 IPOs.

For the insider trading analysis, I gather the trading data from Table 1 of the Thomson Financial Insiders database. Table 1 includes the common stock transactions of insiders reported on SEC Form 4, which includes any changes in insider ownership. Before the passage of the Sarbanes-Oxley Act (SOX) in July 2002, Section 16 of the '33 Act required the Form 4 to be filed within 10 days following the end of each calendar month. Section 403 of SOX, however, amended this requirement so that the Form 4 now must be filed within two days of the transaction execution.

Tests of IPO Earnings Management Timing

The sample used to test H1 is a balanced two-period panel of data in which each of the 298 sample companies is included in the analysis twice. The data for the first firm-year observation comes from the last annual financial statements included in the IPO prospectus, and the data for the second firm-year observation comes from the first 10-K the company files. I use the following model to test H1:

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[13] While SDC provides lockup-related data, it is often incorrect. Of the 298 sample firms, for example, SDC identified 138 firms as not having a lockup agreement. An examination of the prospectuses for these firms, however, identified the existence of a lockup agreement for all of these companies.

$$PADA = \beta_0 + \beta_1 POST + \beta_2 PRELOCK + \beta_3 PRELOCK*POST + \beta_4 AUDITOR + \beta_5 CFO + \beta_6 SIZE + \beta_7 MKTBK + \beta_8 LEV + \beta_9 FCOND + \beta_{10} LOSS + \sum \alpha_j TimeControls + \varepsilon, \quad (10)$$

where firm and time subscripts are omitted for expositional convenience.

The dependent variable, *PADA*, is the performance-adjusted discretionary accrual estimate from equation (9). *POST* equals "1" if the data for the firm-year observation comes from the IPO year, and "0" if the data for the firm-year observation comes from the last annual period included in the offering prospectus. *PRELOCK*, a measure of whether or not the company provides new annual financial data to the market before the lockup agreement expires, equals "1" if the company files its first 10-K *before* its lockup agreement expires, and "0" otherwise. I use data from the SDC New Issues Database and the offering prospectus to determine when the lockup period expires. I then search EDGAR to determine the date of the last annual financial statements included in the prospectus and the date that the company filed its first 10-K. I then determine whether the 10-K filing date occurred before or after the lockup expiration.

H1a examines whether managers of IPO firms that file their first 10-K *before* the lockup expiration tend to act on the incentive to manage earnings before the offering or the incentive to manage earnings before the lockup expires. I test this hypothesis using a joint significance test that $\beta_1 + \beta_3 = 0$ in equation (10). A significantly positive (negative) effect would suggest that *PADA* in the IPO-year is significantly higher (lower) than *PADA* in the pre-IPO year for this group of companies. H1b explores whether IPO companies that file their first 10-K *after* the lockup expires exhibit more earnings management behavior in the pre-IPO or post-IPO financial statements. A statistically significant coefficient on β_1 in equation (10) would provide support for H1b.

The control variables in equation (10) include factors commonly controlled for in recent earnings management studies (e.g., Ashbaugh, LaFond, and Mayhew 2003; Butler, Leone, and Willenborg 2004; Menon and Williams 2004). *AUDITOR* equals "1" if Compustat (DATA149) indicates the presence of a Big N auditor for the firm-year, and "0" otherwise. *CFO* represents cash flows from operations (DATA308) at the end of the period scaled by total assets (DATA6) at the end of the period. *SIZE* equals the natural log of market value, where pre-IPO period market value is measured using the price and number of shares outstanding as of the first day of trading from the CRSP Daily Stock file, and equity market value in the IPO year is the share price multiplied by the number of shares outstanding (DATA199*DATA25) at the end of the post-IPO period. *MKTBK* is the equity market value divided by the book value of assets (DATA6) at the end of the period. *LEV* is total debt (DATA9+DATA34) divided by total assets (DATA6), all at the end of the period. *FCOND* is Zmijewski's (1984) financial condition index, which controls for financial distress.[14] *LOSS* equals "1" if the company had a net loss (DATA172) during the period, and "0" otherwise. I also include time control indicator variables, *TimeControls*, to control for possible systematic differences in discretionary accruals during each period.

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[14] This measure is a Z-statistic that is calculated using the following variables and coefficient estimates that Zmijewski (1984) obtained by using a weighted exogenous sample maximum likelihood method to estimate his probit model:

$$-4.803 - 3.6*(EBXI/A) + 5.4*(LEV) - 0.1*(CASSTS/CLIABS),$$

where *EBXI*, *A*, and *LEV* are measured as previously defined, and *CASSTS* equals total current assets (DATA4) and *CLIABS* equals total current liabilities (DATA5), all measured at the end of the period.

Tests of Post-Lockup Trading and Earnings Management

Examining discretionary accruals before the lockup expires assumes that the amount of insider selling is significant enough to entice the insiders to manage earnings prior to the lockup expiration. H2 predicts that managerial selling after the lockup expires is positively related to the level of discretionary accruals in the annual financial statements available when the lockup expires. To test this hypothesis I run a modified version of equation (10) for the sample companies, and I use data from the last annual financial statements available at the time the lockup expires. I replace the *POST* variable from equation (10) with *SALES* as follows:

$$\begin{aligned} PADA = {} & \gamma_0 + \gamma_1 SALES + \gamma_2 PRELOCK + \gamma_3 PRELOCK * SALES + \gamma_4 AUDITOR \\ & + \gamma_5 CFO + \gamma_6 SIZE + \gamma_7 MKTBK + \gamma_8 LEV + \gamma_9 FCOND + \gamma_{10} LOSS \\ & + \sum \delta_j TimeControls + \xi \end{aligned} \tag{11}$$

The *SALES* variable captures the number of post-lockup expiration shares that the CEO and CFO trade as a proportion of the total number of shares outstanding after the offering. The numerator of the *SALES* variable captures the number of CEO and CFO shares traded during the three month period beginning on the day the lockup expires. I calculate this amount by summing each CEO and CFO common stock sale contained in Table 1 of the Thomson Financial Insiders database. The denominator of the *SALES* variable, total post-IPO shares outstanding, comes from the CRSP Daily Stock File. I expect to find a positive relation between post-lockup expiration insider selling and pre-lockup expiration discretionary accruals, and I include the interaction between *PRELOCK* and *SALES* to allow the relation between *SALES* and *PADA* to differ for firms that file a 10-K *before* the lockup period expiration and those that do not.

4. RESULTS

Sample Description

Table 2 presents the distribution of sample firms by year. The vast majority of the sample (216 companies) went public during 2000. Forty-nine companies completed an IPO in 2001, and 33 companies went public between January 1, 2002 and July 30, 2002. In terms of the date of the financial statement data included in the prospectuses, 193 prospectuses include 1999 data, 64 prospectuses include 2000 data, 39 include 2001 data, and 2 include 2002 data.

Table 3 shows a cross tabulation between the length of the lockup period and whether the company filed a 10-K *before* the lockup expires. Approximately 96 percent of the sample firms (286/298) have a 180-day lockup period, while seven have a 90-day lockup period. The sample includes one firm in each of the 360-, 365-, and 720-day lockup period groups, and two firms in the 540-day group. Finally, a total of 59 sample companies (approximately 20 percent) filed their first 10-K prior to the lockup expiration.

Table 4 includes the variable definitions and Table 5 presents descriptive statistics for the control variables. Virtually all of the sample companies have a Big N auditor in both the pre- and post-IPO periods. On average, cash flows from operations represent -21 percent of assets before the offering, which improves to negative eight percent after the IPO. The median market value after the first day of trading is $453.9 million, and the median market value at the end of the IPO year is $326.5 million. The median market to book ratio is 12 and 2.14 for these respective periods. Total debt represents 31 percent of assets in the prospectus financial statements, and 10 percent of assets at the end of the

IPO year, and the average probability of bankruptcy is low. Finally, 73 percent and 69 percent of the sample had a net loss in the pre- and post-IPO periods, respectively.

Univariate Tests of IPO Earnings Management Timing

Univariate tests provide some evidence of differences in discretionary accruals between the pre- and post-IPO periods. Table 6 provides the means and medians for discretionary accruals categorized by the *POST* and *PRELOCK* variables. The t-tests provide evidence that overall average pre-IPO discretionary accruals are not significantly different from zero, but that average pre-IPO discretionary accruals are significantly positive for the firms that file a 10-K *before* the expiration of the lockup period ($p < 0.05$). These tests also provide evidence that, on average, IPO-year discretionary accruals are significantly positive ($p < 0.01$), and that these positive discretionary accruals occur for both groups of *PRELOCK* firms. The results in terms of medians are generally consistent with those from the t-tests except that median IPO-year discretionary accruals for companies that file a 10-K *after* the expiration of the lockup period are not significantly different from zero.

Table 7 provides tests for differences in mean (median) discretionary accruals between the pre- and post-IPO periods based on paired t-tests (sign tests). The t-test results suggest that average discretionary accruals in the post-IPO period are significantly larger than those in the pre-IPO period ($p < 0.01$). The sign test results provide similar evidence for the medians ($p < 0.05$). The tests also suggest that, for the companies that file a 10-K *before* their lockup period ends, average and median discretionary accruals in the post-IPO period are significantly larger than those from the pre-IPO period ($p < 0.01$ and $p < 0.05$, respectively), evidence in support of H1a. While the t-tests provide evidence in favor of H1b ($p < 0.01$), I fail to find a significant difference in median discretionary accruals between the two periods for companies that file a 10-K *after* their lockup period ends ($p > 0.10$). Thus, the univariate results for H1b are mixed. While the tests above provide evidence in favor of H1a and some support for H1b, these tests do not control for the potential correlation between the test variables and other determinants of discretionary accruals. Therefore, these results should be interpreted with caution.

Univariate Tests of Post-Lockup Trading and Earnings Management

The tests of H2 examine the relation between post-lockup expiration CEO and CFO trading and earnings management behavior in the pre-trading financial statements. Figure 2 provides some insights into CEO and CFO trading patterns (in terms of the number of shares sold) in the months following the offering. First, trading activity appears to spike in month seven. This is consistent with 96 percent of the sample firms having a 180-day lockup period that would expire at the end of month six, which enables insiders to begin selling in month seven.[15] Second, these insiders sell very few shares prior to the lockup expiration. Finally, while Figure 2 does show that the average level of trading increases following the lockup expiration, the average number of shares sold in the other months with spikes (i.e., months 11 and 14) is around half of the average number shares sold during month 7. This suggests that CEO and CFO selling in the month that the lockup expires is much larger, on average, than typical CEO and CFO monthly post-lockup selling.

[15] While Figure 2 shows the highest spike in month 17 for the mean number of shares sold, this spike is due to one firm with month 17 share sales of approximately 7.5 million shares. After excluding this one observation, the average number of shares sold drops from approximately 30,000 shares to 4,800 shares for month 17.

Table 8 provides summary statistics on CEO and CFO trading activity that occurs during the three months following the lockup expiration. The mean CEO and CFO shares sold as a proportion of total shares outstanding is 0.134 percent for all firms, and is 0.555 percent for the subset of sample companies with trading activity. Twenty-four percent (72/298) of the sample firms have CEO or CFO trading during this period, and the maximum percentage of shares sold during this period is approximately five percent. The 24 percent of sample firms with post-lockup expiration CEO and CFO trading that I observe is consistent with the 23 percent insider trading rate that Cao, Field, and Hanka (2004) document.

Table 9 categorizes discretionary accruals for each company by whether or not CEO and CFO trading occurs during the three month post-lockup period, and whether the pre-trading financial statement data comes from the prospectus (i.e., *PRELOCK* = 0) or the first 10-K (i.e., *PRELOCK* = 1). The overall t-test results suggest that the mean discretionary accruals for the firms that trade and the firms that do not trade are significantly positive ($p < 0.05$), but the sign tests are insignificant. In terms of the period of annual financial statements that are publicly available when the lockup expires, I find significantly positive mean ($p < 0.01$) and median ($p < 0.05$) discretionary accruals only for those companies that file a 10-K *before* the lockup expires. Looking closer at the post-IPO period financial statements, the t-tests suggest that average discretionary accruals are significantly positive for the firms that do not trade ($p <0.01$) and for the companies that do trade ($p <0.01$). Only the median for the post-IPO, non-trading sample firms is significantly positive ($p < 0.05$).

Tests of differences between the groups above (presented in Table 10) provide some evidence that mean discretionary accruals are significantly larger for the firms that trade as compared to those that do not trade both overall ($p < 0.10$) and during the post-IPO period ($p < 0.05$). The t-test results suggest that, on average, discretionary accruals in the first post-IPO 10-K are significantly higher overall, but that this result is driven by the firms with post-lockup CEO and CFO trading that file a 10-K *before* the lockup expiration. As mentioned above, however, these tests are univariate in nature and should be interpreted in conjunction with the multivariate tests that follow.

Heteroskedasticity Tests of the Discretionary Accruals Model

I assess the effect of using WLS on the discretionary accruals model by performing White's (1980) heteroskedasticity test. After estimating each cross-sectional industry model, I test for heteroskedasticity and compare the White's test results for the OLS model to those for the WLS model. To perform this analysis, I estimate 344 different regressions over the three-year sample period (172 using OLS and 172 using WLS for the two-digit SIC code industries with at least 20 firms with sufficient data).

White's test rejects the null hypothesis of no heteroskedasticity for 90 percent of the OLS regressions using a cutoff of $p < 0.10$. Comparing the results of White's test after the OLS models to those of the WLS model indicates that the χ^2 statistic decreases in 55 percent of the cases. While these decreases result in the significance level dropping in 27 percent of the cases, 77 percent of the tests suggest that heteroskedasticity still exists after estimating the WLS models using a cutoff of $p < 0.10$. The findings suggest that WLS addresses the heteroskedasticity issue in some models but not in others.

The discretionary accrual estimates I use are basically the residuals from an equation used to estimate the level of normal accruals. Ignoring scaling issues for the moment (which I address in Section 5 below), OLS residuals remain unbiased even in the presence of heteroskedasticity. As the results above indicate, heteroskedasticity still remains after using WLS in the discretionary accrual estimation process, suggesting that the heteroskedasticity is not necessarily proportional to lagged assets. Therefore, scaling by lagged assets might introduce measurement error into the estimation process. Wooldridge (2003) demonstrates that, as long as the measurement error in the dependent variable is not correlated with any of the independent variables, using OLS is not a problem.[16]

Multivariate Tests of IPO Earnings Management Timing

Table 11 provides the results from estimating equation (10). The data used to estimate this model is a balanced, two-year panel. As a result, I use panel methods to control for any time invariant, unobserved heterogeneity created by including the same firm in the analysis more than once. The main concern when using either the fixed or random effects specification is whether the effects are correlated with the explanatory variables in the model. The fixed effects estimator explicitly controls for these effects while the random effects estimator models these effects as part of a composite residual. When the random effects are correlated with an explanatory variable in the model, the

[16] Specifically, Wooldridge (2003) explains that, if the measurement error and the independent variables are uncorrelated, (1) the estimated coefficients will all be unbiased when the measurement error has a zero mean, and (2) the estimated coefficients with the exception of the intercept will be unbiased if the measurement error has a non-zero mean. As I do not interpret the intercept in the model and I do not have reason to believe that I am omitting a relevant variable, this measurement error does not present a problem in the analyses.

estimator is biased. With this in mind, I choose to report the results using the random effects estimator for a number of reasons.

First, the results indicate that the random effects explain a small proportion of the variation in *PADA* (0.087, untabulated), suggesting that using a random effects model in this case is not likely to be problematic. Second, the results from estimating the equation using OLS are qualitatively similar to those from the random effects model, providing further evidence that the random effects explain a small portion of the variation in discretionary accruals and, therefore, do not have a large impact on the estimated coefficients. Third, the fixed effects specification requires that all of the variables in the model have some within group variation across time. An important variable in equation (10), *PRELOCK*, is fixed over time. As a result, the fixed effects model drops this variable from the specification. Attempts to run a fixed effects model in this case would be problematic because the base effect for an important variable that appears in an interaction term would be omitted.

