FASB Interpretation Number 48 (FIN 48) and Corporate Innovation

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ABSTRACT

In this paper, we analyze the real effect of financial statement tax disclosures on corporate innovation activities. In 2007, the FASB issued FIN 48, which mandates the separate disclosure of reserves for unrecognized tax benefits (UTBs). Using patent applications as a measure of corporate innovation, we employ a difference-in-difference research design with publicly listed U.S. firms as the treatment group and privately held U.S. firms not subject to the disclosure requirements as the control group. We hypothesize and find robust evidence that following the onset of FIN 48, the number of patent applications by publicly listed firms decreased. We also provide evidence that the decrease is attributable to incremental innovation, which is more subject to the UTB disclosure requirements. Overall, our evidence provides support for the real effects of disclosures on innovation activities.

JEL: M40, M41, M48, O30, O38

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ABSTRACT

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Keywords: Uncertain tax positions, FIN 48, Radical Innovation, Incremental Innovation, Patents, Backward Citations

JEL: M40, M41, M48, O30, O38

1. Introduction

Theoretical studies suggest that financial reporting and disclosures engender real effects on corporate innovation (Beyer and Guttman 2012, Kanodia and Sapra 2016, Leuz and Wysocki 2016). Recent empirical studies document the real effects of financial reporting on corporate innovation – more conservative reporting is associated with less corporate innovation (see Chang et al. 2015); and better financial statement comparability improves research and development expenditure (R&D) investment efficiency (see Chircop et al. 2020).[1] Starting in 2007, Financial Interpretation Number 48 (FIN 48) requires firms to disclose the provision for unrecognized tax benefits (UTBs). A significant portion of this provision pertains to tax credits for research and experimentation (R&E).[2] These tax credits have a higher likelihood of being overturned by the Internal Revenue Service (IRS) upon audit, and, thus, more commonly appear as UTBs under the FIN 48 standard (Dyreng et al. 2019, Graham et al. 2012, Robinson et al. 2016, Robinson et al. 2017, Towery 2017).[3] Accordingly, our objective is to examine the real effects of UTB disclosures on innovation. We specifically examine whether the onset of FIN 48 affects the level of firms' patent applications, which enables us to measure firms' innovation output and the type of their innovation activities (i.e., radical innovation versus incremental innovation, Hall et al. 2005).

Even though firms disclose R&D expenses in the financial statements, there is no disclosure on the nature of innovation: specifically, the allocation between radical innovation and incremental innovation. The economics and strategy literature emphasizes the importance

[1] Other studies have examined mandated disclosures on tangible (and intangible) investment efficiency, e.g., Chuk (2013) examines pension disclosure, Bonaimé (2015) examines share repurchase disclosure; Shroff (2017) examines general GAAP disclosures, and Goldman (2020) examines tax disclosure.
[2] In this study, we refer to the firms' innovative activities as innovation. Firms claim tax credits corresponding to these innovative activities, which we refer to as the R&E tax credit. Firms also report expenses on their financial statements corresponding to these innovative activities, which we refer to as R&D expenses or expenditures.
[3] We validate the assumption that the FIN 48 required disclosures contain information regarding innovation activities by providing examples of firms' FIN 48 financial statement disclosures that contain direct references (see Appendix A as well as the discussion in Section II).

of different organizational capabilities for radical and incremental innovation (Dewar and Dutton 1986, Nelson 1959, Nelson and Romer 1996, Rosenberg 1997). Radical innovation is likely to yield a new generation of products; for example, Dahlin and Behrens (2005) examine 581 granted patents for tennis rackets and find that industry experts consider two of them – oversized and wide body to be radical innovations and all others to be incremental innovations. In essence, radical innovation is likely to make significant changes, while incremental innovation builds upon radical innovation. We argue that the UTB reserve disclosures mandated under FIN 48 increased the likelihood of tax credits being disallowed for incremental innovation projects by the IRS upon audit. Thus, it changed the expected net present value of incremental innovation projects. We base our argument on the fact that R&E tax credits intend to provide incentives for firms to pursue radical innovation projects. As such, compared to expenditures on radical innovation projects, expenditures on incremental innovation projects are more likely to be disallowed for R&E tax credits upon IRS audit. It follows that expenditures on incremental innovation are more likely to be part of the UTB reserve.

We posit that firms generally have an incentive to decrease the UTB reserve because the mandated disclosure provides information to the taxing authorities about the firm's tax uncertainty (Robinson and Schmidt 2013; Lisowsky et al. 2013, Towery 2017). Prior studies document that the IRS uses these disclosures to help audit uncertain tax positions (see Bozanic et al. 2017, Graham et al. 2011). UTB reserve disclosures decrease the probability of receiving R&E tax credit benefits for incremental innovation projects, thereby lowering the expected net present value of such projects.[4] Radical innovation is likely to result in fewer (albeit more impactful) patents than incremental innovation because typically, many

[4] Discussions with an anonymous Big 4 Senior Manager confirm that disclosing the UTB reserve in the financial statements does, in fact, affect firm's net present value calculations for innovation. Furthermore, such IRS audits could flag other tax benefits that could be disallowed as well.

incremental projects result from one successful radical innovation project. As such, we hypothesize that following the onset of FIN 48, corporate innovation, as measured by patent applications, decreases.

We test our hypothesis using the OECD Orbis database, which covers entity-level patent applications, combined with Compustat information on firm's attributes. We use fiscal years 2003 through 2011, excluding 2007, which is the year of FIN 48 adoption: 2003-2006 is the pre-FIN 48 period and 2008-2011 the post-FIN 48 period. Since the FIN 48 disclosure requirements only apply to U.S. publicly traded firms (henceforth "public firms"), we use a difference-in-difference research design with privately held firms (henceforth "private firms") as the control group. Our final sample consists of 7,586 firm-year observations representing 1,150 unique firms; with 3,339 and 3,679 (165 and 403) firm-year observations for public (private) firms pre-FIN 48 and post-FIN 48 periods, respectively.

We show that the number of patents (measured by the natural log of one plus the number of patent applications) per year for public (private) firms in the pre- and post-FIN 48 periods are 1.468 and 1.309 (0.500 and 0.613), respectively; this corresponds to 3.341 and 2.702 (0.649 and 0.846) patent applications in the pre- and post-FIN 48 periods for public (private) firms, respectively. The decrease (increase) in patent applications for public (private) firms after FIN 48 is (is not) statistically significant, which is consistent with our hypothesis.