That said, I perform a Hausman (1978) specification test to examine whether the difference in coefficients between the fixed and random effects models is systematic. The Hausman test results in rejection of the null hypothesis that the difference in coefficients between the models is not systematic ($p < 0.01$). Aside from the control variables, the only statistically significant coefficient in the fixed effects specification is the *PRELOCK*POST* estimate, which is significantly positive and of a similar magnitude in both models. Therefore, for the reasons outlined above, I report the results using a random effects specification.

This model tests whether discretionary accruals differ significantly between the periods surrounding the IPO for the whole sample along with various subsets of the sample. The overall model F statistic is significant ($p < 0.01$) and the model R^2 is 0.106. Additionally, as discussed above, the random effects explain a small proportion of the variation of the composite residual.[17]

For the companies that file a 10-K *before* the end of the lockup period, H1a predicts a significant difference between *PADA* in the periods surrounding the IPO. As predicted, $\beta_1 + \beta_3$ is statistically significant ($p < 0.05$) and it is positive.[18] This provides evidence supporting H1a and indicates that post-IPO discretionary accruals are significantly larger than those in the period before the IPO for the companies that file a 10-K *before* the end of the lockup period. H1b explores whether post-IPO *PADA* is significantly different from pre-IPO *PADA* for IPO companies that file their first 10-K *after* the lockup expires. I fail to reject the null hypothesis that $\beta_1 = 0$ ($p > 0.10$). Thus, H1b is not supported.

Overall, the univariate and multivariate results provide evidence that, on average, post-IPO discretionary accruals are significantly positive, and that they are significantly larger than those in the pre-IPO period. Furthermore, the evidence indicates that this relation is confined to the group of companies that file a 10-K *before* the lockup expiration, which suggests that managers might increase discretionary accruals in the

[17] The results of estimating equation (5) using OLS with robust standard errors clustered by firm are qualitatively similar to those presented in Table 11.
[18] The joint significance test of $\beta_1 + \beta_3 = 0$ using a fixed effects specification is not statistically significant. As discussed earlier, however, the fixed effects specification omits an important variable, *PRELOCK*, while the random effects specification does not. Therefore, I report the results using the random effects estimator.

newly-filed financial statements in anticipation of selling shares after the lockup expires. Tests of H2 provide more direct tests of this issue.

The results for the control variables are generally as expected. Consistent with Ashbaugh, LaFond, and Mayhew (2003) and Menon and Williams (2004), I find a significantly negative relation between *PADA* and (1) cash flows from operations and (2) firm size. As expected, I also find that the probability of bankruptcy is negatively related to *PADA* (Menon and Williams 2004). Similar to Klein (2002), I find a significantly positive relation between leverage and discretionary accruals. I find no significant relation between the presence of a Big N auditor, the market to book ratio, and the presence of a loss and *PADA*. Finally, the untabulated results for the time control variables suggest that discretionary accruals in 1999 ($p < 0.05$) and 2002 ($p < 0.10$) are significantly lower than those in 2000, the omitted category.

Multivariate Tests of Post-Lockup Trading and Earnings Management

Park and Park (2004) suggest the possibility that insider trading is an endogenous explanatory variable when used in models with discretionary accruals as a dependent variable. As a sensitivity test, they re-estimate their main model using two-stage least squares, and conclude that their results are not affected by controlling for this potential endogeneity. To test whether the insider trading variable, *SALES*, is endogeneous, I run a variant of the Hausman (1978) test. In order to run this test, however, I must first identify potential instrumental variables that are likely to be correlated with *SALES*, but not *PADA*.

Aggarwal, Krigman, and Womack (2002) examine IPO-firm insider selling that occurs after underwriter lockups expire. They find that post-lockup expiration insider sales is a function of a number of variables: market value, venture capital backing (*VC* equals "1" if the SDC New Issues Database identifies the IPO as having venture capital participation, and "0" otherwise), analyst coverage indicator (*COV* equals "1" if the IPO has research coverage on First Call, and "0" otherwise), short interest at the lockup expiration (*SI* equals the short interest outstanding as reported on Bloomberg or in the Standard and Poor's *Daily Stock Price Record* for NYSE and AMEX firms or directly from NASDAQ for the NASDAQ firms), and analyst recommendations (divided between lead and non-lead analysts). Following Aggarwal, Krigman, and Womack (2002), I measure analyst recommendations using two variables, *NLEAD*, which equals the number of non-lead analyst recommendations on First Call, and *LEAD*, which equals the number of lead-underwriter analyst recommendations on First Call.

In addition to the variables listed above, I include three other instrumental variables. The first variable measures the proportion of secondary shares (i.e., shares included in the offering being sold by pre-IPO owners) sold in the IPO (*SECONDARY*), which I obtain from the SDC New Issues Database. The second variable measures insider ownership (*INSOWN*) after the offering, which I obtain from the SDC New Issues Database (when available) or hand collect from the offering prospectus. The final variable measures whether or not the CEO or CFO sells shares prior to the lockup expiration (*PRETRADE*), which I create from the data included in Table 1 of the Thomson Financial Insiders database. These measures of insider ownership and insider sales are expected to be related to post-lockup expiration CEO and CFO share sales.

The first step of this test is to regress the potentially endogeneous variable, *SALES*, on the exogeneous variables in equation (11) along with the instrumental variables listed above. Next, I save the residuals from this equation and include them as an independent variable in equation (11) along with *SALES* and the control variables. For this test, I exclude the interaction term (*PRELOCK*SALES*) because I am testing whether one piece of the interaction term, *SALES,* is endogeneous. The final test of endogeneity is the t-test of the whether the coefficient on the residual (*RESID*) is statistically significant.

Table 12 includes the results from both of these regressions. Model 1 includes the model regressing *SALES* on the exogenous variables from equation (11) along with the instrumental variables. The overall model is significant ($p < 0.01$) and the adjusted R^2 is 0.07. More importantly, the joint F-test that *SI* = *VC* = *COV* = *NLEAD* = *LEAD* = *PRETRADE* = *SECONDARY* = *INSOWN* = 0 is highly significant ($p < 0.01$), suggesting that at least one of the instrumental variables is a good predictor of *SALES.* Model 2 then estimates equation (11) with *RESID* and without the interaction term. The t-test for the coefficient on *RESID* is insignificant ($p = 0.62$). This suggests that *SALES* is not endogeneous in equation (11).

Table 13 provides the results from estimating equation (11) using OLS.[19] This model tests H2, which predicts a positive relation between post-lockup CEO and CFO trading and discretionary accruals in the financial statements available to the public when the lockup expires. The overall model F statistic is significant ($p < 0.01$) and the model adjusted R^2 is 0.20. The coefficient on *PRELOCK*SALES* is significantly positive ($p < 0.10$). This result indicates that the effect of *SALES* on *PADA* depends on which

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[19] The p-values reported throughout the paper for the OLS estimations are calculated using White (1980) standard errors. The inferences are unchanged running OLS with non-robust standard errors.

financial statements are publicly available at the time the lockup expires. The joint test that *SALES* + *SALES**POST* = 0 is significantly positive ($p < 0.10$), which suggests that, for companies that file a new 10-K *before* the expiration of the lockup agreement, the relation between the percentage of CEO and CFO shares traded and discretionary accruals is significantly positive.

Similar to the results for equation (10) I find a significantly negative relation between cash flows from operations, firm size, and the probability of bankruptcy and discretionary accruals, and a positive relation between and *LEV* and *PADA*. In contrast to the equation (10) results and consistent with Ashbaugh, LaFond, and Mayhew (2003), *MKTBK* is significantly positively related to *PADA* in equation (11). The untabulated results for the time control variables suggest that discretionary accruals in 1999 ($p < 0.05$) are significantly lower than those in 2000, the omitted category.

5. SENSITIVITY TESTS

Analysts' Earnings Estimates

One of the arguments for the reasonableness of using IPO-year discretionary accruals in the IPO setting is that management will be under pressure from its lead underwriter(s) to meet their analysts' established earnings targets (Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). This argument provides an alternative explanation for the observed findings for H1. As a result, this section addresses the concern that the pressure to meet analysts' forecasts is a correlated omitted variable in the original analyses.

To address this issue, I create a variable that captures whether or not the company meets or exceeds the mean First Call consensus earnings target in the IPO year. I begin by estimating non-discretionary earnings by subtracting my estimate of discretionary accruals from the income number used to estimate discretionary accruals (DATA123). Next, I divide non-discretionary earnings by total common shares outstanding (DATA25). This number represents my estimate of actual non-discretionary EPS. Next, I compare the last reported mean First Call consensus EPS forecast (obtained from the First Call Historical Database) for the IPO year end to the non-discretionary EPS number I calculate. I then create a variable, *ATABOVE*, that equals "1" if non-discretionary EPS is greater than or equal to the mean First Call consensus EPS forecast, and "0" otherwise. Finally, I add the *ATABOVE* variable to equation (10) and estimate the model running OLS on sample data from the IPO year.

Table 14 presents the results of estimating equation (10) using OLS on the IPO-year data both excluding and including the *ATABOVE* variable. The sample used for the first two models includes all of the sample companies while the third and fourth models include only the firms with an available First Call consensus EPS estimate for the IPO year. The results for Model 1 and Model 3 are consistent with the conclusions for H1; that is, IPO-year discretionary accruals are significantly larger for companies that file a new 10-K *before* the lockup expires than those firms that file a new 10-K *after* the lockup expires for the whole sample ($p < 0.05$) and for the subset of companies that have a First Call EPS estimate available ($p < 0.05$). *ATABOVE* is negative and highly significant in Model 2 ($p < 0.01$) and Model 4 ($p < 0.01$) suggesting that companies with estimated non-discretionary earnings at or above analysts' consensus forecasts have significantly lower discretionary accruals. More importantly, however, is that the results for the coefficients on the *PRELOCK* variable remain significantly positive after controlling for whether or not non-discretionary earnings are at or above the consensus forecast, suggesting that the main results are not sensitive to whether or not companies have pressure to manage earnings to meet or beat analysts' forecasts.

Fiscal Year Timing

PRELOCK is a measure of whether or not the company provides new annual financial data to the market before the lockup agreement expires. In the main analyses, this measure equals "1" if the company files its first 10-K *before* its lockup agreement expires, and "0" otherwise, regardless of whether or not the company files additional 10-Qs between the 10-K filing date and the lockup expiration date. Given the possibility that a company could file a new 10-Q before the lockup expires, the annual data for some of the *PRELOCK* = 1 companies is not the most recent financial data that is publicly available at the time the lockup expires. For these companies, CEOs and CFOs who plan to sell shares after the lockup expiration would have the opportunity to manage the earnings they disclose in the last 10-Q that is publicly released before the lockup expires. As a result, I would expect the results that I document for H1 to be strongest for the subset of firms whose 10-K is the last publicly filed financial statements at the time the lockup expires. To test this assertion, I define a new variable, *QPRELOCK*, which equals "1" if the company files its first 10-K *before* its lockup agreement expires *and* the company is not required to file a new 10-Q until after the lockup expiration, and "0" otherwise. To decide whether each *PRELOCK* = 1 firm is also a *QPRELOCK* = 1 firm, I determine whether the company would have been required to file a new 10-Q between the 10-K due date and the lockup expiration date.[20]

Table 15 includes two random effects models that test whether the H1 results are strongest for the companies that file a new 10-K *before* their lockup expires but do not file a new 10-Q until after the lockup expires. In Model 1, I replace the *PRELOCK* variable with *QPRELOCK*, and in Model 2 I estimate equation (10) excluding the companies for which *QPRELOCK* = 1. Consistent with the results for *PRELOCK**POST* from equation (10), Model 1 documents a significantly positive coefficient on *QPRELOCK**POST* ($p < 0.10$). Furthermore, the joint significance test for H1a is significant at the same levels as in equation (10) ($p < 0.05$). The results from Model 2 suggest that, after excluding the *QPRELOCK* = 1 companies, I no longer detect a significant relation between *POST* + *PRELOCK**POST*, and *PADA*. Taken together, these results suggest that the firms that file a 10-K *before* the lockup expires have significantly higher discretionary accruals, and that this relation is driven by the companies that do not file new financial statements between the 10-K filing and the date the lockup expires.

Type of Shares Issued (Primary versus Combined Offerings)

An interesting question regarding the timing of the incentive to manage earnings in the periods surrounding IPOs is who is selling the shares. When a company goes public by issuing new securities (i.e., primary shares), the company receives the proceeds from the offering and, in turn, can use these funds to finance its operating and investing

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[20] During the sample period, 10-K filings were due within 90 days of the end of a fiscal year, and 10-Q filings were due within 45 days of the end of each of the first three fiscal quarters. Using these requirements, I assign a value of "1" to *QPRELOCK* if the company files its first 10-K before its lockup period expires, and the lockup period expires within 135 days (90 days + 45 days) of its fiscal year end.

activities. On the other hand, the offering might include shares being sold by pre-IPO owners (typically insiders), and the proceeds from those shares (i.e., secondary shares) go to the previous owners. Given that the proceeds from primary shares flow to the company while the proceeds from secondary shares flow to the prior owners, whether or not the offering includes secondary shares could have an impact on the timing of earnings management surrounding the IPO.

The main results reported in Section 4 of this study suggest that CEO and CFO trading is positively associated with discretionary accruals in the post-IPO period when the company releases new annual financial statements after the offering but before insider trading can begin. To further test whether insider selling affects the timing of earnings management behavior, I examine the effect of an offering including secondary shares on the results for H1. I expect to observe that the significantly higher IPO-year discretionary accruals are concentrated in the *PRELOCK* firms when no secondary shares are sold in the offering. In addition, if insider sales really do drive the incentive to manage earnings, I would expect to find weaker IPO-year results for offerings that include secondary shares along with higher pre-IPO discretionary accruals.

I examine the effect of the mix of shares issued between primary and secondary shares by estimating equation (10) separately for the offerings that include (1) solely primary shares and (2) a combination of primary and secondary shares. In untabulated results I find that 265 of the 298 sample firms issue only primary shares in the IPO, and that the level of discretionary accruals included in first 10-K is significantly higher than the level of discretionary accruals included in the pre-IPO financial statements, but only for companies that file a new 10-K *before* the lockup expiration ($p < 0.05$). For the remaining 33 IPO firms that issue a combination of primary and secondary shares, on the other hand, the results suggest that IPO-year discretionary accruals are significantly lower than those in the pre-IPO period ($p < 0.10$), and that this result holds for both *PRELOCK* = 0 and *PRELOCK* = 1 firms (i.e., the coefficient on *PRELOCK**POST* is not significant). These findings provide further evidence that the timing of IPO-firm earnings management is related to the timing of when insiders can sell their shares.

Proportion of Equity Issued and Retained

Two questions related to the type of shares issued (i.e., primary versus secondary shares) are whether the amount of shares issued in the IPO and/or retained by insiders after the offering affect the observed relations. It is conceivable that the companies that issue a smaller proportion of shares in their IPO might do so in anticipation of selling additional shares later in an SEO. As a result, these firms might not have as big of an incentive to manage earnings around the time of their IPO if they plan to issue shares sometime in the future. While I am unable to measure the intent of management at the time of the IPO, I can observe whether their firm does or does not subsequently perform an SEO. Therefore, I attempt to control for this possibility by using the SDC New Issues Database to identify the firms that issue equity in subsequent SEOs. Interestingly, when I estimate equation (10) separately for companies that undertake an SEO within one year of the IPO, the overall model for the firms with subsequent SEOs becomes insignificant. In the model for firms that do not undertake a subsequent SEO, the test of *POST* + *POST**PRELOCK* = 0 remains significant ($p < 0.05$). I use a similar split on the sample that I use to estimate equation (11), and I find that the test of *SALES* + *SALES**PRELOCK* = 0 for the firms that do not undertake a subsequent SEO becomes more significant ($p < 0.01$). I fail to find a significant relation between *SALES* and *PADA* for the companies that do undertake a subsequent SEO.[21] These results suggest that, for the firms that file a new 10-K *before* the lockup expires, discretionary accruals are significantly larger in the IPO year than those in the pre-IPO year but that this result only holds for the firms that do not hold an SEO within one year of the offering. Furthermore, these findings suggest that the positive relation between *SALES* and *PADA* for the *PRELOCK* = 1 firms is confined to the companies that do not issue equity in a subsequent SEO.

One might argue that pre-lockup-period discretionary accruals are associated with the level of ownership retained by insiders. Leland and Pyle (1977) analytically demonstrate that retained ownership is a signal of firm quality that helps to reduce information asymmetry, and empirical evidence suggests that retained ownership is positively associated with IPO initial market value (Clarkson et al. 1991; Downes and Heinkel 1982). This suggests that retained ownership is a positive signal to the market of the future prospects of the firm because the original owners decide to hold onto their shares in the firm. While managers who hold onto a larger proportion of the firm might have more motivation to manage earnings in order to increase the IPO proceeds, the signaling argument suggests that managers of these firms might not need to manage earnings because these are higher quality companies. As a result, I measure retained insider ownership using the percentage of shares that directors and executive officers hold as disclosed in the prospectus. I then add this variable as an additional control

[21] The results of these analyses are not sensitive to splitting the sample based on whether or not the company undertakes an SEO within one, two, three, or four years of the offering.

variable in equations (10) and (11). The insider ownership variable is not statistically significant in either model, and the inferences from the models that include insider ownership are qualitatively similar to those drawn from equations (10) and (11) suggesting that, in the IPO setting, insider ownership does not have a statistically significant effect on discretionary accruals.

Underwriter Reputation

Prestigious underwriters tend to take lower risk firms public (Carter and Manaster 1990). One dimension of this risk is the aggressiveness of the IPO-firms' financial reporting behavior. Therefore, companies with more reputable underwriters might tend to engage in less earnings management around the time of the offering because they are higher quality companies. To control for underwriter reputation, I use the Loughran and Ritter (2004) underwriter rankings list that has been updated through 2004 and is available on Jay Ritter's website (Ritter).[22] These rankings build on those of Carter, Dark, and Singh (1998) and Carter and Manaster (1990). Using this list and the lead underwriter(s) for each sample company obtained from the SDC New Issues Database, I create an indicator variable that captures whether or not the lead underwriter (or one of the lead underwriters in the case of multiple leads) is identified on the prestigious underwriters list. Splitting the sample based on this variable, I re-estimate equations (10) and (11) separately for the firms with a reputable underwriter and for those without to see whether the inferences are sensitive to this classification. The results suggest that my findings for H1 and H2 are confined to the companies with underwriters that are not

[22] This list includes the following underwriters: ABN Amro, Banc of America Securities, Bear Stearns, Citigroup, CIBC, Credit Suisse First Boston, Deutsche Bank, Goldman Sachs, HSBC Securities, JP Morgan, Lazard, Lehman Brothers, Merrill Lynch, Morgan Stanley, Salomon Smith Barney, Sandler O'Neill Partners, Thomas Weisel Partners LLC, and UBS.

listed on the prestigious underwriters list, suggesting that firms that have a prestigious lead underwriter are less likely to manage earnings. Three potential explanations for this finding are that (1) reputable underwriters tend to take companies public that have a lower propensity to manage earnings, (2) reputable underwriters are able to constrain earnings management behavior, or (3) having a reputable underwriter provides a signal to the market of firm quality that alleviates the need to manage earnings. Future research might address this issue further.

Discretionary Accruals Model Specification

In this section, I re-estimate equation (10) and equation (11) using a different measure of the dependent variable, *PADA*. Instead of using discretionary accrual estimates from the WLS regressions that scale the variables by lagged assets (WLS estimates), I use the estimates from OLS regressions that include lagged assets as an independent variable (OLS estimates). The remainder of the estimation process (i.e., in terms of the variable definitions, performance adjustment, independent variables, and models used) is identical to that described in the Research Design and Results sections above.

First, I compare the H1 results from equation (10) estimated using the WLS estimates (see Table 11) to those from equation (10) estimated using the OLS estimates (untabulated). Using the OLS estimates, the equation (10) overall F statistic is significant ($p < 0.05$) but at a lower level than that for the equation using the WLS estimates ($p < 0.01$). The model R^2 is 0.044 using the OLS estimates, which is lower than that reported in Table11 (0.106). Similar to those from the model using the WLS estimates, the random effects in the model with OLS estimates explain a small proportion of the variation of the composite residual (0.14). The results for the test variables, however, differ based on whether I use WLS or OLS estimates. Using the OLS estimates, I fail to reject the null hypothesis that the coefficients on *POST* and *POST**PRELOCK* are significantly different from zero at conventional levels. Furthermore, I find no evidence to suggest that the sum of these coefficients is significantly different from zero using the OLS model estimates. Thus, using the OLS model estimates, I fail to find any statistically significant differences in discretionary accruals between the pre- and post-IPO periods.

For the control variables, the coefficient signs and significance for cash flows from operations, leverage, and financial condition are consistent between the models. The coefficient on *SIZE* is significantly negative when using the WLS estimates while it is insignificant when using the OLS estimates, and *LOSS* is significantly negative when using the OLS estimates while it is insignificant when using the WLS estimates. Finally, none of the time controls are statistically significant in the model using the OLS estimates whereas the coefficients for 1999 and 2002 are significantly negative in the model using the WLS estimates.

Next, I compare the H2 results from estimating equation (11) using the WLS estimates (see Table 13) to those from equation (11) estimated using the OLS estimates (untabulated). While the WLS model provides evidence in favor of H2, the overall F statistic in the model using the OLS estimates is not significant.

The conclusions drawn from testing H1 and H2 using discretionary accruals estimated from the WLS model are different from those using discretionary accruals estimated using the OLS model. Prior literature has concluded that the scaled version of this model is able to detect earnings manipulation (see e.g. Dechow, Sloan, and Sweeny 1995), while prior research has not assessed the power of the unscaled OLS specification in detecting this behavior. Future research might assess the ability of the unscaled OLS model that includes a scale proxy in detecting earnings management.

6. SUMMARY AND CONCLUSION

Previous research in the IPO setting documents high levels of discretionary accruals in the periods surrounding an IPO (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Friedlan 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). My paper contributes to this literature by furthering our understanding of what drives the incentive for earnings management for companies going public. Specifically, I test whether, on average, IPO-firm management records larger discretionary accruals in the period before the IPO (presumably to boost offering proceeds) or whether they record larger discretionary accruals in the period before management can begin trading shares (presumably to help support the stock price when the lockup expires). The results indicate that the companies that file a 10-K *before* the lockup period expires exhibit more earnings management behavior in those 10-K financial statements as compared to the prospectus financial statements. This suggests that managers might increase discretionary accruals in the newly-filed financial statements in anticipation of selling shares after the lockup expires.

To test this conclusion, I examine the relation between post-lockup IPO-firm insider trades and discretionary accruals in the annual financial statements from the period before the trading is allowed to occur. The trading analyses support this assertion; that is, I observe a significantly positive relation between the proportion of CEO and CFO shares traded following the lockup period expiration and discretionary accruals, and this relation is limited to the firms that file a new 10-K *before* the lockup period expires. Taken together, the results suggest that earnings management behavior is more prevalent in the first 10-K filed than in the offering prospectus, that it is concentrated in the firms that file this 10-K before their lockup period expires, and that it is related to insider trading.

A limitation of this study is its use of discretionary accruals as a proxy for earnings management. Because I cannot observe IPO-firm earnings management behavior directly, I estimate accounting discretion using a modified version of the Jones (1991) model, which might measure discretionary accruals with error. Additionally, the model does not discriminate between earnings management that is within GAAP and earnings management that violates GAAP. I do, however, control for misspecification in the model associated with extreme performance using a portfolio matching technique commonly used in prior literature.

The sensitivity tests raise another question about the discretionary accruals model used. I find that the results using a discretionary accrual estimate from a scaled model differ from those from an unscaled model. Previous research, however, uses a scaled model, and the literature documents that discretionary accruals estimated using a scaled model are associated with opportunistic IPO-firm behavior (DuCharme, Malatesta, and Sefcik 2004; Teoh, Wong, and Rao 1998). Future research might further examine the ability of the unscaled OLS model that includes a scale proxy to detect earnings management.

Subject to the limitations discussed above, the results of this study will help to further develop our understanding of the incentives to manage earnings around the time of an IPO. The evidence provided might be of interest to regulators and IPO investors as it can help them to identify the specific period around the offering in which the financial statements are most likely to have been affected by managements' attempts to increase earnings.

PART 1: REFERENCES

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PART 1: APPENDIX

TABLE 1: SAMPLE SELECTION – PART 1

Description	Number of Firms
IPOs on SDC[a]	459
Insufficient Compustat data	(95)
Sales = 0	(21)
Missing control variables	(20)
EDGAR filing missing	(9)
Discretionary accrual outliers	(8)
Fewer than 20 companies in two-digit SIC	(4)
Not on Compustat	(4)
Total	298

[a] Four hundred sixty-six IPOs met the sample criteria using SDC. Of these 466 firms, however, the prospectuses for two firms revealed that the firms are foreign, and the prospectus for one firm identified it as a closed-end fund. Examination of the Compustat data for four other firms indicated that the firms were either in the insurance industry (3 firms) or banking industry (1 firm).

TABLE 2: IPO DISTRIBUTION BY YEAR

Year	Offering Date	Prospectus Financial Statement Date
1999	-	193
2000	216	64
2001	49	39
2002	33	2
Total	298	298

TABLE 3: LOCKUP AGREEMENT LENGTH

Lockup Length	No Pre-Lockup Expiration 10-K	Pre-Lockup Expiration 10-K	Total
90 days	6	1	7
180 days	233	53	286
360 days	-	1	1
365 days	-	1	1
540 days	-	2	2
720 days	-	1	1
	239	59	298

TABLE 4: VARIABLE DEFINITIONS – PART 1

Variable Name		Variable Definition
AUDITOR	=	1 if Compustat (DATA149) indicates the presence of a Big N auditor for the firm-year, and 0 otherwise.
CFO	=	cash flows from operations (DATA308) at the end of the period scaled by total assets (DATA6) at the end of the period.
MKTVAL	=	equity market value, where pre-IPO market value is measured by the market value of equity at the end of the first trading day, and equity market value in the IPO year is the share price multiplied by the number of shares outstanding (DATA199*DATA25) at the end of the IPO year.
MKTBK	=	equity market value divided by the book value of assets (DATA6).
LEV	=	total debt (DATA9+DATA34) divided by total assets (DATA6), all at the end of the period.
FCOND	=	Zmijewski's (1984) financial condition index, which controls for financial distress.
LOSS	=	1 if the company had a net loss (DATA172) during the period, and 0 otherwise.
PRELOCK	=	1 if the company filed its first 10-K before its lockup agreement expired, and 0 otherwise.
SALES	=	CEO/CFO trading during the three-month period beginning when the lockup expires as a percentage of total shares outstanding.
POST	=	1 if the firm-year observation represents data from the IPO year, and 0 if the observation comes from the last annual period prior to the offering.
SIZE	=	the natural log of MKTVAL.
PADA	=	performance-adjusted discretionary accruals estimated using a cross-sectional version of the modified-Jones model.
VC	=	1 if the SDC New Issues Database identifies the IPO as having venture capital participation, and 0 otherwise.
COV	=	1 if the IPO has research coverage on the First Call Historical Database, and 0 otherwise.
SI	=	the short interest outstanding for the month of the lockup expiration as reported on Bloomberg or in the Standard and Poor's Daily Stock Price Record for NYSE and AMEX firms or as received directly from NASDAQ for the NASDAQ firms.

TABLE 4: CONTINUED

Variable Name		Variable Definition
NLEAD	=	the number of non-lead analyst recommendations on the First Call Historical Database during the period between the IPO and one month following the lockup expiration.
LEAD	=	the number of lead analyst recommendations on the First Call Historical Database during the period between the IPO and one month following the lockup expiration.
PRETRADE	=	1 if the CEO or CFO sells shares prior to the lockup expiration according to Table 1 of the Thomson Financial Insiders database, 0 otherwise.
SECONDARY	=	the proportion of shares that existing shareholders sell in the IPO (i.e., secondary shares) obtained from the SDC New Issues Database.
INSOWN	=	insider ownership obtained from either the SDC New Issues Database (when available) or from the offering prospectus.
RESID	=	predicted residuals from the first-stage equation used to perform a Hausman test for endogeneity.
ATABOVE	=	1 if non-discretionary EPS is at or above the mean First Call analyst consensus EPS forecast (obtained from the First Call Historical Database) for the IPO year, 0 otherwise.
QPRELOCK	=	1 if the company files its first 10-K before its lockup agreement expires and the company is not required to file a new 10-Q until after the lockup expiration, and 0 otherwise.

TABLE 5: DESCRIPTIVE STATISTICS FOR CONTROL VARIABLES – PART 1

Variable Name	Pre-IPO	IPO Year	Total
AUDITOR	0.97	0.96	0.97
	(1)	(1)	(1)
	[0.16]	[0.20]	[0.18]
CFO	-0.21	-0.08	-0.15
	(-0.11)	(-0.05)	(-0.07)
	[0.49]	[0.22]	[0.39]
MKTVAL	963.5	888.6	926.1
	(453.9)	(326.5)	(396.1)
	[1,721.9]	[3,583.1]	[2,808.9]
MKTBK	25.13	2.83	13.98
	(12.00)	(2.14)	(3.57)
	[36.92]	[2.54]	[28.42]
LEV	0.31	0.10	0.21
	(0.16)	(0.02)	(0.05)
	[0.42]	[0.22]	[0.35]
FCOND	-2.02	-4.34	-3.18
	(-2.59)	(-4.58)	(-3.76)
	[3.74]	[1.83]	[3.16]
LOSS	0.73	0.69	0.71
	(1)	(1)	(1)
	[0.44]	[0.46]	[0.45]

See Table 4 for variable definitions. The numbers above represent means, (medians), and [standard deviations].

TABLE 6: DESCRIPTIVE STATISTICS FOR DISCRETIONARY ACCRUALS (H1)

	Pre-IPO	**IPO Year**	**Overall**
No Pre-Lockup Expiration 10-K	0.01	**1.33**	**0.67**
(PRELOCK = 0)	(-0.02)	(0.03)	(0.01)
	[239]	[239]	[478]
Pre-Lockup Expiration 10-K	**0.40**	**2.73**	**1.57**
(PRELOCK = 1)	**(0.04)**	**(0.24)**	**(0.13)**
	[59]	[59]	[118]
Overall	0.08	**1.61**	
	(0.00)	**(0.04)**	
	[298]	[298]	

See Table 4 for variable definitions. Tests of significance for the means (medians) are based on t-tests (sign tests). Bold numbers denote significance at the 5% level or better, one-tailed. The numbers in brackets represent the number of observations.