We estimate the difference-in-difference research design by regressing the number of patent applications on an indicator variable for public firms, an indicator variable for post-FIN 48, and their interaction; and control for firm-size, profitability, and growth. Our variable of interest is the interaction of post-FIN 48 and the indicator variable for the public firms. Based on our hypothesis, we expect the coefficient to be negative. We provide evidence that while patent applications for private firms are not statistically different across the pre- and post-FIN 48 periods, patent applications for public firms exhibit a decrease in the post-FIN 48 relative to the private firms, supporting our hypothesis.

Following Trajtenberg (1990), we use the backward citations in the patent applications to classify the patents as emanating from radical or incremental innovation. Specifically, radical innovation is likely to be novel and thus does not rely on prior knowledge as measured by references to existing patents, i.e., backward citations. In particular, we classify a patent as a radical (incremental) patent if there is no (at least one) backward citation in the patent application. We expect and find a decrease in public firms' overall patent applications relative to private firms after FIN 48 to be attributable to incremental patent applications. Furthermore, we find that neither public nor private firms exhibit a significant change in radical innovation between the pre and post-FIN 48 periods. These results provide support to the assumption in our hypothesis that the decrease in patent applications is attributable to incremental innovation.

We next provide clarity on the mechanism. FIN 48 requires firms to enhance their UTB disclosures and firms must adopt new accounting for the standard. While both the treatment and control observations in our sample were required to follow the new standard on accounting for UTBs, only the treatment firms were required to disclose the new reserves. To support our conjecture that it is the disclosure of UTBs and not the change in measurement of UTBs that is driving our finding, we perform two analyses. First, we use textual analysis to show that, among our treatment firms, the results are concentrated among observations which, in their tax footnote, have a longer sentence count, greater number of words per paragraph, and a greater number of complex words. Second, using our public-firm sample only, we examine the FIN 48 adoption restatements and compare the change in innovation among firms with increases to UTBs due to FIN 48 relative to those that decreased or did not change their UTB reserves. Should the decrease in innovation be driven, in part, by the change in the measurement, then we would see firms with an increase in UTB reserves exhibit a more significant decline in innovation following the onset of FIN 48. We do not note any significant differences between the two groups. Thus, we provide evidence that our results are, in fact, a function of the enhanced disclosure requirements for our treatment firms and are unlikely to be due to a change in accounting measurement of the UTB reserve.[5]

We conduct additional cross-sectional analyses. First, we examine the assumption of IRS audit potential embedded in our hypothesis. For this purpose, we use firm-size as a proxy for IRS audit likelihood. We follow prior literature which argues that large-sized firms are likely to have been audited by the IRS in the pre-FIN 48 period (Hoopes et al. 2012; Ayers et al. 2019); and as such, we expect and find our results to be attributable to small-sized firms because of the FIN 48 disclosures. This finding suggests that FIN 48 disclosures adversely affect innovation in public firms that were less likely on the tax authorities' radar for audits before FIN 48, i.e., smaller sized public firms. We also find that our results are more substantial for firms with more tax uncertainty, which is consistent with the notion that firms with more tax uncertainty attract more IRS attention. Third, we examine whether the Great Recession of 2008 confounds our result by forcing financially constrained firms to undertake fewer innovation projects. For this purpose, we partition the sample into profit and loss firms, under the premise that loss firms are likely to be financially constrained. We find that our findings are attributable to both profit and loss firms suggesting that our results are unlikely to be confounded by the Great Recession.

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[5] These findings support those of Goldman (2020), who provides evidence that, following FIN 48, firms with enhanced disclosures lowered R&D expenses, relative to firms who did not. However, Goldman (2020) fails to find a relation between FIN 48 and all innovation activities. We enhance the inferences drawn by Goldman (2020) in three key ways. First, we use patent applications, which is a more direct way of measuring innovation activities relative to R&D expenses. Second, we provide evidence of a reallocation of innovation activities. Third, we continue to find similar inferences as Goldman (2020) when both studies focus on the disclosure effect of FIN 48 on innovation relative to the measurement effect. As a result, while the two studies complement one another by finding the effects of FIN 48 on innovation primarily occur through the disclosure channel, our study extends Goldman (2020) by clarifying the effects of FIN 48 on innovation and exploring the types of innovation that firms engage in following the onset of FIN 48.

Even though private firms provide an excellent control group because these firms had to change the measurement of their FIN 48 reserves but did not have to abide by the FIN 48 disclosures, financial database requirements limit the control variables in our research design. Furthermore, private firms could also be affected by other time-varying effects of innovation coincident with the onset of FIN 48. For this reason, we consider an alternative control group that consists of Canadian public firms. Similar to our findings with private firms, we find a significant decrease in U.S. public firms' innovation activities compared to Canadian public firms. We conduct other additional analyses, including performing an entropy balance, a coarsened exact match, examining all firms with patent data by removing the control variables from our regression, examining alternate event windows, and an alternate measure of innovation. In all of these tests, we find results that are qualitatively similar and support our hypothesis.

Our study makes three important contributions. First, we extend the literature examining the real effects of disclosures. We hypothesize and find UTB disclosure lowers firms' incentives for investing in incremental innovation, leading to fewer patent applications. These findings extend our insights into the real effects of financial reporting and disclosure (see Roychowdhury et al. 2019). The onset of FIN 48 provides an excellent natural experiment for isolating the real effects of disclosure because, while FIN 48 mandates UTB disclosure for publicly traded firms, it does not affect reporting requirements for private firms. While FIN 48 changes the measurement of UTB reserves for all firms, because the main difference between our treatment and control observations in this setting is the disclosure of UTBs, we can attribute our findings to disclosure rather than other changes such as measurement. Accordingly, an important implication of our finding is that mandating disclosures that provide information on the nature of innovation could affect firms' innovative processes. By implication, our findings suggest that Chang et al.'s (2015) result that conservative financial reporting is associated with fewer patents could potentially be driven by conservative firms engaging in a different composition of radical versus incremental innovation.

Second, we contribute to the consequences of tax disclosure literature. While prior literature provides evidence that tax expense informs external parties by providing another measure of income that is informative (Hanlon 2005, Lev and Nissim 2004), some studies suggest that UTB reserve disclosures may not have the same beneficial attributes (Robinson and Schmidt 2013; Cazier et al. 2015, Robinson et al. 2016, Robinson et al. 2017). We extend this literature by examining a significant consequence of the UTB reserve disclosures – a change in corporate innovation for incremental innovation.[6] In doing so, we contribute to an emerging literature examining the real effects of FIN 48 by finding a decline in real activities following the new standard (Goldman 2020; Williams and Williams 2018).