TABLE 7: UNIVARIATE TESTS OF DIFFERENCES FOR DISCRETIONARY ACCRUALS (H1)

	Paired t-test		**Sign test**	
	Diff.	**p-value**	**Diff.**	**p-value**
IPO Year vs. pre-IPO	**1.53**	0.000	**0.04**	0.042
IPO Year vs. pre-IPO (PRELOCK = 1)	**2.33**	0.000	**0.20**	0.018
IPO Year vs. pre-IPO (PRELOCK = 0)	**1.33**	0.000	0.04	0.301

See Table 4 for variable definitions. Tests for differences in means are based on paired t-tests. Tests for differences in medians are based on sign tests. Bold numbers denote significance at the 5% level or better, two-tailed.

TABLE 8: CEO AND CFO TRADING

	n	Mean	Min	Median	Max
SALES for all firms	298	0.134%	0.000%	0.000%	5.119%
SALES for firms with CEO/CFO trading	72	0.555%	0.005%	0.267%	5.119%

See Table 4 for variable definitions.

TABLE 9: DESCRIPTIVE STATISTICS FOR DISCRETIONARY ACCRUALS (H2)

	Pre-IPO	IPO Year	Overall
No post-lockup CEO and CFO Trading	0.04	**1.83**	**0.35**
(SALES = 0)	(-0.00)	**(0.23)**	(0.02)
	[186]	[40]	[226]
Post-lockup CEO and CFO Trading	(0.10)	**4.64**	**1.15**
(SALES > 0)	(-0.08)	(0.24)	(-0.03)
	[53]	[19]	[72]
Overall	0.01	**2.73**	
	(-0.02)	**(0.24)**	
	[239]	[59]	

See Table 4 for variable definitions. Tests of significance for the means (medians) are based on t-tests (sign tests). Bold numbers denote significance at the 5% level or better, one-tailed. The numbers in brackets represent the number of observations.

TABLE 10: UNIVARIATE TESTS OF DIFFERENCES FOR DISCRETIONARY ACCRUALS (H2)

	t-test		**Wilcoxon test**	
	Diff.	**p-value**	**Diff.**	**p-value**
Trading vs. No Trading	**0.80**	0.053	-0.06	0.744
Trading vs. No Trading (Prospectus Financials)	-0.14	0.658	-0.08	0.250
Trading vs. No Trading (10-K Financials)	**2.81**	0.048	0.02	0.228

See Table 4 for variable definitions. Tests for differences in means are based on two-sample t-tests. Non-parametric tests for differences are based on Wilcoxon tests. Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 11: MULTIVARIATE TIMING MODEL

	Estimate	**Std. Error**	**Z-stat**	**p-value**
INTERCEPT	1.54	1.38	1.12	0.265
POST	0.38	0.49	0.77	0.439
PRELOCK	0.23	0.57	0.41	0.681
PRELOCK*POST	**1.32**	0.77	1.73	0.084
AUDITOR	0.83	0.92	0.90	0.369
CFO	**-1.93**	0.76	-2.55	0.011
SIZE	**-0.58**	0.15	-3.88	0.000
MKTBK	0.00	0.01	0.37	0.712
LEV	**2.21**	0.89	2.48	0.013
FCOND	**-0.39**	0.13	-3.10	0.002
LOSS	0.10	0.41	0.25	0.805
Hypothesis Tests				
H1a: POST + PRELOCK*POST = 0	**1.71**			0.029
H1b: POST = 0	0.38			0.439
n		596		
Prob > χ^2		0.000		
R^2		0.106		

See Table 4 for variable definitions. The dependent variable in this model is PADA. This model is estimated using the random effects specification. The data set includes financial data from both the pre-IPO and IPO year for each sample firm. The time control dummy variables are not reported for expositional convenience. In comparison to 2000, the omitted year, the time coefficients for 1999 ($p < 0.05$) and 2002 ($p < 0.10$) are significantly negative. Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 12: TRADING MODEL TESTS FOR ENDOGENEITY

	Model 1		Model 2	
	Estimate	p-value	Estimate	p-value
INTERCEPT	0.12	0.628	0.02	0.990
SALES			1.61	0.280
RESID			-0.76	0.624
PRELOCK	0.08	0.329	**1.53**	0.012
AUDITOR	-0.19	0.196	-0.07	0.950
CFO	-0.11	0.332	**-2.86**	0.001
SIZE	-0.00	0.919	**-0.43**	0.033
MKTBK	0.00	0.136	0.01	0.413
LEV	0.13	0.369	**3.88**	0.000
FCOND	**-0.03**	0.098	**-0.64**	0.000
LOSS	-0.01	0.853	0.11	0.828
SI	0.00	0.246		
VC	-0.05	0.402		
COV	-0.04	0.612		
NLEAD	0.00	0.608		
LEAD	-0.00	0.946		
PRETRADE	**0.39**	0.000		
SECONDARY	-0.00	0.303		
INSOWN	0.00	0.145		
n	294		294	
Prob > F	0.003		0.000	
Adjusted R^2	0.072		0.163	

See Table 4 for variable definitions. The dependent variable in Model 1 is SALES and the dependent variable in Model 2 is PADA. These models are estimated using OLS. The data for each company comes from the last annual financial statements filed before the lockup period expires. The time control dummy variables are not reported for expositional convenience. In comparison to 2000, the omitted year, the time coefficient for 1999 is significantly negative ($p < 0.05$) in Model 2. Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 13: MULTIVARIATE TRADING MODELS

	Estimate	p-value
INTERCEPT	0.04	0.983
SALES	-0.00	0.993
PRELOCK	1.04	0.131
PRELOCK*SALES	**3.14**	0.057
AUDITOR	0.28	0.746
CFO	**-2.84**	0.000
SIZE	**-0.49**	0.008
MKTBK	**0.01**	0.000
LEV	**4.16**	0.000
FCOND	**-0.67**	0.022
LOSS	0.10	0.841
<u>Hypothesis Tests</u>		
SALES + PRELOCK*SALES = 0	**3.14**	0.054
SALES	-0.00	0.993
n	298	
Prob > F	0.000	
Adjusted R^2	0.203	

See Table 4 for variable definitions. The dependent variable is PADA. This model is estimated using OLS with White (1980) standard errors. The data for each company comes from the last annual financial statements filed before the lockup period expires. The time control dummy variables are not reported for expositional convenience. In comparison to 2000, the omitted year, the time coefficient for 1999 is significantly negative ($p < 0.01$). Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 14: ANALYSTS' EARNINGS FORECASTS

	All Sample Firms (n = 298)				**Sample Firms with First Call EPS Estimates (n = 95)**			
	Model 1		**Model 2**		**Model 3**		**Model 4**	
	Estimate	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
INTERCEPT	2.45	0.404	3.19	0.276	3.13	0.541	5.49	0.260
PRELOCK	**1.75**	0.037	**1.38**	0.096	**7.97**	0.017	**7.15**	0.020
ATABOVE			**-3.47**	0.000			**-4.89**	0.000
AUDITOR	1.31	0.330	1.68	0.213	0.32	0.836	1.73	0.276
CFO	**-4.27**	0.089	-3.21	0.177	-4.38	0.400	-1.03	0.822
MKTBK	0.08	0.634	0.13	0.433	-0.09	0.727	0.05	0.835
LEV	1.63	0.355	1.67	0.362	-2.34	0.596	-2.05	0.650
FCOND	-0.12	0.696	-0.06	0.847	-0.07	0.905	0.18	0.731
SIZE	**-0.55**	0.078	**-0.57**	0.063	-0.55	0.284	-0.62	0.234
LOSS	-0.21	0.742	-0.29	0.643	0.76	0.586	0.53	0.673
Prob > F	0.006		0.000		0.073		0.002	
Adjusted R^2	0.093		0.136		0.109		0.322	

See Table 4 for variable definitions. The dependent variable in each of these models is PADA. These models are estimated using OLS with White (1980) standard errors. The data for each company comes from the period covered by the first 10-K filed after the IPO. The time control dummy variables are not reported for expositional convenience. In comparison to 2000, the omitted year, the time coefficients for 2001 and 2002 are both significantly negative in Model 1 ($p < 0.05$ and $p < 0.10$, respectively). The time coefficients for 2001, 2002, and 2003 are significantly negative in Model 2 ($p < 0.01$, $p < 0.01$, and $p < 0.05$, respectively). Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 15: TIMING OF THE 10-K FILING

	Model 1		Model 2	
	Estimate	**p-value**	**Estimate**	**p-value**
INTERCEPT	1.88	0.170	**2.51**	0.072
POST	0.43	0.375	0.38	0.438
QPRELOCK	0.37	0.565		
QPRELOCK*POST	**1.65**	0.059		
PRELOCK			0.51	0.689
PRELOCK*POST			-0.23	0.809
AUDITOR	0.61	0.503	0.53	0.559
CFO	**-1.88**	0.013	**-1.72**	0.024
SIZE	**-0.59**	0.000	**-0.67**	0.000
MKTBK	0.00	0.701	0.00	0.550
LEV	**2.14**	0.016	**2.27**	0.013
FCOND	**-0.38**	0.003	**-0.36**	0.004
LOSS	0.07	0.869	0.08	0.850
n	596		512	
Prob > χ^2	0.000		0.000	
R^2	0.110		0.105	
Hypothesis Tests				
H1a: POST + QPRELOCK*POST = 0	**2.08**	0.020	0.15	0.489
H1b: POST = 0	0.43	0.375	0.38	0.765

See Table 4 for variable definitions. The dependent variable in each of these models is PADA. These models are estimated using random effects models. The data set includes financial data from both the pre-IPO and IPO year for each sample firm. The time control dummy variables are not reported for expositional convenience. In comparison to 2000, the omitted year, the time coefficients for 1999 ($p < 0.05$ and $p < 0.05$) and 2002 ($p < 0.05$ and $p < 0.10$) are significantly negative in Model 1 and Model 2, respectively. Bold numbers denote significance at the 10% level or better, two-tailed.

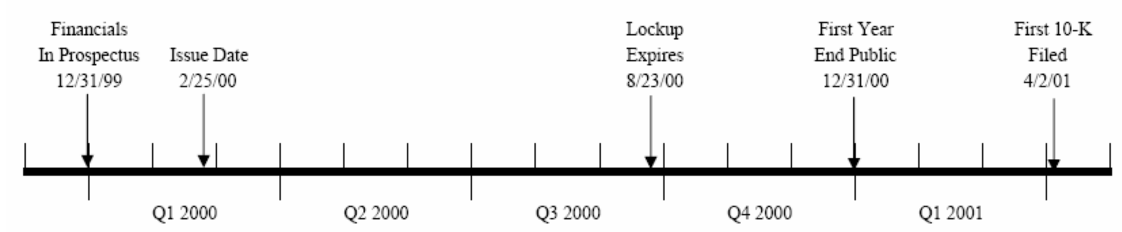

FIGURE 1: HOTELS.COM IPO AND FINANCIAL REPORTING TIMELINE
Timeline of events surrounding the Hotels.com IPO. This timeline provides an example of a firm that files its first 10-K *after* the lockup period expires. Had Hotels.com filed its first 10-K between February 25, 2000 and August 23, 2000, the company would provide an example of how a company could file its first 10-K *before* the lockup period expires.

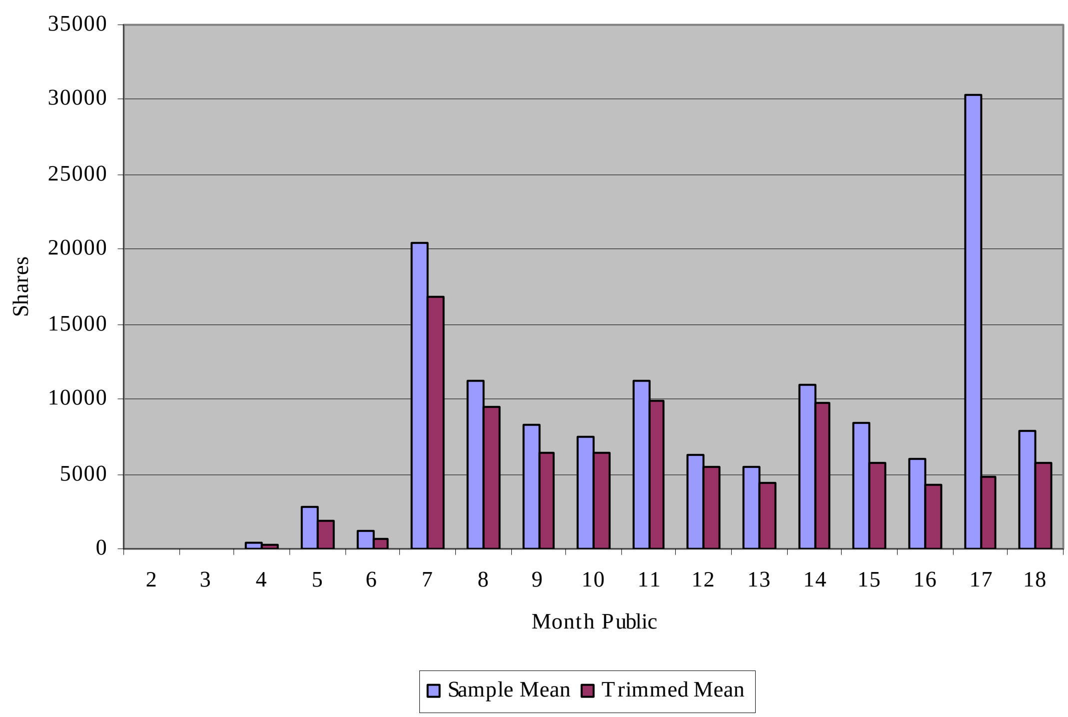

FIGURE 2: CEO AND CFO POST-IPO TRADING ACTIVITY
Graph of CEO and CFO sales during each month following the IPO. The sample consists of 298 firms that went public between January 2000 and July 30, 2002. The graph contains the mean and trimmed mean (trimmed at the 99th and 1st percentiles) number of shares sold by the CEO and CFO captured in the Thomson Financial Insiders database. The data spans from the second through the 18th month public.

PART 2: CHANGES IN IPO-FIRM EARNINGS MANAGEMENT BEHAVIOR IN THE POST-SOX PERIOD

1. INTRODUCTION

For all public companies, the Sarbanes-Oxley Act of 2002 (SOX) stiffened the penalties for certain corporate malfeasance. Presumably, these increased penalties would result in top-level executives ensuring that their company issues more accurate financial reports. Cohen, Dey, and Lys (2005) support this presumption with a finding that the level of earnings management decreased in the period following the signing of SOX. Their analysis includes the population of public companies listed on Compustat and CRSP. While their findings are certainly interesting, it is also important to understand the impact of SOX on various subsets of firms with unique, previously documented motivations to manage earnings.

In this study, I examine the subset of companies undertaking an initial public offering (IPO) to determine whether these companies have responded similarly to the general population of firms in terms of restraining their earnings management behavior in the period following the passage of SOX. Companies going public have a unique incentive to increase income around the time of the offering. Typically, very little information is publicly available for these companies prior to the preparation of their registration statement and the dissemination of their prospectus. As a result, investors place a large amount of weight on the information included in the IPO prospectus including, in particular, the financial statements for valuation purposes.

At the time of the IPO, the incentive is strong to maximize the proceeds the company receives from the offering. Similarly, selling shareholders want to maximize the proceeds they receive from the shares that they liquidate in or sometime after the offering. Thus, the company and its pre-offering owners have the incentive to communicate information that leads to a higher valuation of the company. One possible area that involves significant judgment is the preparation of the financial statements, and the flexibility afforded to managers who prepare GAAP-based financial statements provides a potential opportunity for insiders to use discretion to affect IPO-firm earnings and, in turn, the stock price.