We differ from the concurrent studies of Goldman (2020) and Williams and Williams (2018) across a number of dimensions. First, we examine the effects of FIN 48 on innovation, proxied using the number of new patents and patent allocation between radical and incremental patents. Meanwhile, Goldman (2020) finds a decline in corporate investment (capital expenditures, acquisition expenditures, and R&D expenditures), and Williams and Williams (2018) sees a decline in R&D expenses. While R&D expenses may correlate with innovation, practitioners suggest that "successful innovation process has very little to do with the exact amounts of money we spend" (Forbes 2016) and "spending is not a prerequisite for innovation." (The New Economy 2017) Meanwhile, the patent application count has long

[6] Our study complements the findings of a concurrent study by Williams and Williams (2018) that examines the relation between the FIN 48 disclosure requirements and corporate innovation. Their study finds that regulatory scrutiny lowers firm innovation measured by patent issuances and citations. Given that the R&E tax credit is based on the expenditures, the regulatory scrutiny should pertain to R&D expenditures (i.e., the patent application) and not the R&D output. We also complement Goldman (2020) who provides evidence that firms total investment decreases following FIN 48 with no changes to investment efficiency. Our findings extend this study by documenting that the change in investment depends on the nature of the investment. That is, firms decrease their investments more if they have to include UTB reserves in comparison to investments where no UTB reserves are required.

been established and validated as a measure of innovation (Acs et al. 2002). Second, by examining a difference in behavior for public versus private firms, our study holds constant the changes in accounting measurement and allows us to examine the real effects of disclosure more directly. This methodology diverges from Goldman (2020) and Williams and Williams (2018), who have to employ quasi-natural experiments using cross-sectional variation (Williams and Williams 2018) or a multinational setting (Goldman 2020) to indirectly draw inferences on whether their findings are due to disclosure versus measurement. Lastly, by employing a new methodology to identify patents as radical and incremental, we can yield inferences on the level of changes in innovation and the nature of the innovation. As a result, our findings complement and extend the concurrent literature by providing evidence that FIN 48 results in lower incremental innovation activities and helps answer the call from Blouin and Robinson (2014) to examine the real effects of FIN 48.[7]

Lastly, we extend the literature that examines corporate innovation. The literature suggests that firm innovation leads to future growth (Kogan and Papanikolaou 2013, Kogan et al. 2017). Prior studies also document that firms change their innovation practices in response to real external shocks such as credit supply (Amore et al. 2013), obtaining equity financing (Bena and Li 2014, Bernstein 2015, Ferreira et al. 2014), firm underperformance (Yu et al. 2019), increases in tax burden (Mukherjee et al. 2017), and changes in investor protection (Contigiani et al. 2018). We extend this literature by showing the consequences of disclosures on the nature of innovation on patent applications.

[7] Also related to our study is Jacob et al. (2019), who provide evidence that following the staggered adoption of Schedule UTP starting in 2010, firms delayed large capital expenditures. While their findings of a decline in investment due to tax uncertainty disclosures complement our findings, their study also cannot speak to the effect on corporate innovation nor public disclosure.

2. Background and Hypothesis Development

2.1 Background

Our research objective is to examine the real effects of disclosure on corporate innovation, as measured by patent applications. The disclosure regime is the new standard on tax financial statement disclosure promulgated in FIN 48.

2.1.1 Financial Interpretation Number (FIN) 48

The FASB issued FIN 48 in July 2006; the standard covers the recognition, measurement, and disclosure of unrecognized tax benefits (UTB) effective from fiscal years after December 15, 2006 (Blouin et al. 2007, Blouin et al. 2010). Under FIN 48, firms must use a two-step process for each UTB. The first step involves recognition: firms must examine each tax position and determine if the position is "more likely than not" to be overruled or upheld upon IRS audit. The second step is measurement: for each position that is "more likely than not" to be overruled upon audit, the firm must establish a reserve/provision for the expected value of the potential amount owed. Positions remain in reserve for the lesser of the IRS audit process's conclusion or three years, which is the statute of limitations for these corporate tax activities.

Important for our study, FIN 48 *mandates the disclosure* of the reserves for UTBs. Before FIN 48, under Statement of Financial Accounting Standard (SFAS) No. 5 for Contingent Liabilities, even though in principle UTBs were required to be recognized and measured, separate disclosures were not required. In particular, FIN 48 requires firms to disclose the UTBs as a full tabular reconciliation in the footnotes to the financial statements. The reconciliation includes the beginning balance, current year increases to the reserve, changes to the reserve relating to prior year positions, and the amount of UTB reserves that roll off the reconciliation due to a settlement with the tax authorities or a lapse in the statute of limitations.[8] Additionally, many firms include discussions about the roll forwards and explicitly reference the nature and circumstances of their reserves that predominantly have their innovation projects (Robinson and Schmidt 2013, see Appendix A for examples of such disclosures under FIN 48).

Many studies examine the consequences of FIN 48. Frischmann et al. (2008) fail to find a significant market reaction to the events leading up to the issuance of FIN 48. This finding is consistent with the notion that, in principle, FIN 48 is similar to the previous standard, i.e., SFAS 5; and as such, the new standard is firm value-neutral. Subsequent studies document the many benefits of the UTB disclosures: specifically, UTB disclosures (a) are informative about firms' tax sheltering activity (Lisowsky et al. 2013); (b) reduce earnings management (Gupta et al. 2016), and (c) decrease information asymmetry between managers and external stakeholders (Chung et al. 2019). Other studies, however, show adverse consequences of these disclosures: specifically, UTB disclosures (a) lower the informativeness and value relevance of tax disclosures (Robinson and Schmidt 2013; Robinson et al. 2016, Robinson et al. 2017); (b) temporarily increase external audit costs (Erickson et al. 2016); (c) decrease the information advantage spillover for external auditors who provide tax services to their clients (Gleason et al. 2018), (d) lower investment (Goldman 2020), and (e) lower R&D expenditures.

In this paper, we use FIN 48 as a quasi-natural experiment to examine the real effects of disclosure. Specifically, provided the effect of the accounting standard governing the measurement of UTBs affects both our treatment and control observations similarly in the pre

[8] During the standard-setting process, numerous entities publicly commented to the FASB that they were not in favor of the enhanced disclosure requirements and that the new standard may have unintended effects, probably because it could curtail inefficient investments in their pet projects. Consistent with this notion, Goldman (2020) examines how FIN 48 affects the information provided to external stakeholders and finds a decline in total investment.

versus post-period, we examine whether the mandated disclosures of UTB reserves affect corporate innovation.