Counteracting these incentives is the potential liability under the Securities Act of 1933 (the ’33 Act) for the company, its officers and directors, its underwriters, and its auditors. Interestingly, however, empirical evidence suggests that, on average, IPO-firm managers take advantage of the opportunity to make income-increasing judgments in the financial statements around the time of the IPO (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Friedlan 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). Furthermore, DuCharme, Malatesta, and Sefcik (2004) and Teoh, Wong, and Rao (1998) provide support for the argument that this evidence is at least partially explained by opportunistic earnings manipulation. It is this behavior, in spite of the increased liability, that makes it interesting to examine whether the SOX effect documented by Cohen, Dey, and Lys (2005) holds around the time of the IPO. This study will shed light on the question of whether IPO-firm managers, who, on average, were not previously deterred by the potential for increased liability, respond to the changes brought about in the post-SOX environment and whether they respond similarly to non-IPO-firm managers. Said differently, this paper addresses whether SOX has reigned in the opportunistic financial reporting behavior of the past for companies undertaking an IPO.

The results suggest that IPO firms experience a significant decrease in earnings management after the passage of SOX. Furthermore, the findings provide evidence that this decrease is larger than that of the population of non-IPO firms and that average post-SOX discretionary accruals for IPO firms are not statistically different from those of non-IPO firms. I also find that there are a number of significant differences between the characteristics of pre- and post-SOX IPOs. After controlling for these differences, the significant decrease in discretionary accruals in the post-SOX period remains. While these results suggest that the main findings are not sensitive to controlling for changes in the characteristics examined between the pre- and post-SOX periods, I am unable to explicitly control for the possible selection bias resulting from the decisions of certain firms to remain private after SOX.

This study contributes to the literature in a number of ways. First, this paper might be of interest to regulators by providing documentation of the effects SOX has had on the financial reporting behavior of smaller firms around the time they go public as more than 75 percent of the sample companies meet the market value definition of smaller public companies as defined by the Advisory Committee on Smaller Public Companies (2006). Furthermore, I provide evidence that the results for these companies may drive the results that I observe for the entire sample.

Second, the examination of the earnings management behavior of IPOs in the post-SOX period relative to the pre-SOX period is appealing to investors because prior literature suggests that earnings management is more prevalent in IPO firms than in the general population of public companies (Friedlan 1994) and that this behavior tends to attract shareholder lawsuits (DuCharme, Malatesta, and Sefcik 2004). To the extent that earnings management in newly public companies has decreased, this might suggest that the post-SOX environment is reducing the post-offering wealth losses to IPO investors, a possible topic for future research.

This paper is organized as follows. The next section discusses the relevant literature and discusses the research question that I address. I discuss the sample and outline the research design in the third section. Section 4 discusses the results, and Section 5 provides some additional analyses. The last section concludes with a summary.

2. BACKGROUND AND HYPOTHESIS DEVELOPMENT

IPOs and Earnings Management

The goal of any public security offering is for the company and its owners who are selling shares to receive the highest possible value in exchange for the securities sold. Empirical evidence suggests that earnings are value relevant for IPOs (DuCharme, Malatesta, and Sefcik 2001; Ritter 1984). To the extent that earnings are one of the primary measures used to value these firms, the issuer selling securities has a strong financial incentive to increase pre-issuance earnings and, thereby, maximize its share price at the time of issuance. DuCharme, Malatesta, and Sefick (2001) provide evidence that, after controlling for cash flows from operations, pre-IPO accruals (both discretionary and non-discretionary) are positively associated with the initial value of the IPO. These results suggest that total accounting earnings have incremental explanatory power above cash flows in an IPO-valuation setting and that the income effects of discretionary accruals tend to be included in the initial IPO valuation.

U.S. GAAP requires managers to use discretion when deciding between relatively conservative or aggressive applications of accounting principles. Additionally, accounting standards during the sample period provided IPO-firm managers with a unique opportunity to retroactively change accounting methods without the normal reporting requirements (APB 1971, paragraph 29).[1] Companies changing accounting methods would typically be required to report the cumulative effect of a change in accounting principle as a separate item in the calculation of net income. The exemption for IPO firms, however, allows managers to change accounting principles by simply retroactively restating all pre-issuance financial statements. Thus, in addition to the incentive to make income-increasing decisions to maximize the share price, IPO-firm managers also had the ability to do so because of the unique exceptions to the accounting rules that apply to companies undertaking an IPO. Neill and Pourciau (1995) study accounting choice in the IPO setting and conclude that the choice of income-increasing inventory costing and depreciation methods are associated with the receipt of higher offering proceeds. They do, however, note that these decisions are not necessarily problematic because these choices are within the boundaries of GAAP.

Other studies examine the prevalence of earnings management behavior at the time of the offering by estimating discretionary accruals. Friedlan (1994) finds income increasing discretionary accruals in the financial statements issued closest to but before the offering date. Consistent with Friedlan (1994), a number of studies document

[1] Statement of Financial Accounting Standards No. 154, Accounting Changes and Error Corrections, supersedes APB Opinion 20 for accounting changes and error corrections that occur in fiscal years beginning after December 15, 2005. This new standard does not include the exemption allowing offering firms to retroactively restate prior periods without the normal reporting requirements of a change in accounting principle.

significantly positive abnormal accruals and earnings in the IPO year (DuCharme, Malatesta, and Sefcik 2004; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998) and in the year before the IPO (DuCharme, Malatesta, and Sefcik 2001). Teoh, Wong, and Rao (1998) also show that, along with earnings, abnormal accruals decrease over the years subsequent to the IPO suggesting that managers increase IPO-year earnings to a level that is not sustainable in the future. Teoh, Welch, and Wong (1998) and DuCharme, Malatesta, and Sefcik (2001) find that firms with higher levels of discretionary accruals around the time of the IPO experience worse post-issuance stock market performance than IPO firms with lower levels of discretionary accruals.

One explanation for the results mentioned above is that managers “opportunistically” manipulate earnings upward during the periods surrounding IPOs and that these firms receive a higher share price for the shares issued (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Friedlan 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). The opportunism argument is that investors fail to account for the earnings manipulation by reducing the price they pay to invest. As the positive accruals reverse over the years after the IPO, however, the earnings growth of these firms is not large enough to counteract the negative effect of these reversals. As a result, the high discretionary accrual firms’ stock prices must adjust to the lower than expected growth.

Another possible explanation for the positive association between abnormal accruals and IPO offer value is that these accruals are a mechanism that managers use to communicate legitimate private information to the market (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004). As a result, investors are willing to pay a higher price to purchase the shares at the time of the offering. A third possible explanation for these results is that the models used to estimate abnormal accruals do so with error (Beneish 1998; Soffer 2001). The results of Teoh, Wong, and Rao (1998) and DuCharme, Malatesta, and Sefcik (2004) provide evidence on which of these competing explanations most likely explains the results of previous research, which I discuss in the next section.

Managerial Opportunism

Teoh, Wong, and Rao (1998) supplement their discretionary accrual tests with a comparison of the allowance for bad debts and depreciation methods between matched pairs of their IPO-sample companies and non-IPO control firms matched on industry and return on sales. Their depreciation results suggest that the majority of the IPO companies use similar methods as those of the control firms. However, they do find that when the IPO-firm depreciation methods deviate from those of the matched company, the IPO firms tend to choose income-increasing methods. With respect to the allowance for bad debts, Teoh, Wong, and Rao (1998) document that their sample companies tend to under-reserve for bad debts in the year before and the year of the offering, but that this difference disappears the year after the IPO. These results provide some evidence in favor of the opportunism explanation.

DuCharme, Malatesta, and Sefcik (2004) test the association between discretionary accruals, litigation, and settlements for IPO firms. They document a negative relation between discretionary accruals and post-issue returns and that these lower post-issue returns are associated with an increased probability of shareholder lawsuits. Furthermore, they find that higher levels of discretionary accruals (in absolute dollars) are associated with higher settlements (in absolute dollars). DuCharme, Malatesta, and Sefcik (2004, 47) conclude that “this evidence strongly supports the opportunism hypothesis.” Furthermore, these results also provide evidence that, in the IPO context, discretionary accrual models detect behavior that can result in an increased probability of litigation against the firm.

Liability for Earnings Management

Section 11 of the ’33 Act outlines the unique legal ramifications that govern the information disclosed in the prospectus. “To promote full and honest disclosure to investors, §11 loosens many of the common law elements and purposely puts fear in the hearts of potential defendants in the hope of creating a diligent pack of ‘information watchdogs’” (Palmiter 2002, 173). Section 11 allows investors to sue if the registration statement contained material misstatements or omissions as long as the investors did not have knowledge of these misstatements or omissions. Furthermore, the burden of proof in such cases rests with the defendants who might include the issuer, the executives or anyone else who signs the registration statement, the directors, the underwriters, and the accountants or other experts who allow their opinion to be included in the registration statement.

While the ’33 Act only applies to companies offering securities, the liability provisions of and rules promulgated under the Securities and Exchange Act of 1934 (the ’34 Act) apply to all public companies regardless of whether or not these companies recently issued securities. Section 10(b) of the ’34 Act and Rule 10b-5 promulgated under the ’34 Act (the “anti-fraud provisions”) provide the liability provisions for fraud. Unlike the Section 11 liability, the anti-fraud provisions place the burden of proof on the plaintiff. Other differences include the Rule 10b-5 requirements that the defendant intended to deceive, that the plaintiff relied on the information in a securities transaction, and that the reliance resulted in a loss. Thus, the threshold for liability under Section 11 of the ’33 Act for IPOs is much lower than the anti-fraud provisions for all public companies.

SOX stiffened the penalties for certain corporate malfeasance. Under Section 304 of SOX, for example, the CEO and CFO of companies required to restate financial statements due to misconduct must reimburse the issuer for (1) any bonuses, incentive-based compensation, or equity-based compensation earned and (2) any profits earned by selling securities during the 12-month period following the filing of those financial statements. Other noteworthy provisions of SOX relating to increased penalties include the requirement that the CEO and CFO provide signed certifications as to the fairness of the financial statements and an outline of the criminal penalties for providing false certifications (Sections 302 and 906), the increased statute of limitations for bringing securities fraud actions (Section 804), the addition of securities fraud to the list of criminal activities listed in the U.S. Code (Section 807), the ability of the SEC to prohibit securities laws violators from serving as directors or executive officers of an issuer (Section 1105), and the increased criminal penalties under the ’34 Act (Section 1106). It is certainly reasonable to think that these stiffer penalties might result in top-level executives ensuring that their company issues more accurate financial reports.

Cohen, Dey, and Lys (2005) provide evidence that earnings management has decreased in the post-SOX world. They find that the average level of earnings management decreased in the period following the signing of SOX for a broad sample of public companies. As such, these companies do not all have the same motivation to manage earnings. Firms undertaking an IPO, on the other hand, have unique incentives around the time of the offering to use discretion to record income increasing adjustments, and prior literature suggests that managers act on these incentives (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Friedlan 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998).

As a result, I examine the subset of IPO companies to understand how IPO-firm earnings management compares to that of the general population of firms in the post-SOX period. In spite of the significantly more stringent liability provisions for firms undertaking an offering, the unique incentive to increase income around the time of the offering appears to lead IPO-firm managers to record opportunistic accruals in this setting (DuCharme, Malatesta, and Sefcik 2004; Teoh, Wong, and Rao 1998). This paper addresses whether the changes in the post-SOX environment have restrained the opportunistic financial reporting behavior for companies undertaking an IPO. That is, this study examines whether IPO-firm managers, who, on average, were not previously deterred by the potential for increased liability, respond to the changes brought about in the post-SOX period and whether these managers respond similarly to non-IPO-firm managers.

3. RESEARCH DESIGN

Discretionary Accruals Estimation

I use a modified version of the Jones (1991) model to estimate discretionary accruals, my proxy for earnings management. The Jones (1991) model suggests that the expected level of accruals is a function of the change in revenues and the level of property, plant, and equipment. I calculate total accruals for all Compustat firms using equation (2) from Hribar and Collins (2002, 109) as follows:

$$TACC_{j,t} = EBXI_{j,t} - CFOPS_{j,t}, \quad (1)$$

where j and t are firm and time subscripts, respectively, and $TACC$ equals total accruals, $EBXI$ equals income before extraordinary items from the statement of cash flows (DATA123), $CFOPS$ equals net cash flows from operating activities adjusted for extraordinary items and discontinued operations (DATA308 – DATA124).

I estimate non-discretionary accruals using the cross-sectional method outlined in DeFond and Jiambalvo (1994) and an accounts receivable adjustment as outlined in DeFond and Park (1997). This method groups all companies with sufficient Compustat data with other companies from the same two-digit SIC code in the years in which earnings management is hypothesized and estimates non-discretionary accruals using the following equation:[2]

[2] Two-digit SIC codes group old- and new-economy firms together. To subdivide companies into old- and new-economy groups, I use the new-economy definitions from Murphy (2003). This affects the two-digit SIC code groups listed below. Companies in the four-digit SIC code groups included in parentheses are considered new-economy companies, while all other four-digit SIC code groups are not. The affected groups are as follows: 35 (3570, 3571, 3572, 3576, and 3577), 36 (3661 and 3674), 48 (4812 and 4813), 50 (5045), 59 (5961), and 73 (7370, 7371, 7372, and 7373).

$$\frac{TACC_{j,t}}{A_{j,t-1}} = \beta_0 + \beta_1 \frac{1}{A_{j,t-1}} + \beta_2 \frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{A_{j,t-1}} + \beta_3 \frac{PPE_{j,t}}{A_{j,t-1}} + \varepsilon_{j,t}, \qquad (2)$$

where *j* and *t* are firm and time subscripts, respectively, and *TACC* equals total accruals from equation (1), *A* equals total assets (DATA6), $\Delta Sales$ equals the change in revenues (DATA12) from the prior period, ΔAR equals the change in receivables (DATA2) from the prior period, and *PPE* equals the level of gross property, plant, and equipment (DATA7).[3] I estimate the model for industries with at least 20 observations.

Discretionary accruals for the sample firms are then calculated using the estimates from equation (2):

$$DA_{j,t} = \frac{TACC_{j,t}}{A_{j,t-1}} - \hat{\beta}_0 - \hat{\beta}_1 \frac{1}{A_{j,t-1}} - \hat{\beta}_2 \frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{A_{j,t-1}} - \hat{\beta}_3 \frac{PPE_{j,t}}{A_{j,t-1}}, \qquad (3)$$

where *j* and *t* are firm and time subscripts, respectively, and *DA* equals discretionary accruals and each $\hat{\beta}$ represents the industry-specific coefficient estimated from equation (2). Thus, $DA_{j,t}$ is basically the residual from equation (2).

Finally, I adjust the discretionary accruals calculated in equation (3) using a portfolio approach employed in prior literature (e.g., Ashbaugh, LaFond, and Mayhew 2003; Kasznik 1999; and Klein 2002). Kothari, Leone, and Wasley (2005) demonstrate that adjusting for the discretionary accruals of similarly performing firms can correct for the misspecification of discretionary accrual models previously documented for companies with extreme performance (see e.g., Dechow, Sloan, and Sweeney 1995). I adjust for performance by first assigning the sample companies to a return on sales (ROS)

[3] Following Teoh, Welch, and Wong (1998) and Teoh, Wong, and Rao (1998), I estimate equation (2) by excluding all sample firms and all firms that conducted an SEO in the respective year from the estimation sample. Given that Compustat now includes pre-IPO data, I also exclude IPO companies during the pre-IPO period.

decile. Each decile contains firms from the same industry with sufficient Compustat data to calculate discretionary accruals and ROS. I create the deciles by sorting the firms in each industry based on their ROS value in a given year and assigning them each to one of 10 groups based on their ROS ranking. Finally, I calculate the median discretionary accruals for each decile (excluding the sample companies), and I subtract this value from the sample firms' discretionary accrual estimate. While Kothari, Leone, and Wasley (2005) use return on assets (ROA) to measure performance, I use ROS because, as Teoh, Wong, and Rao (1998) highlight, ROA in the IPO year should be lower because the IPO proceeds would immediately increase assets, whereas IPO proceeds are less likely to have an immediate impact on sales. The performance adjusted discretionary accrual is calculated as follows:

$$PADA_{j,t} = DA_{j,t} - MedianDA_{ROS\ Decile,t}, \tag{4}$$

where j and t are firm and time subscripts, respectively, and *PADA* equals performance-adjusted discretionary accruals, $DA_{j,t}$ equals discretionary accruals calculated using equation (3) for the sample firm, and $MedianDA_{ROS\ Decile,t}$ equals the median discretionary accrual for the given ROS decile.