2.1.2 Effects of Financial Reporting and Disclosure on Corporate Innovation

Kanodia and Sapra (2016) develop a theoretical framework and show that financial reporting and disclosure entails real effects on firms' decisions. They summarize their main idea in the following statement:

> "The measurement and disclosure rules that govern the functioning of accounting systems – which economic transactions are measured and which are not measured, how they are measured and aggregated, what is disclosed to capital markets and how frequently disclosures are made – have significant effects on the real decisions that firms make […]. The accounting regime is an integral and important component of the economic environment that determines how firms allocate resources. A change in the accounting regime, just like other changes in the economic environment, will result in a new equilibrium with different decisions and prices" (Kanodia and Sapra 2016, p. 624).

In essence, theoretical studies show that financial statement reporting and disclosures affect real firm decisions. Accordingly, many concurrent studies examine the interplay of financial reporting and innovation. Chang et al. (2015) find that more conservative financial reporting is associated with fewer patents. Lin et al. (2020) examine the association between shareholder litigation and corporate innovation; specifically, they use a difference-in-difference research design to examine whether the staggered change in universal demand laws affects corporate innovation and find that reduction in shareholder litigation exposure helps to increase corporate innovation activity, especially those associated with radical innovations. Chircop et al. (2020) find that better financial statement comparability is associated with more efficient investments (both tangible and R&D investments).[9] Overall,

[9] Other studies have examined the legal regime and within firm information environment on corporate innovation, which are conceptually related to information/disclosures affecting investments. Specifically, Gao and Zhang (2019) find that compared to firms that are not required to disclose internal control weakness, firms that are subject to disclose internal control weakness after the Sarbanes Oxley Act exhibit a decrease in patent activity; however, Miller et al. (2018) find that firms with better *ex post* internal controls are associated with more patenting activity.

consistent with the theoretical studies, the empirical studies document the relation between financial reporting and corporate innovation. In contrast to these studies, the onset of FIN 48 for public versus private firms provides a setting for isolating the disclosure effects on real decisions.

2.2 Hypothesis Development

2.2.1 Radical Innovation Versus Incremental Innovation

The economics and strategy literature classifies the nature of innovation into radical versus incremental (Dewar and Dutton 1986, Nelson 1959, Nelson and Romer 1996, Rosenberg 1997). Radical innovation refers to disruptive or discontinuous innovations that either enable introducing a new product category or change existing product categories in substantive ways.[10] Radical innovation is likely to result in fewer (albeit more impactful) patents than incremental innovation because typically, many incremental projects result from one successful radical project. Intuitively, a single high impact radical innovation project and patent could lead to many new products and development related patents.

Dahlin and Behrens (2005) posit that radical innovations have three characteristics: they are novel, unique, and impact future technology (similar to the qualifying expenses tests laid out by the IRS for R&E tax credits). The authors apply this framework to 581 patents granted to tennis racket firms between 1971 and 2001, and identify six patents based on the criteria, out of which two – the oversized and wide body rackets, are considered as radical patents by industry experts. Thus, Dahlin and Behrens (2005) provide evidence that most patents are related to incremental innovation. However, two patents resulting from radical innovation lead to hundreds of additional incremental innovation patents. Consistent with

[10] Conceptually, and more loosely another way to look at this bifurcation is that radical innovation is the "research" portion of innovation and incremental innovation is the "development" portion of innovation. The management literature refers to radical innovation as explorative research and to incremental innovation as exploitive research.

these points, Peeters and Potterie (2006) survey 1,301 CEOs of firms in Belgium and find that firms engaged in radical innovation are more active with patenting activity. Still, most patents pertain to incremental innovation projects (also see Artz et al. 2010). Lin et al. (2020) examine the universal demand law adoption and corporate innovation and provide evidence that patents on incremental innovation are roughly ten times more likely than patents on radical innovation. Overall, these studies provide evidence that incremental innovation is expected to generate more patents than radical innovation.

2.2.2 *Innovation and UTB Disclosure*

The Internal Revenue Code permits tax credits for innovation expenditures. Section 41(d) of the Internal Revenue Code defines qualified research that is eligible for an R&E tax credit as an innovation activity that is "technological in nature" and "the application of which is intended to be useful in the development of a new or improved business component." Consistent with the high level of uncertainty of R&E tax credits, Towery (2017) uses proprietary IRS data and identifies R&E tax credits as the most common tax position that constitutes firms' uncertain tax positions, and thus, is a common inclusion in the UTB reserve.

The UTB reserve disclosures provide information on the uncertainty of the firm's tax positions. While firms are not necessarily required to discuss their R&E tax credits in their UTB reserve disclosures (Towery 2017), Robinson and Schmidt (2013) provide evidence that firms often do include extensive discussions of these reserves. Appendix A provides some examples of the uncertain tax benefit disclosures.[11] Many firms (e.g., Boeing and Celgene) explicitly state that R&E tax credits are a substantial portion of their UTB reserve. Furthermore, in their UTB disclosures, firms like Cisco and Dow Chemical discuss court

[11] Firms often use "R&E" and "R&D" interchangeably when referring to the R&E tax credit.

cases that could affect the likelihood that they will obtain the R&E tax credit. Raytheon discusses how tax legislation affects their R&E tax credits.[12] Given that R&E tax credits form a substantive portion of UTBs, we posit that the disclosure requirements under FIN 48 are likely to provide information on the nature of innovation that is not available in the financial statements.[13]

As firms publicly disclose UTB reserve information in their financial statements, they also inherently provide this information to the IRS, an adversarial party charged with examining the legitimacy of the qualified expenses (Goldman 2020). Bozanic et al. (2017) document that following the onset of FIN 48, the IRS increased financial statement downloads. Survey evidence from Graham et al. (2011) shows that executives were concerned that the new disclosures would become a roadmap used by the IRS during tax audits. Discussions with practitioners suggest that, upon the onset of FIN 48, managers reevaluated investment decisions by considering the incremental detection risk imposed by the new UTB reserve disclosures (i.e., a real effect of disclosure (Kanodia and Sapra 2016)). Simply put, in a cost-benefit framework, under the FIN 48 regime, the cost of conducting innovation increases because it is more likely that R&E tax credit claims will be overturned. Thus, the probability of choosing an innovation project decreases because it is less likely to be a positive net present value project.

While the UTB reserve disclosures provide general information on the firm's innovation activities, it also provides useful information regarding the nature of innovation. The IRS audit guidelines for R&E tax credits emphasize that the critical criteria to pass the "qualified research" test are that the project is developing new products and services. Noting

[12] We note that not every firm explicitly discusses R&E tax credits in their UTB disclosures. However, to the extent that firms mention that R&E constitutes a substantial portion of the UTB, it indirectly provides information that the firm is engaged in a substantial amount of incremental R&E projects. This additional information disclosed in the UTB reserve results in the real effects of disclosure hypothesis as posited by us.
[13] Even though firms disclose R&D expenditures in the financial statements, they disclose almost no information on the nature of innovation, i.e., radical versus incremental.

that radical innovation is more likely related to basic research (i.e., new technologies, extending battery life, non-technology-based material science research) and that incremental innovation generally pertains to development projects that focus on building-upon and commercializing radical innovation, radical innovation projects are more likely to meet the qualifying research criteria than incremental. The IRS audit guidelines emphasize the intent of R&E tax credits for radical projects by stating that even if a firm has obtained a patent, the research activity may not satisfy the definition of qualified research. In effect, the key criteria that govern qualified research for the R&E tax credit are based on the intent of the innovation activity.

To summarize, since incremental innovation is likely to be related to the development and not basic research, such projects are *less* likely to pass the qualified research test. If firms reserve tax credits for most of their innovation projects and if the IRS audits the tax return, the incremental innovation projects are more likely to constitute UTBs relative to radical innovation [for example, see FedEx v. United States, Dkt. No. 08-2423 (W.D. Tenn. 2009)].[14] This effect would lead to fewer incremental innovation projects passing the positive net present value criterion in the post-FIN 48 period. Thus, fewer funds would be allocated to incremental innovation projects. Because incremental innovation typically generates more patent applications (e.g., Dahlin and Behrens 2005, Lin et al. 2020) than radical innovation, we predict that the patenting activity of firms affected by FIN 48 (required to disclose UTB reserves), relative to firms unaffected by FIN 48 (not required to disclose UTB reserves), will be lower in the post-FIN 48 period, relative to the pre-FIN 48 period.

[14] See http://www.technologytax.com/fedex-v-united-states-dkt-no-08-2423-w-d-tenn-2009/. The IRS rejected FedEx's claim that a software project with the objective of eliminating revenue leakage was qualified research project. FedEx challenged IRS's ruling arguing that the project is radical because it faced considerable technological challenge and that is why it "failed."

Hypothesis: *Compared to the pre-FIN 48 period, firms disclosing UTB reserves have fewer patent applications in the post-FIN 48 period.*

Despite our prediction that innovation may decrease following the onset of FIN 48, in untabulated analysis, R&D expenditures in firms' financial statements among public firms increase throughout our sample period. Given that R&D expenses increase rather than decrease, we would expect this to bias us against finding results. Additional tension to the hypothesis arises from the neo-classical notion that disclosures, in general, will not affect real decisions. Since FIN 48 only governs the disclosure and does not change the principles to recognize created reserves for UTBs, firms may not change their innovation investment strategy. However, we choose to state the hypothesis as above because various empirical studies have documented investment strategy changes due to financial reporting (Biddle and Hilary 2006, McNichols and Stubben 2008, Biddle et al. 2009).

3. Research Design

3.1 Identification Strategy

To exploit the shock in mandated disclosure without a fundamental change in recognition requirements, we use private firms as the control group and public firms as the treatment group. FIN 48 is a U.S. GAAP reporting requirement that changes the accounting for UTBs for all firms but only changes the disclosure of UTBs for publicly-traded firms. In effect, we use a difference-in-difference research design to isolate the effects of the UTB reserve *disclosure* requirements on corporate innovation, as measured through patent applications.

3.2 Empirical Model

To test our hypothesis, we estimate the following model:

$$Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + Year\ F.E. + \varepsilon_{i,t} \quad (1)$$

Following Fang et al. (2014), Seru (2014), and Agarwal et al. (2018), the dependent variable in Equation (1), *Patent_Count*, is the natural logarithm of one plus the total number of patent applications filed by the firm in a year.[15] Equation (1) incorporates a difference-in-difference research design. The first difference separates the treatment firms, i.e., the public firms, from the control firms, i.e., the private firms. For this purpose, we create an indicator variable denoted *Public* that equals one if the firm is a public firm, and 0 otherwise. The second difference separates the treatment years from the control years: FIN 48 went into effect for fiscal years following December 15, 2006, and thus firm-year observations 2007 or later. As such, we create an indicator variable denoted *Post-FIN48* that equals one if the observation's fiscal-year is 2007 or later, and 0 otherwise. To test the hypothesis, the variable of interest is the interaction of these two indicator variables, i.e., *Public×Post-FIN48.* Based on the hypothesis, we expect the interaction coefficient, i.e., *Public×Post-FIN48*, to be negative ($\beta_3 < 0$).

We control for firm size, profitability, and growth (see, for example, Ciftci and Cready 2011, Curtis et al. 2020). The control for size is especially important since we use patent activity. For example, Cohen and Klepper (1996) show that smaller sized firms have disproportionately more patenting activity (see Stylized Fact 4, p. 930). We calculate size (*Size*) using the natural log of total assets, profitability (*ROA*) calculated as net income (NI) scaled by average total assets (AT), and growth (*ΔSales*) calculated as the change in sales

[15] We do not consider the input measure R&D expenditure provided in firms' financial statements for three reasons. First, R&D expenses are not generally available for privately held firms. Second, firms exhibit variation in the way they allocate R&D to projects and apply for R&E tax credits. Third, we cannot separate R&D expenses into radical and incremental research expenses.

scaled by prior year sales. We winsorize continuous variables. Furthermore, we control for loss firms (*NegROA*) and firms with negative growth (*NegΔSales*). *NegROA* (*NegΔSales*) is an indicator variable that equals one if *ROA* (*ΔSales*) is negative and zero otherwise. We include these controls because firms that face financing constraints due to poor performance are likely to adversely affect their innovation activities (Li 2011, Nanda and Rhodes-Kropf 2013). Given that FIN 48 went into effect around the Great Recession of 2008, it is especially important to control for firms' financial constraints.[16] We correct the standard errors used to compute the t-statistic by clustering by firm (Petersen 2009). See Appendix B for a detailed description of the variables included in our analysis.