Sample Selection

The sample of IPO firms comes from the Securities Data Corporation (SDC) New Issues Database, which includes firm-commitment IPOs. I include companies that completed common stock IPOs during the period between 2000 and 2004. The sample excludes ADRs, REITs (SIC code 6798), partnerships, closed-end funds, unit offers, financial institutions (SIC codes 6000-6199), and insurance companies (SIC codes 6300-6411). I exclude financial institutions and insurance companies because the accrual composition for these firms is unique to these industries. These restrictions result in 683 IPOs. I further restrict the sample to include firms with sufficient financial data available on the 2004 Compustat Industrial, Full Coverage, and Research files. Seventy-four companies do not have Compustat data for some of the control variables, and 30 firms have insufficient data to estimate discretionary accruals. Twenty-four companies have zero sales, which results in an undefined ROS value for performance matching. The IPO-year data for 13 companies is 2005, and this data is not available on the 2004 Compustat update. Twelve companies are not on Compustat, and 11 companies are missing EDGAR filings. Finally, I exclude eight firms because they have discretionary accrual values greater than (less than) the 99^{th} (1^{st}) percentile and were not excluded for other reasons, and four firms from industries with fewer than 20 observations. The final sample includes 507 companies. Table 1 provides a summary of the sample selection process.[4]

The Effect of the post-SOX Environment on IPO-firm Earnings Management

This study will examine whether, on average, IPO-firm managers who were not previously deterred by the potential for increased liability responded to the changes brought about in the post-SOX period and whether these managers respond similarly to non-IPO-firm managers. In Part 1 of this dissertation, I find that IPO-firm earnings management is confined to the IPO year and, furthermore, that this behavior is concentrated in the firms that file a new 10-K *before* the lockup period expires. This finding is consistent with managers manipulating earnings in the new financial information they provide to the market in anticipation of the lockup period expiring to

[4] All Tables and Figures for Part 2 are included under the heading Part 2: Appendix.

help increase the share price before the managers sell their shares.[5] Therefore, I estimate equation (5) using OLS for IPO-year financial data to address this research question:

$$\begin{aligned} PADA = {} & \beta_0 + \beta_1 SOX + \beta_2 PRELOCK + \beta_3 PRELOCK * SOX + \beta_4 AUDITOR \\ & + \beta_5 CFO + \beta_6 SIZE + \beta_7 MKTBK + \beta_8 LEV + \beta_9 FCOND + \beta_{10} LOSS \\ & + \sum \alpha_j TimeControls + \varepsilon, \end{aligned} \qquad (5)$$

The dependent variable, *PADA*, is the performance-adjusted discretionary accrual estimate from equation (4). The test variable, *SOX*, equals “1” if the IPO firm files its first 10-K after July 30, 2002, the date that SOX was signed into law, and “0” otherwise. *PRELOCK* is a measure of whether or not the company provides new annual financial data to the market before the lockup agreement expires. It equals “1” if the company files its first 10-K *before* its lockup agreement expires, and “0” otherwise. I then interact this variable with *SOX* to allow the relation between *SOX* and *PADA* to change based on whether the 10-K financial statements are filed *before* or *after* insiders can begin trading.

I expect a negative relation between the presence of large auditors and *PADA*. DeAngelo (1981) argues that audit firm size and audit quality are positively related. Heninger (2001) finds a positive relation between income increasing abnormal accruals and the probability that an auditor is sued. Because SOX increased the scrutiny on audit firms, auditors would presumably require their clients to make more adjustments for discretionary accruals after the passage of SOX. I measure auditor size using an indicator variable, *AUDITOR*, which equals “1” if Compustat (DATA149) indicates the presence

[5] Lockups are agreements between the lead underwriter and company insiders that restrict these insiders from selling shares for a certain period of time following the offering. This period is usually 180 days and almost all offerings have a lockup arrangement.

of a Big N auditor for the firm-year, and "0" otherwise. I expect a negative coefficient on *AUDITOR*.[6]

The other control variables included in equation (5) are factors commonly controlled for in recent earnings management studies (e.g., Ashbaugh, LaFond, and Mayhew 2003; Butler, Leone, and Willenborg 2004; Menon and Williams 2004). *CFO* represents cash flows from operations (DATA308) at the end of the period scaled by total assets (DATA6) at the end of the period. *SIZE* equals the natural log of market value (the share price multiplied by the number of shares outstanding [DATA199*DATA25]) at the end of the IPO year. *MKTBK* is the market value of equity divided by the book value of assets (DATA6), all at the end of the period. *LEV* is total debt (DATA9+DATA34) divided by total assets (DATA6), all at the end of the period. *FCOND* is Zmijewski's (1984) financial condition index, which controls for financial distress.[7] *LOSS* equals "1" if the company had a net loss (DATA172) during the period, and "0" otherwise. I also include time control indicator variables, *TimeControls*, to control for possible systematic differences in discretionary accruals during each period.

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[6] One example of how SOX increased scrutiny for auditors is that it established the Public Company Accounting Oversight Board (PCAOB), which replaces the peer review process that was used for quality control in the past.

[7] This measure is calculated using the following variables and coefficient estimates that Zmijewski (1984) obtained by using a weighted exogenous sample maximum likelihood method to estimate his probit model:

$$-4.803 - 3.6*(EBXI/ASSETS) + 5.4*(LEV) - 0.1*(CASSTS/CLIABS),$$

where *EBXI* equals income before extraordinary items from the statement of cash flows (DATA123), *ASSETS* equals total assets (DATA6), *LEV* equals total debt (DATA9+DATA34) divided by total assets (DATA6), *CASSTS* equals total current assets (DATA4), and *CLIABS* equals total current liabilities (DATA5), all measured at the end of the period.

4. RESULTS

Descriptive Statistics

Table 2 presents the distribution of sample firms by year. Panel A shows that the most active sample year for IPOs is 2000, when 258 companies went public. Fifty-one pre-SOX IPOs completed an offering in 2001, and four post-SOX IPOs went public during 2001. Forty-eight companies went public in 2002, 43 in 2003, and 103 in 2004. Panel B indicates that, in terms of the 10-K dates, 233 include 2000 data, 71 include 2001 data, five include 2002 data filed before July 30, 2002, and 46 include 2002 data filed after July 30, 2002. Forty-five companies include 2003 data, and 107 include 2004 data.

Table 3 provides the variable definitions, and Table 4 presents descriptive statistics for the control variables in the IPO year. Virtually all of the sample companies have a Big N auditor in both the pre- and post-SOX periods. On average, cash flows from operations represent -11 percent of assets before SOX, which improves to two percent after SOX. The median market value at the end of the first fiscal year in the pre-SOX period is $351.7 million, and the median market value in the post-SOX period is $352.4 million. The mean market to book ratio is 3.11 and 2.65 for these respective periods. Total debt represents nine percent of assets in the pre-SOX period and 18 percent of assets for the post-SOX offerings, and the average probability of bankruptcy is low in both periods. Finally, 72 percent and 41 percent of the sample had a net loss in the pre- and post-SOX periods, respectively.

Table 5 presents the summary statistics for the dependent variable, performance-adjusted discretionary accruals, from the first 10-K filed after the offering. Overall, average pre-SOX discretionary accruals are significantly positive for the entire sample and for each group of companies defined based on when they file their first 10-K relative to the expiration of the lockup ($p < 0.01$ in each case). The post-SOX discretionary accruals are not statistically significant overall or for either group of *PRELOCK* companies. The results in terms of the medians are generally consistent with those of the means except that the median for the companies that file their first 10-K *after* the lockup expires is not statistically significant.

Table 6 presents univariate tests of differences between the pre- and post-SOX discretionary accruals presented in Table 5. The two-sample t-tests and the non-parametric Wilcoxon tests indicate that discretionary accruals have decreased significantly. These findings suggest that IPO-year discretionary accruals are significantly lower in the post-SOX period than those in the pre-SOX period, providing preliminary support that IPO-firm earnings management has decreased in the post-SOX environment.

Multivariate Tests

The test variables in equation (5) differ depending on whether the firm files its first 10-K *before* or *after* the lockup expiration. For the companies that file their first 10-K *after* the lockup expiration, the coefficient on the *SOX* variable provides an estimate of the difference in average discretionary accruals between the pre- and post-SOX periods. The coefficient on *SOX* + *PRELOCK**SOX* provides an estimate of the difference in average discretionary accruals between the pre- and post-SOX periods for the companies that filed their first 10-K *before* the lockup expiration. Significantly negative coefficient estimates for these relations would indicate that, on average, IPO-firm earnings management has decreased in the post-SOX period after controlling for other determinants of discretionary accruals. An insignificant coefficient, on the other hand, would suggest that average discretionary accruals are not significantly different between the pre- and post-SOX periods.

Table 7 presents the results from estimating equation (5) using OLS. In equation (5) I am interested in testing the SOX policy effect after controlling for any non-policy-related differences in earnings management behavior in each individual year. The problem with including the *TimeControls* along with the *SOX* variable, however, is that these variables are defined based on the date of the financial data included in the model. Thus, by definition, the *SOX* variable and the *TimeControls* are highly correlated. This high correlation can lead to inflated standard errors and, in turn, lower t-statistics. Omitting the *TimeControls*, on the other hand, could lead to omitted variables bias. As a result, I estimate the model including and excluding the *TimeControls*, and I report the results side-by-side in Table 7.

The overall F statistic for each model is significant ($p < 0.01$) and the model adjusted R^2 is 0.06 for Model 1 and 0.054 for Model 2. In general, the coefficients in the two models are similar in terms of signs and significance. More importantly, the coefficient on the *SOX* variable is very similar in both models, and this coefficient is significantly negative in Model 2, but insignificant in Model 1. Given the general consistency in coefficients in each of the models, I omit the *TimeControls* throughout the rest of the paper.

As discussed above, the coefficient on *SOX* is significantly negative ($p < 0.10$). Furthermore, the estimate of *SOX* + *SOX*PRELOCK* is significantly negative ($p < 0.10$). These findings suggest that, compared to the pre-SOX period, average discretionary accruals are significantly lower for IPO firms in the post-SOX period, regardless of whether the company filed its first 10-K *before* or *after* the lockup expiration. Taken together with the univariate tests discussed above, the results provide evidence that IPO-firm earnings management has decreased in the post-SOX environment. Furthermore, the findings indicate that this change has resulted in a reduction in the magnitude of average IPO-firm discretionary accruals from being significantly positive to being indistinguishable from zero.

The results for the control variables suggest that *CFO* and *SIZE* are negatively related to *PADA* ($p < 0.05$ and $p < 0.10$, respectively). I find no significant relation between *PADA* and the presence of a Big N auditor, the market to book ratio, leverage, financial condition, or the presence of a loss.

5. ADDITIONAL ANALYSES

IPO Composition

The results from estimating equation (5) are consistent with a change in earnings management behavior for the companies that file their first 10-K after the passage of SOX. An alternative explanation for these findings is that the companies choosing to go public in the pre-SOX period were more likely to manage earnings, and that these types of companies are no longer going public. While employing a Heckman selection model would control for this alternative explanation (and the potential selection bias due to omitting this control), I am unable to run such a model. Estimating a Heckman model requires data for firms that go public and those that do not. Given the lack of available data for private firms that decide to remain private, I cannot estimate this model.

Despite this limitation, I still attempt to address this issue by first testing for differences in a number of IPO-firm characteristics for offerings before and after SOX. I then compare the industry composition of the IPO sample between these two periods. Finally, I add each measure that is significantly different between the pre- and post-SOX periods to equation (5), and I re-estimate the model.

Table 8 presents the means and medians for a number of characteristics. First, I test for significant differences in a number of different size measures. I find no statistically significant differences in mean total assets, sales, or market value at the end of the IPO year. With the exception of sales ($p < 0.01$), the results of the non-parametric tests are consistent with those of the t-tests. Net income (market value at the end of the first trading day), on the other hand, demonstrates a significant increase (decrease) between the periods ($p < 0.01$ except for the t-test for net income for which $p < 0.05$).

The next group of variables is related to financing. As shown in Table 8, (and also in Table 4) the mean (median) debt to assets ratio (*LEV*) for pre-SOX offerings is nine percent (two percent) which increases to 18 percent (three percent). Both the t-test and Wilcoxon test suggest that these increases are statistically significant ($p < 0.01$ and $p < 0.05$, respectively). The changes in the offering price and the number of shares offered are not significantly different between the periods. Finally, underpricing drastically decreased from an average (median) of 49 percent (25 percent) of the offering price in the pre-SOX period to 11 percent (nine percent) in the post-SOX period (both significant at $p < 0.01$).

In terms of operating performance, I examine sales growth, industry-adjusted return on sales, and industry-adjusted return on assets. The results suggest that significant differences exist between the periods for each of these three measures of performance (except for the mean for *INDROS*). Because all of the distributions are left skewed, I focus on the medians for these measures.[8] The trend for sales growth is negative, with a significant decrease from 90 percent to 30 percent ($p < 0.01$). In the pre-SOX period, the median firm has a net loss that represents 39 percent of sales and five percent of assets, and both measures show significant increases ($p < 0.01$) in the post-SOX period.

The next category includes exchange listing statistics. The first three items show the percentages of firms that went public on the NYSE, AMEX, and NASDAQ. The proportions show a significant increase for listings on the NYSE from 10 percent to 23 percent ($p < 0.01$) between the pre- and post-SOX periods, respectively, and a significant decrease in the proportion of NASDAQ IPOs from 88 percent to 74 percent, respectively ($p < 0.01$). AMEX listings remained constant at two percent during both periods.

The final grouping includes a number of other characteristics. The average (median) age of the offerings at the time of the IPO in the pre-SOX period are approximately 11 (six) years, and these values increase to approximately 18 (eight) years ($p < 0.01$ for both parametric and non-parametric tests). I measure IPO age as the number of years between the IPO founding date and the year of the offering using the Field-Ritter dataset of company founding dates when available (Field and Karpoff 2002;

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[8] The unusual values for both the ROS and SGROW measures are due to companies that have very low sales numbers in the denominator of each calculation.

Loughran and Ritter 2004). This data is available on Jay Ritter's website (Ritter). For the firms not included in Field-Karpoff dataset, I obtain the founding dates from Dun & Bradstreet's *Million Dollar Directory*, the company's website, or the offering prospectus. The mean (median) level of institutional ownership [obtained from the Thomson Financial CDA/Spectrum Institutional (13f) Holdings database] also increases from 29 percent (26 percent) before SOX to 39 percent (34 percent) after SOX ($p < 0.01$ for both the t-test and Wilcoxon test). The proportion of firms with reputable underwriters (74 percent in the pre-SOX period and 77 percent in the post-SOX period) is not significantly different between the periods. To control for underwriter reputation, I use the Loughran and Ritter (2004) underwriter rankings list that has been updated through 2004 and is available on Jay Ritter's website (Ritter).[9] The proportion of venture capital backed firms (66 percent in the pre-SOX period and 55 percent in the post-SOX period) and the proportion of firms with analyst research coverage on the First Call Historical Database (76 percent in the pre-SOX period and 92 percent in the post-SOX period) are significantly different between the two periods ($p < 0.01$ in both cases).