4. Empirical Analyses

4.1 Sample Selection and Descriptive Statistics

The sample selection begins with firms with patent data available in the Bureau Van Dijk's (BvD) Orbis dataset. The Orbis dataset covers worldwide firm patent activity based on patent applications from the European Patent Office's Worldwide Patent Statistical Database (PATSTAT). PATSTAT information is linked to individual company names by the OECD and BvD (see Ribeiro et al. 2010).[17] We use the patent application date instead of the patent grant date since the application time is more closely related to the period of innovative activity of a firm than when the patent is granted. Given the difference-in-difference design, it is important for our purpose that patents granted in the post-FIN 48 period should not be attributable to innovation expenditures and projects undertaken in the pre-FIN 48 period.

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[16] Even though prior studies include additional control variables such as cash holdings, percentage of institutional ownership, managerial ability, among many others, data on these variables are not available in the Orbis database. In robustness tests, to help mitigate possible concerns related to correlated omitted variables we include firm fixed effects and find qualitatively similar results.

[17] The Orbis data represents approximately 83 million patents representing approximately 270,000 active and inactive public and private firms.

While the Orbis dataset includes firms across numerous countries, we specifically focus on the U.S. to mitigate concerns that differences in innovation activities and tax laws across various countries could confound our results. We match the Orbis patent dataset to Compustat financial data for U.S. public firms and S&P Capital IQ financial data for private firms. After matching the three datasets we are left with an initial sample size of 20,383 firm-year observations over 4,205 firms. We first remove firms with missing total assets or firms with total assets of less than 10 million. This restriction is particularly important when examining private firms because we do not want small private firms to bias our inferences (Farre-Mensa and Ljungqvist 2016). We next exclude financial firms because the framework for radical innovation and incremental innovation may not be appropriate for such firms. Next, we exclude firm-year observations pertaining to 2007 because that was the year FIN 48 went into effect. Removing this year allows sufficient time for firms to adjust their innovation decisions. We also require that sample firms exist in both the pre-FIN 48 and the post-FIN 48 period to mitigate potential bias due to new firms entering the sample in the post-FIN 48 period, especially for the control group. Specifically, we exclude firms that entered the sample after FIN 48. We also exclude U.S. private firms with a listed U.S. parent. Finally, we exclude firms with no patent applications during the sample period because these may not be firms with any innovation activities and drop all observations where control variables are missing. We further require that firms have control variables at least once in the post-FIN 48 period. After making these sample cuts, we are left with 7,586 public and private firm-year observations, representing 1,150 firms. Table 1, Panel A details the sample selection procedure. [18]

[18] The number of firms is higher in the post-FIN 48 period than the pre-FIN 48 period because more of the control variables are available for the post-FIN 48 period than for the pre-FIN48 period. In Table 8 Panel A, we estimate Equation (1) without requiring the existence of the control variables for an expanded sample of 319,018 firm-year observations and find qualitatively similar results.

Table 1, Panel B, presents the sample's breakdown by the treatment group, i.e., the public firms, and the control group, i.e., the private firms. The public sample contains 3,339 (885) and 3,679 (975) firm-year observations (firms) in the pre- and post-FIN 48 periods, respectively; and the private sample contains 165 (107) and 403 (175) firm-year observations (firms) in the pre- and post-FIN 48 periods, respectively. Given the sample selection criteria, the observations are distributed evenly across the pre-FIN 48 period (46.2% = 3,504 / 7,586) and post-FIN 48 period (53.8% = 4,082 / 7,586).

Table 1, Panels C and D present the descriptive statistics for the explanatory variables in Equation (1): Panel C presents the descriptive statistics for the overall sample, and Panel D presents the descriptive statistics separately for the public firms and private firms across the pre- and post-FIN 48 periods. Overall, both the public and private firms' characteristics are different across the pre- and post-FIN 48 periods, emphasizing the importance of controlling for these factors. The mean ROA for the public firms and private firms in the pre-FIN 48 (post-FIN 48) period are 0.070 and 0.114 (0.059 and 0.087), respectively; both the public firms and private firms exhibit lower profitability in the post-FIN 48 period than in the pre-FIN 48 period possibly due to the recession. Furthermore, public firms exhibit lower profitability potentially because of lower risk or more conservative accounting standards.

4.2 Difference-in-Difference Analysis Results

We examine the parallel trend assumption embedded in the difference-in-difference analysis. In Figure 1, we map out the trend in patent application activity over our sample period for our treatment group of U.S. public firms and our control group of U.S. private firms. To do so, we estimate conditional year-dummy effects and a 95 percent confidence interval for each year, with the year 2006 denoting the base as a reference point and benchmark. In the pre-FIN 48 period, we document that estimated coefficients and their confidence interval for all three groups from 2003-2005 are not significantly different from zero, i.e., we find no significant differences to the values relative to the base year, 2006. The lack of significant differences provides evidence of a parallel-trend in the pre-FIN 48 period between our treatment and control firms. Furthermore, following the onset of FIN 48, the treatment firms, i.e., U.S. public firms, display significantly lower patent application activity, but U.S. private firms no significant decrease, which is consistent with our hypothesis.

Table 2, Panel A, presents the univariate test of the hypothesis. The mean *Patent_Count* for public firms decreases from 1.468 before FIN 48 to 1.309 after FIN 48 which is statistically significant (t-statistic = -4.222, $p < 0.01$). Conversely, the mean *Patent_Count* for private firms increases from 0.500 before FIN 48 to 0.613 after FIN 48, which is not statistically different from zero (t-statistic = 1.553). This evidence supports the hypothesis.

4.2.1 Primary Results

Table 2, Panel B presents the results of estimating Equation (1) without control variables in column (1) and with control variables in column (2), where both show similar results, and we discuss the latter. The coefficient estimate on *Public* is 0.219 (t-statistic = 2.077, $p < 0.05$), suggesting that in the pre-FIN 48 period, the public firms' innovation activity is higher than the innovation activity of private firms, as measured through patent applications. The coefficient on *Post-FIN48* is non-significant, which suggests that after 2007, private firms did not exhibit a substantial change in patent activity. The coefficient on the interaction between *Public* and *Post-FIN48* is -0.224 (t-statistic = -3.229, $p < 0.01$). This statistic supports the hypothesis: specifically, following the onset of FIN 48, firms affected by these disclosure requirements (i.e., public firms) exhibit a greater decrease in innovation than firms unaffected by the disclosure requirements (i.e., private firms).

4.2.2 Radical Versus Incremental Innovation

Our hypothesis is predicated on a decrease in innovation activities because incremental innovation projects are not likely to receive the R&E tax credit, thereby decreasing the expected net present value of such projects. We examine this presumption in our hypothesis by classifying patent applications as incremental or radical. Because firms do not directly disclose the classification of patents as "Radical" or "Incremental", we use the idea embedded in Hall et al. (2001) to classify patent applications as incremental or radical innovation (see also, Balsmeier et al. 2017, Brav et al. 2016).[19] In particular, we use the "References Cited" in the patent application, which we refer to as backward citations in the patent applications to classify patents as incremental or radical.[20] Using this framework, we classify a patent as a radical patent if there is no backward citation in the patent application; correspondingly, we classify a patent as an incremental patent if there is at least one backward citation. Based on the hypothesis, we expect that incremental innovation patents drive our results reported in Table 2.

Table 3 presents the results when patent applications are classified as radical or incremental using zero backward citations as the cut-off. Table 3, Panel A provides the mean radical (*Radical_Patent_Count)* and incremental (*Incremental_Patent_Count) Patent_Count* for public and private firms in the pre- and post-FIN48 periods. The means for *Radical_Patent_Count* for the public (private) firms in the pre- and post-FIN 48 periods are 0.574 and 0.568 (0.102 and 0.112), respectively, and statistically not different from each

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[19] The description in this section draws upon Hall et al. (2001).

[20] Backward citations serve an important legal role in that it demarcates the sphere of new innovation that the firm lays claim to as its intellectual property. In the U.S., the applicant has a legal duty to disclose the prior knowledge; however, the patent examiner who approves/grants the patents is the ultimate arbiter of existing knowledge, i.e., the backward citations and new knowledge. Trajtenberg et al. (1997) examine patents generated by universities and corporations and use the backward citation measure on the premise that "basic patents would have fewer and/or less important predecessors…" (p. 29): here the term basic patents pertains to radical innovation. They find that university patents that are more likely to be radical exhibit fewer backward citations than corporation patents, especially for older patents. Simply put, radical innovation is likely to push the knowledge envelope and thus does not rely on prior knowledge as measured by old patents. Conversely, incremental patents build upon prior knowledge, either radical or incremental, and as such are likely to have more backward citations.

other. However, patent applications on radical innovation are statistically higher for public firms than for private firms before and after FIN 48. The pattern of incremental innovation is starkly different from that of radical innovation. The mean *Incremental_Patent_Count* for public (private) firms in the pre- and post-FIN 48 periods are 1.543 and 1.359 (1.160 and 1.248). While the decrease in incremental innovation for public firms is statistically significant (difference = -0.183, t-stat = -4.597, $p < 0.01$), the increase for private firms is insignificant. The decrease in public firms' incremental innovation is consistent with our expectations that FIN 48 adversely affects innovation activities amongst firms required to disclose UTBs publicly.

Table 3, Panel B provides the estimate of Equation (1), when we classify patent applications as radical versus incremental based on zero backward citations. Column (1) provides the results when the dependent variable is *Radical_Patent_Count*, and Column (2) provides the results when the dependent variable is *Incremental_Patent_Count*. For Column (1), the coefficient on the interaction between *Public* and *Post-FIN48* is not significant. In Column (2), the coefficient on the interaction term is significant and is -0.218 (t-statistic = -3.073, $p < 0.01$). In untabulated analysis, when the *Radical_Patent_Count* and *Incremental_Patent_Count* are stacked in one regression with an interaction term to test the difference across the two columns, we find that the two estimated coefficients on the interaction between *Public* and *Post-FIN48* (*Public*×*Post-FIN48*) are significantly different from each other (-0.211, t-statistic = -2.662, $p < 0.01$). Thus, our multivariate evidence is consistent with the univariate statistics in that the new disclosure requirements under FIN 48 are associated with a decrease in incremental innovation.[21]

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[21] The zero cut-off of zero backward citations for radical innovation may not be appropriate because radical innovation is also likely to depend on some prior knowledge; that is, even though the patent examiner and the firm are required to provide the appropriate backward citations, they may not do so. As such, in unreported analysis, we also use patents with few backward citations and classify them as radical as well. Specifically, we use the empirical distribution of the backward citations and classify patents in the bottom 10%, 20% and 30% of backward citations and those with zero backward citations as radical patents and find qualitatively similar results.

4.3 Cross-Sectional Analyses

We perform cross-sectional tests to examine important presumptions in our hypothesis, such as the underlying disclosure mechanism and the likelihood of a firm facing IRS audit. We also perform tests to mitigate concerns related to the potential confounding effect of the Great Recession of 2008, potentially imposing financial constraints that adversely impact corporate innovation.

4.3.1 Underlying Mechanism: Disclosure vs. Measurement

FIN 48 imposes two significant changes on UTBs: (1) disclosure of tabular reconciliation of the UTB reserve and (2) more conservative reporting of the reserve. An advantage of our setting is that we use treatment (public) and control (private) observations that face the same changes to the reporting of the reserve and only differ in what firms publicly disclose. Thus the exogenous variation derives from firms' disclosures. To provide additional clarity on the underlying mechanism, we perform two tests. First, we use textual analysis to examine cross-sectional variation in firms' tax footnote disclosures. Second, we use FIN 48 restatement data to identify cross-sectional variation in firms' change in accounting estimates when adopting FIN 48. Should the disclosure mechanism be the predominant channel, we expect firms with greater disclosure to drive our findings.[22]

Table 4, Panel A, presents our disclosure analysis. Column (1), (2), (3), and (4) present our analysis when our cross-sectional variation is *Disclosure,* which takes a value of 1 if the observation has a high amount of disclosure (high sentence count, high words per paragraph, paragraph complexity, and sentence complexity, respectively), and 0 otherwise. Consistent with expectations, we find that firms with more substantial and complex tax

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[22] We perform our disclosure and measurement cross-sectional tests on our public subset of firms Table 4, Panel A, given no disclosure information for private firms and both private firms and firms with missing restatement data in Table 4, Panel B.

footnote disclosures have significantly lower innovation following the onset of FIN 48, relative to firms with less substantial or complex tax footnote disclosures (Column (1): coef. = -0.211, t-stat = -2.817, $p < 0.01$; Column (2): coef. = -0.192, t-stat = -2.301, $p < 0.05$; Column (3): coef. = -0.188, t-stat = -1.980, $p < 0.05$; Column (4): coef. = -0.163, t-stat -2.101, $p < 0.05$).