Table 9, Panel A, examines the change in industry composition between the periods before and after SOX using industry groupings from Teoh, Wong, and Rao (1998). For each period, the table includes (1) the number of IPO firms in each industry group and (2) the number of these firms as a percentage of the total number of either pre- or post-SOX offerings. The categories with large changes include the Electronic Equipment and Computer Equipment & Services categories. Upon further examination

[9] This list includes the following underwriters: ABN Amro, Banc of America Securities, Bear Stearns, Citigroup, CIBC, Credit Suisse First Boston, Deutsche Bank, Goldman Sachs, HSBC Securities, JP Morgan, Lazard, Lehman Brothers, Merrill Lynch, Morgan Stanley, Salomon Smith Barney, Sandler O'Neill Partners, Thomas Weisel Partners LLC, and UBS.

of the data, the changes in these industries occur mainly in the New Economy two-digit SIC code groups.

Table 9, Panel B, confirms this finding and indicates that the percentage of New Economy offerings decreased from 44 percent in the pre-SOX period to 28 percent in the post-SOX period. Untabulated Pearson and Likelihood-ratio χ^2 tests of independence between the categories suggest that the type of firm is not independent of the period of the offering ($p < 0.01$ for both tests).

The results of this analysis suggest that pre- and post-SOX IPO firms differ on a number of dimensions including net income, market value at the end of the first trading day, leverage, underpricing, sales growth, industry adjusted return on assets, industry adjusted return on sales, listing on the NYSE, listing on the NASDAQ, firm age at the time of the IPO, institutional ownership, venture capital backing, and analyst research coverage. Furthermore, the mix of companies between the New and Old Economy grouping has also changed. I include each of these variables in equation (5), and I re-estimate the model.[10] The untabulated results from this analysis suggest that, after controlling for the changes in the composition of firms going public after SOX, I continue to detect a significant decrease in average IPO-firm discretionary accruals in the post-SOX period. This result holds for the *PRELOCK* = 0 firms ($p < 0.10$) and the *PRELOCK* = 1 firms ($p < 0.05$). Of the additional variables that I include in equation (5), only industry adjusted return on assets and venture capital backing are statistically significant, both of which are positive ($p < 0.05$). While these findings suggest that the

[10] Before running this analysis, I take the natural log of the market value after the first trading day and also the firm age at the time of the IPO.

main results are not sensitive to controlling for changes in the characteristics examined between the pre- and post-SOX periods, this does not eliminate the possibility that the results are driven by selection bias. Future research might assess this alternative explanation more formally.

Comparison to the Population

In this section I test whether the results that I document for IPOs in Table 7 differ from those of non-IPO firms in the overall population. To test this question I run the following model using panel methods on the population of Compustat firms with sufficient data for the years 2000 – 2004:

$$\begin{aligned}|PADA| = {} & \gamma_0 + \gamma_1 SOX + \gamma_2 IPO + \gamma_3 IPO*SOX + \gamma_4 AUDITOR + \gamma_5 LISTED \\ & + \gamma_6 CFO + \gamma_7 SIZE + \gamma_8 MKTBK + \gamma_9 LEV + \gamma_{10} FCOND + \gamma_{11} LOSS + \xi,\end{aligned} \quad (6)$$

where the dependent variable is the absolute value of *PADA* because some firms in the population will have the motivation to record income-increasing accruals while other firms will have the motivation to record income-decreasing accruals. Measuring the dependent variable as the absolute value of *PADA* captures the magnitude of discretionary accruals, regardless of whether they are income-increasing or income-decreasing accruals.

The independent variables are as previously defined in equation (5) except for *IPO* and *LISTED*. *IPO* is an indicator variable that equals “1” if the firm-year observation comes from the first 10-K filed for IPO firms, and “0” otherwise. *LISTED* equals “1” if the company was listed on NYSE or AMEX, or quoted on NASDAQ, and “0” otherwise. Shortly after the signing of SOX, the major U.S. stock exchanges adopted more stringent governance rules as a part of their listing standards (see NYSE 2003;

NASD 2003). These changes might have an effect on the earnings management of listed firms above that of SOX. As a result, I control for whether Compustat identifies the respective firm as being listed on NYSE or AMEX, or quoted on NASDAQ. I expect a negative relation between *LISTED* and *PADA*.

Equation (6) also includes an interaction between *SOX* and *IPO*. The γ_2 coefficient measures the pre-SOX difference in the magnitude of average discretionary accruals between IPO and non-IPO firms. The estimate of the $\gamma_2 + \gamma_3$ coefficient measures the post-SOX difference in the magnitude of average discretionary accruals between IPO and non-IPO firms. As discussed earlier, average discretionary accruals for IPO firms tend to be positive around the time of the offering in the pre-SOX period (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Friedlan 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). Furthermore, Friedlan (1994) documents that earnings management is more prevalent for IPOs than for non-IPOs. Therefore, I expect to observe a positive coefficient for γ_2.

Given the more prevalent earnings management behavior for IPOs, these firms would have more room to improve in the post-SOX period relative to their non-IPO peers. Failure to reject the null hypothesis that $\gamma_2 + \gamma_3 = 0$ would suggest that the magnitude of IPO-firm discretionary accruals is not significantly different from that of non-IPO firms in the period after SOX. On the other hand, the evidence of positive discretionary accruals for IPO companies is consistent with increased potential liability (e.g., under the ’33 Act) not having an impact on the behavior of IPO-firm management. In the post-SOX period, IPO firms still face the same incentives to manage earnings around the time of the offering, and the changes that have occurred might not bear high enough potential costs to restrain this behavior. Evidence that $\gamma_2 + \gamma_3 > 0$ would suggest that the magnitude of discretionary accruals remains significantly higher for IPO companies relative to non-IPO companies in the post-SOX period. Finally, it is possible that, on average, IPO firms become more conservative than non-IPO firms, resulting in a smaller magnitude of post-SOX discretionary accruals than those of non-IPO firms. A significantly negative joint test for $\gamma_2 + \gamma_3$ would support this argument.

Table 10 presents the results from estimating equation (6) using a fixed effects model.[11] The overall model is highly significant ($p < 0.01$) and the overall R^2 is 0.015. The coefficient estimate for γ_2 is positive and statistically significant ($p < 0.01$), providing evidence consistent with Friedlan (1994) that average discretionary accruals for IPO-firms are significantly higher, at least in the period before SOX. Turning to the post-SOX period, I fail to reject the null hypothesis that $\gamma_2 + \gamma_3 = 0$. This result indicates that, in the post-SOX period, average discretionary accruals for IPO firms are not significantly different from those for non-IPO firms. Thus, the difference in the magnitude of discretionary accruals from the pre-SOX period disappears in the post-SOX period, suggesting that post-SOX IPO-firm discretionary accruals are in line with those of non-IPO firms.

[11] The Hausman (1978) test (untabulated) provides strong evidence that the differences between the estimated coefficients for equation (6) using the fixed effects estimator and the random effects estimator are systematic ($p < 0.01$). Furthermore, the test that the fixed effects are jointly equal to zero is highly significant ($p < 0.01$) and the estimated correlation between the fixed effects and the fitted values is -0.44. As a result, I estimate equation (6) using a fixed effects model.

Interestingly, the coefficient on γ_1 is not significantly different from zero suggesting no difference in the magnitude of discretionary accruals between the pre- and post-SOX period for non-IPO firms. These results are in contrast to those of Cohen, Dey, and Lys (2005) who find that earnings management behavior decreased in the post-SOX period. In untabulated results, I estimate equation (6) dropping the *IPO* variable and the interaction between *SOX* and *IPO*. The coefficient on the *SOX* variable in this model is negative and statistically significant ($p < 0.05$), consistent with Cohen, Dey, and Lys (2005).

The control variable results suggest a significantly negative relation between (1) cash flows from operations and (2) financial condition and the absolute value of discretionary accruals ($p < 0.01$ for both relations). I also find a significantly positive relation between the magnitude of discretionary accruals and (1) *SIZE* and (2) leverage ($p < 0.01$ for both relations). Finally, the results suggest that *AUDITOR*, *LISTED*, *MKTBK*, and *LOSS* are not significantly related to *PADA*.

Alternative Definitions of the Pre- and Post- Periods

In the main analyses, I test for differences in discretionary accruals for IPO firms in the pre- and post-SOX periods. I define these periods based on the date that the sample companies file their first 10-K relative to the SOX enactment date of July 30, 2002. In this section, I test the sensitivity of the results documented above using an alternative definition to see whether the decrease in discretionary accruals occurred before the signing of SOX. Specifically, I discard all companies that file their first 10-K between October 16, 2001 and July 30, 2002. On October 16, 2001, Enron announced that it was taking a $1.01 billion charge in the company's third quarter financial statements, quickly setting off a chain of events in the business world that resulted in the passage of SOX. It is possible that the increased attention to the problems at Enron might have brought about a change in behavior before July 30, 2002.

Fifty firms filed their first 10-K during this period. When I re-estimate equation (5) excluding these 50 companies, the model remains highly significant ($p < 0.01$) and the adjusted R^2 remains close to 0.06 (untabulated). Both the *SOX* base effect and the *PRELOCK*SOX* interaction term are statistically significant ($p < 0.05$ and $p < 0.10$) and negative, suggesting that the original equation (5) results are not sensitive to the inclusion or exclusion of the post-Enron and pre-SOX offerings. Thus, the fifty post-Enron and pre-SOX offerings are not driving my results.

SIZE Subsets

Some have expressed concerns about the effects of SOX on small businesses (see e.g., AEA 2005). As a result of these concerns, the SEC formed an advisory committee to study the effect of SOX on smaller public companies (SEC 2004), and the SEC delayed the effective date for the internal control requirements under Section 404 for smaller public companies in both March and September 2005. More recently, the Advisory Committee on Smaller Public Companies (2006) recommended that the SEC fully exempt certain microcap (market capitalization < $128.2 million) companies from the Section 404 provisions entirely, and that the SEC drop the auditor attestation requirement for certain smallcap (market capitalization $128.2 - $787.1 million) companies. While the SEC ultimately rejected these recommendations (see SEC 2006), the committee's size definitions provide meaningful size groupings for me to use in this analysis.

One hundred seven sample companies are microcap companies. Of these 107 companies, 80 are pre-SOX offerings and 27 are post-SOX offerings. Two hundred eighty companies are smallcap companies, 157 of which are pre-SOX IPO firms and 123 are post-SOX IPO firms. Finally, 120 sample firms (72 pre-SOX and 48 post-SOX) are larger companies.

Based on the size definitions discussed above, I estimate equation (5) with some modifications. First, I include indicator variables for the microcap and smallcap companies, and I interact these with the *SOX* variable to test whether the relation between *SOX* and *PADA* differs based on the size of the IPO firms. Second, to eliminate the need for a three-way interaction term, I exclude the *PRELOCK* variable and the interaction between *SOX* and *PRELOCK*. This is reasonable given that the coefficients on these terms are not significant in any of the previous specifications.[12]

The overall model is significant ($p < 0.01$) with an adjusted R^2 of 0.07 (untabulated). While the *SOX* base effect and both of the interaction terms are not individually significant, the sum of the *SOX* base effect and the interaction between *SOX* and the microcap indicator variable is significantly negative ($p < 0.10$). This suggests that that average discretionary accruals are significantly lower in the post-SOX period as compared to the pre-SOX period for the smallest IPOs. For smallcap and larger public company offerings, however, I find no significant difference in pre- and post-SOX discretionary accruals. The results of this analysis suggest that the significant decrease

[12] Ideally, I would estimate equation (5) separately for the microcap, smallcap, and large public companies. Only one of these models is more than marginally significant suggesting a possible lack of power. Thus, I pool the observations and test the differential size effects using interaction terms.

that I observe for post-SOX discretionary accruals is driven by the companies with an IPO year market capitalization of $128.2 million or less.

Discretionary Accruals Model Specification

In Part 1 of this dissertation, I discuss two alternative versions of the modified Jones model – a scaled WLS model and an unscaled OLS model that includes a scale proxy variable. The difference between these two models is that the former scales all of the variables in the model by lagged assets and the latter does not scale the variables but, instead, includes lagged assets as a control variable in the model. In this section, I re-estimate equation (5) using the unscaled OLS model to estimate the dependent variable, *PADA*. The remainder of the estimation process (i.e., in terms of the variable definitions, performance adjustment, independent variables, and models used) is identical to that described in the Research Design and Results sections above.

When I re-estimate equation (5) using the unscaled *PADA* measure, the untabulated results suggest that the overall model is only marginally significant ($p = 0.096$), and the adjusted R^2 is 0.031. I fail to reject the null hypothesis that the coefficients on *SOX* and *SOX*PRELOCK* are significantly different from zero at conventional levels. Furthermore, I find no evidence that the joint test of the sum of these coefficients is different from zero. Thus, using the unscaled OLS model estimates, I fail to find any statistically significant differences in average discretionary accruals between the pre- and post-SOX periods.

Consistent with the results in Table 7 for equation (5), I find a significantly negative relation between cash flows from operations and *PADA*. In contrast to the equation (5) results using the scaled *PADA* measure, I find a significantly negative relation between *PADA* and (1) *FCOND* and (2) *LOSS* ($p < 0.05$ in both cases) and a significantly positive relation between *LEV* and *PADA* ($p < 0.10$) using the unscaled measure. Finally, *SIZE* is insignificant using the unscaled measure, while it is significantly negative using the scaled measure in equation (5).

In summary, the conclusions drawn from testing the differences in pre- and post-SOX discretionary accruals for IPO firms depend on whether I estimate *PADA* using a scaled or unscaled measure. Prior literature has concluded that the scaled version of this model is able to detect earnings manipulation (see e.g. Dechow, Sloan, and Sweeny 1995), while prior research has not assessed the power of the unscaled OLS specification in detecting this behavior. Future research might assess the ability of the unscaled OLS model that includes a scale proxy in detecting earnings management.

6. SUMMARY AND CONCLUSION

This paper examines the earnings management of IPO companies before and after the signing of SOX. Despite the increased liability for firms offering securities under the ’33 Act, previous research documents that managers record significantly positive discretionary accruals around the time of the IPO (DuCharme, Malatesta, and Sefcik 2001; DuCharme, Malatesta, and Sefcik 2004; Friedlan 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998). The regulations in SOX impose stiffer penalties for certain corporate malfeasance that is incremental to the regulations in the ’33 Act. As a result, I examine whether the new regulatory environment following the passage of SOX has constrained the previously documented earnings management of IPO-firm managers.

I also examine how the IPO-firm response compares to that of non-IPO firms documented by Cohen, Dey, and Lys (2005).

I find that IPO firms experience a significant decrease in earnings management after the passage of SOX. The results also provide evidence that this decrease is driven by the smallest public companies. Furthermore, I provide evidence that the decrease results in IPO-firm discretionary accruals decreasing to levels that are indistinguishable from those of non-IPO firms. Finally, I find a number of significant differences between the characteristics of pre- and post-SOX IPO firms. After controlling for these differences, the decrease in discretionary accruals in the post-SOX period remains, suggesting that the results are not driven by changes in the composition of IPOs between the pre- and post-SOX periods. While I attempt to dispel this possibility by controlling for these changes, this analysis does not completely eliminate the possibility that the results are driven by a selection bias that results from a change in the types of companies going public. This possibility is one limitation of the current study.

A second limitation of this study is that I use a proxy variable, discretionary accruals, to measure earnings management because I cannot observe IPO-firm earnings management behavior directly. While I employ commonly used techniques to estimate accounting discretion, my proxy is likely to measure discretionary accruals with error. Additionally, the model does not discriminate between within GAAP adjustments and those that violate GAAP. I do, however, control for misspecification in the model associated with extreme performance using a portfolio matching technique commonly used in prior literature.

The sensitivity tests raise another question about the discretionary accruals model used. I find that the results using a discretionary accrual estimate from a scaled model differ from those from an unscaled model with a scale proxy. Previous research, however, uses a scaled model, and the literature documents that discretionary accruals estimated using a scaled model are associated with opportunistic IPO-firm behavior (DuCharme, Malatesta, and Sefcik 2004; Teoh, Wong, and Rao 1998). Future research might further examine the ability of the unscaled OLS model that includes a scale proxy to detect earnings management. Finally, another limitation is that this study does not directly address the effects of SOX on earnings management. Instead, the results can only speak to what has happened in the post-SOX environment.

Subject to these limitations, this paper addresses the concerns of regulators by providing documentation of changes in the financial reporting behavior of firms going public after the passage of SOX. Examining the earnings management behavior of IPOs in the post-SOX period relative to the pre-SOX period might be appealing to investors and regulators because prior literature suggests that earnings management is more prevalent in IPO firms than in the general population of public companies (Friedlan 1994) and that this behavior tends to attract shareholder lawsuits (DuCharme, Malatesta, and Sefcik 2004).

PART 2: REFERENCES

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PART 2: APPENDIX

TABLE 1: SAMPLE SELECTION – PART 2

Description	Number of Firms
IPOs from SDC New Issues Database[a]	683
Missing control variables	(74)
Insufficient data to estimate accruals	(30)
Sales = 0	(24)
IPO Year is 2005	(13)
Not on Compustat	(12)
EDGAR filing missing	(11)
Discretionary acrual outliers	(8)
Fewer than 20 companies in two-digit SIC	(4)
Total	507

[a] Seven hundred six IPOs met the sample criteria using the SDC New Issues Database. Of these 706 firms, however, the prospectuses revealed the following: three firms are unit offerings, three firms are limited-partnerships, two firms are foreign, one firm is as a closed-end fund, and one firm is a trust. Examination of the Compustat data for 12 other firms indicated that the firms were either in the insurance industry (three firms) or banking industry (nine firms). Finally, one offering appeared twice in the SDC New Issues Database.

TABLE 2: SAMPLE DESCRIPTION

	Pre-SOX	**Post-SOX**	**Total**
Panel A: IPO Calendar Year			
2000	258	0	258
2001	51	4	55
2002	0	48	48
2003	0	43	43
2004	0	103	103
Total	309	198	507
Panel B: IPO-year Financial Data			
2000	233	0	233
2001	71	0	71
2002	5	46	51
2003	0	45	45
2004	0	107	107
Total	309	198	507

TABLE 3: VARIABLE DEFINITIONS – PART 2

Variable Name		Variable Definition
AUDITOR	=	1 if Compustat (DATA149) indicates the presence of a Big N auditor for the firm-year, and 0 otherwise.
CFO	=	cash flows from operations (DATA308) at the end of the period scaled by total assets (DATA6) at the end of the period.
MKTVAL	=	market value of equity in the IPO year calculated as price multiplied by the number of shares outstanding (DATA199*DATA25) at the end of the IPO year.
MKTBK	=	equity market value divided by the book value of assets (DATA6).
LEV	=	total debt (DATA9+DATA34) divided by total assets (DATA6), all at the end of the period.
FCOND	=	Zmijewski's (1984) financial condition index, which controls for financial distress.
LOSS	=	1 if the company had a net loss (DATA172) during the period, and 0 otherwise.
PRELOCK	=	1 if the company filed its first 10-K before its lockup agreement expired, and 0 otherwise.
SOX	=	1 if the company filed its first 10-K after July 30, 2002, the date that SOX was signed into law, and 0 otherwise.
SIZE	=	the natural log of MKTVAL.
Y200X	=	1 if the financial data comes from the respective calendar year, and 0 otherwise.
PADA	=	performance-adjusted discretionary accruals estimated using a cross-sectional version of the modified-Jones model.
ASSETS	=	total assets at the end of the IPO year (DATA6).
SALES	=	total revenues for the IPO year (DATA12).
INCOME	=	net income for the IPO year (DATA172).
INITMVAL	=	market value of the offering measured on the first trading day from CRSP.
OFPRC	=	offering price per share taken from the prospectus.
OFFRSIZE	=	number of shares offered taken from the prospectus.
UNDPRC	=	percentage of underpricing measured as the percentage change from the offering price to the closing share price at the end of the first trading day.
SGROW	=	sales growth between the pre-IPO period and the IPO year.

TABLE 3: CONTINUED

Variable Name		Variable Definition
INDROS	=	return on sales for each firm adjusted for the median return on sales for the respective two-digit SIC code industry.
INDROA	=	return on assets for each firm adjusted for the median return on assets for the respective two-digit SIC code industry.
NYSE	=	1 if the firm is listed on the NYSE, and 0 otherwise.
AMEX	=	1 if the firm is listed on the AMEX, and 0 otherwise.
NSDQ	=	1 if the firm is listed on the NSDQ, and 0 otherwise.
AGE	=	the age of the offering at the time of the IPO.
INSTIT	=	percentage of shares held by institutional investors at the end of the IPO year obtained from the Thomson Financial CDA/Spectrum Institutional (13f) Holdings database.
REPUND	=	1 if the lead underwriter is listed as a reputable underwriter in the Loughran and Ritter (2004) underwriter rankings list available on Jay Ritter's website, and 0 otherwise.
VC	=	1 if the SDC New Issues Database identifies the IPO as having venture capital participation, and 0 otherwise.
COV	=	1 if the IPO has research coverage on the First Call Historical Database, and 0 otherwise.
IPO	=	1 if the observation comes from the IPO-year, and 0 otherwise.

TABLE 4: DESCRIPTIVE STATISTICS FOR CONTROL VARIABLES – PART 2

	Pre-SOX (n = 309)	Post-SOX (n = 198)	Overall (n = 507)
AUDITOR	0.95	0.93	0.94
	(1)	(1)	(0)
	[0.22]	[0.26]	[1.00]
CFO	-0.11	0.02	-0.06
	(-0.08)	(0.06)	(-0.01)
	[0.23]	[0.19]	[0.23]
MKTVAL	929.4	1,027.4	967.7
	(351.7)	(352.4)	(351.7)
	[3,561.5]	[3,929.3]	[3,706.0]
MKTBK	3.11	2.65	2.93
	(2.25)	(2.21)	(2.22)
	[3.16]	[2.16]	[2.82]
LEV	0.09	0.18	0.13
	(0.02)	(0.03)	(0.02)
	[0.17]	[0.30]	[0.23]
FCOND	-4.36	-4.09	-4.25
	(-4.58)	(-4.44)	(-4.55)
	[1.93]	[1.76]	[1.87]
LOSS	0.72	0.41	0.60
	(1)	(0)	(1)
	[0.45]	[0.49]	[0.49]

See Table 3 for variable definitions. The numbers above represent means, (medians), and [standard deviations].

TABLE 5: DESCRIPTIVE STATISTICS FOR DISCRETIONARY ACCRUALS

	Pre-SOX	Post-SOX	Overall
No Pre-Lockup Expiration 10-K	**1.58**	0.09	**1.09**
(PRELOCK = 0)	(0.02)	(-0.04)	(-0.00)
	[242]	[117]	[359]
Pre-Lockup Expiration 10-K	**2.21**	0.22	**1.12**
(PRELOCK = 1)	**(0.23)**	(-0.03)	(0.01)
	[67]	[81]	[148]
Overall	**1.71**	0.14	
	(0.06)	(-0.04)	
	[309]	[198]	

See Table 3 for variable definitions. Tests of significance for the means (medians) are based on t-tests (sign tests). Bold numbers denote significance at the 5% level or better, one-tailed. The numbers in brackets represent the number of observations.

TABLE 6: UNIVARIATE TESTS OF DIFFERENCES FOR DISCRETIONARY ACCRUALS

	t-tests		Wilcoxon test	
	Diff.	p-value	Diff.	p-value
Pre-SOX vs. Post-SOX	**-1.57**	0.001	**-0.09**	0.001
Pre-SOX vs. Post-SOX (PRELOCK = 1)	**-1.99**	0.051	**-0.26**	0.002
Pre-SOX vs. Post-SOX (PRELOCK = 0)	**-1.48**	0.009	**-0.06**	0.036

See Table 3 for variable definitions. Tests for differences in means are based on two-sample t-tests and non-parametric tests for differences are based on Wilcoxon tests. Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 7: MULTIVARIATE MODELS

	Model 1		Model 2	
	Estimate	**p-value**	**Estimate**	**p-value**
INTERCEPT	**5.02**	0.059	4.14	0.103
SOX	-0.84	0.358	**-0.87**	0.062
PRELOCK	1.11	0.174	1.02	0.210
PRELOCK*SOX	-0.59	0.608	-0.91	0.393
AUDITOR	-0.62	0.726	-0.76	0.669
CFO	-2.74	0.127	**-3.55**	0.044
SIZE	**-0.44**	0.075	**-0.41**	0.083
MKTBK	-0.03	0.780	-0.01	0.895
LEV	-0.20	0.900	0.11	0.944
FCOND	0.08	0.768	-0.01	0.976
LOSS	0.05	0.930	0.09	0.853
Y2001	**-1.40**	0.009		
Y2002	-0.75	0.436		
Y2003	-1.88	0.121		
Y2004	-0.04	0.973		
<u>Joint Test</u>				
SOX + PRELOCK*SOX = 0	-1.43	0.284	**-1.78**	0.080
n	507		507	
Prob > F	0.001		0.001	
Adjusted R^2	0.060		0.054	

See Table 3 for variable definitions. The dependent variable in this model is PADA. This model is estimated using OLS with White (1980) standard errors. The data set includes financial data from the first 10-K filed by each sample firm. Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 8: SAMPLE COMPOSITION

	Pre-SOX	**Post-SOX**	**Diff.**
Size			
ASSETS	736.40	516.18	-220.21
	(125.56)	(157.55)	(32.00)
SALES	462.82	514.71	51.89
	(37.38)	(134.51)	**(97.13)**
INCOME	-28.37	14.03	**42.40**
	(-13.86)	(2.44)	**(16.29)**
INITMVAL	1,212.00	611.60	**-600.40**
	(472.27)	(312.95)	**(-159.32)**
	[309]	[195]	
MKTVAL	929.37	1,027.44	98.07
	(351.68)	(352.43)	(0.75)
Financing			
LEV	0.09	0.18	**0.10**
	(0.02)	(0.03)	**(0.01)**
OFPRC	14.50	14.01	-0.49
	(14.00)	(14.00)	(0.00)
OFFRSIZE	195.68	155.59	-40.09
	(79.90)	(85.00)	(5.10)
UNDPRC	0.49	0.11	**-0.38**
	(0.25)	(0.09)	**(-0.16)**
	[309]	[195]	
Operating Performance			
SGROW	4.39	0.90	**-3.50**
	(0.90)	(0.30)	**(-0.60)**
	[299]	[194]	
INDROS	-5.28	-6.16	-0.88
	(-0.39)	(0.02)	**(0.41)**
INDROA	-0.13	-0.01	**0.12**
	(-0.05)	(0.02)	**(0.07)**
Exchange Listing			
NYSE	0.10	0.23	**0.13**
AMEX	0.02	0.02	0.00
NSDQ	0.88	0.74	**-0.15**

TABLE 8: CONTINUED

	Pre-SOX	Post-SOX	Diff.
Other Characteristics			
AGE	11.48	18.03	**6.55**
	(6)	(8)	**(2)**
INSTIT	0.29	0.39	**0.10**
	(0.26)	(0.34)	**(0.09)**
	[273]	[193]	
REPUND	0.74	0.77	0.03
VC	0.66	0.55	**-0.11**
COV	0.76	0.92	**0.17**

See Table 3 for variable definitions. The numbers above represent means, (medians), and [number of observations when different from 309 for pre-SOX offerings and 198 post-SOX offerings]. Medians are not presented for proportions. Tests for differences in means are based on two-sample t-tests. Non-parametric tests for differences are based on Wilcoxon tests. Tests for differences in proportions are based on two-sample tests of proportions. Bold numbers denote significance at the 10% level or better, two-tailed.

TABLE 9: INDUSTRY COMPOSITION

	SIC Codes	Pre-SOX Obs	Pre-SOX Percent	Post-SOX Obs	Post-SOX Percent
Panel A: Changes in Industry Composition by Industry Group					
Chemical Products	28	32	10%	28	14%
Communications	48	13	4%	7	4%
Durable Goods	50	1	0%	6	3%
Eating and Drinking Establishments	58	4	1%	8	4%
Electronic Equipment	36	58	19%	13	7%
Food Products	20	2	1%	1	1%
Health	80	10	3%	2	1%
Oil and Gas	13	5	2%	4	2%
Paper & Paper Products	24-27	2	1%	5	3%
Manufacturing	30-34	4	1%	4	2%
Scientific Instruments	38	35	11%	21	11%
Transportation	37, 39, 40-42, 44, 45	2	1%	11	6%
Retail	53, 54, 56, 57, 59	4	1%	12	6%
Entertainment Services	70, 78, 79	0	0%	6	3%
Financial Services	61, 65	0	0%	6	3%
Computer Equipment & Services	35, 73	110	36%	44	22%
All Others		27	9%	20	10%
Total		309	100%	198	100%
Panel B: Comparison of Old Economy vs. New Economy					
Old Economy		174	56%	142	72%
New Economy		135	44%	56	28%
Total		309	100%	198	100%

Industry definitions are from Teoh, Wong, and Rao (1998). The All Others category includes the following two-digit SIC codes: 23, 29, 47, 49, 51, 55, 67, 82, and 87. New Economy firms come from the following affected SIC code groups: 35 (3570, 3571, 3572, 3576, and 3577), 36 (3661 and 3674), 48 (4812 and 4813), 50 (5045), 59 (5961), and 73 (7370, 7371, 7372, and 7373) (Murphy 2003). The Old Economy firms include all sample firms not defined as New Economy firms.

TABLE 10: COMPARISON OF IPOS TO POPULATION

	Estimate	**Std. Error**	**t-stat**	**p-value**
INTERCEPT	**-0.43**	0.26	-1.65	0.098
SOX	-0.02	0.02	-1.11	0.269
IPO	**0.72**	0.11	6.52	0.000
IPO*SOX	**-1.00**	0.33	-3.04	0.002
AUDITOR	-0.07	0.06	-1.15	0.250
LISTED	0.47	0.34	1.39	0.165
CFO	**-0.06**	0.01	-4.51	0.000
SIZE	**0.20**	0.01	13.70	0.000
MKTBK	-0.00	0.00	-0.94	0.346
LEV	**0.02**	0.01	3.67	0.000
FCOND	**-0.00**	0.00	-4.39	0.000
LOSS	-0.04	0.03	-1.06	0.288
<u>Joint Test</u>				
IPO + IPO*SOX = 0	-0.28			0.371
n		28,728		
Number of firms		7,849		
Prob > F		0.000		
R^2		0.015		

See Table 3 for variable definitions. The dependent variable in this model is the absolute value of PADA. This model is estimated using the fixed effects specification. The data set includes all firms with sufficient data between 2000 and 2004. Bold numbers denote significance at the 10% level or better, two-tailed.

VITA

Scott N. Bronson is from Hartford, CT. He attended elementary, middle, and high school in Grove City, OH. Scott received his undergraduate degree in Accountancy from Miami University in Oxford, OH in 1997. While working on his undergraduate degree, he spent the winter semester of 1996 as an intern with Coopers & Lybrand in Columbus, OH. After graduating from Miami, Scott moved to Chicago, IL where he spent three years as an auditor with PricewaterhouseCoopers and two years as a financial analyst and accounting supervisor with the international division of Trans Union. In 2002, he moved to Knoxville, TN to pursue his Ph.D. in Business Administration at the University of Tennessee. Scott is a Certified Public Accountant in Illinois.