Meanwhile, for our test examining a change in accounting estimation, we obtain data on the restatements due to FIN 48 directly from Audit Analytics. Following FIN 48, many firms restated their tax footnote to include the updated UTB reserve calculation. In doing so, most firms (67%, according to Audit Analytics (2008)), who restated their financials, for this reason, increased their UTB reserve. Using this dataset, we split our observations into those who had an increase in UTB reserve due to FIN 48 versus those that either decreased or did not change their reserve. We expect that if the decline in innovation is a function of the higher UTB reserves given the accounting estimation changes (see Graham et al. 2005 and Williams and Williams 2018), then our findings would be more significant for the group that significantly increases their UTB reserve. This effect would be further pronounced because the affected firms are taking both a one-time increase to their reserves plus are more likely to have higher reserves moving forward. Table 4, Panel B, presents our bifurcated analysis with our firms that increase their UTBs due to FIN 48 in Column (1) and our firms that decrease or do not change their UTBs due to FIN 48 in Column (2). We find that both firms with increases (Column (1): coef. = -0.173, t-stat = -3.546, $p < 0.01$) and decreases or no change (Column (2): coef. = -0.299, t-stat = -4.819, $p < 0.01$) exhibit a decline in innovation following FIN 48. Using an F-test, we do not find a significant difference between the coefficients, i.e., firms with increases in UTB reserve changes and decreases or no change in UTB reserves show a similar effect after FIN 48.

The results in Table 4 provide consistent evidence that our results are a function of the change in disclosure rather than the accounting estimation change.

4.3.2 Firms More versus Less Likely to Face IRS Audit

Whether a firm faces IRS audit before the onset of FIN 48 likely influences the effect of UTB disclosures on firm innovation because firms that are already under audit in the pre-FIN 48 period would be already providing significant amounts of information to the IRS (Hoopes et al. 2012; Ayers et al. 2019). Put differently, for firms audited by the IRS in the pre-FIN 48 period or under continuous audit, the new information released to the tax authorities after the onset of FIN 48 is likely smaller than the information released for firms that are not audited in the pre-FIN 48 period. As a result, we expect the impact of UTB reserve disclosures on corporate innovation activities to be lower for firms that were audited or under continuous audit by the tax authorities in the pre-FIN 48 period.

Even though data on whether a firm is or is not audited by the IRS is not publicly available, recent studies provide insights into the likelihood of an IRS audit. Hoopes et al. (2012) examine TRAC data to analyze the role of IRS audit probability on tax avoidance. Furthermore, Beck and Lisowsky (2014) and Ayers et al. (2019) use special access to data on the Compliance Assurance Process (CAP) audit program and Coordinated Industry Case (CIC) program, respectively, to understand the effects of being audited continuously by the IRS on firm attributes. A common insight across all three studies is that firm-size is a significant driver of IRS audit probability.

Using size as an indicator for the probability of an IRS audit, we bifurcate our sample into small versus large firms, based on a median split of the average *Size*. Table 5, Panel A, Columns (1) and (2) present the results from estimating Equation (1) for small-sized (*SmallFirm* = 1) and large-sized firms (*SmallFirm* = 0), respectively. Consistent with expectations, the results suggest that the findings in Table 2 are attributable to small-sized firms. Specifically, the coefficient on *Public×Post-FIN48* is negative for small-sized firms (β_3 = -0.194, t-statistic = -2.583, $p < 0.05$) and statistically insignificant for large-sized firms on Table 5, Panel A.[23]

A high UTB reserve level is likely indicative of a firm with many uncertain tax positions (Scholes et al. 2014, Drake et al. 2016, Towery 2017), which likely draws IRS attention. Accordingly, we expect that firms with a larger UTB reserve disclosure have a more significant decrease in patent applications than firms with a smaller UTB reserve disclosure.[24] For this test, we examine only public firms since private firms do not have UTB disclosures. We eliminate firms without UTB data available in Compustat as it is not clear whether these firms have zero UTBs or if their UTB information is not available in the data set (Lisowsky et al. 2013).[25] We average the firm's UTB reserve levels per firm over post FIN 48 years for our remaining observations. We split our sample at the median and separately examine firms that have high (*LowUTB* = 0) and low UTB (*LowUTB* = 1) reserve disclosures.

Table 5, Panel B provides the results of estimating a modified version of Equation (1) with firms with a low UTB reserve in Column (1) and firms with a high UTB reserve in Column (2).[26] Across both columns, the coefficient on *Post-FIN48* is negative and significant (coefficient = -0.165, t-statistic = -4.016, $p < 0.01$ in Column (1), coefficient = -0.302, t-statistic = -6.414, $p < 0.01$ in Column (2)). An F-test for equality of the two coefficients suggests that the 0.137 difference is statistically significant (t-statistic = 2.189, $p < 0.05$). This finding suggests that firms with larger UTB reserve disclosures have significantly greater

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[23] In untabulated analysis, we separately examine incremental and radical patent counts, and find that the results are attributable to incremental innovation patent count for small-sized firms.
[24] In untabulate analyses, we substitute UTB reserves with cash ETR volatility to measure tax uncertainty (Guenther et al. 2017) and split between low versus high cash ETR volatility. Our inferences remain unchanged.
[25] In untabulated analysis, we replace missing UTB data with zero and find qualitatively similar results.
[26] Because UTB reserves are only available for *Public* firms, we adjust Equation (1) to remove the *Public* and *Public×Post-FIN48* terms, and our variable of interest is *Post-FIN48*.

decreases to patent applications following FIN 48, relative to firms with smaller UTB reserve disclosures.[27]

4.3.3 Profit versus Loss Firms

The onset of FIN 48 disclosure requirement roughly coincides with the Great Recession of 2008. The results that we infer as supporting our hypothesis could be attributed to recession because public firms could have faced a greater financial constraint than private firms, which in turn could have decreased patent applications for public firms more so than for private firms. We re-estimate Equation (1) by bifurcating the sample into profit and loss firms in Table 5, Panel C. On the one hand, loss firms are likely to be more susceptible to financing constraints than profit firms, and hence, such firms are likely to exhibit a decrease in innovation. However, on the other hand, loss firms are not likely to be subjected to tax strategies as much as profit firms. These opposing effects suggest that our results should not be attributable to either profit or loss firms.

We present our results in Table 5, Panel C, and find support for our hypothesis for both profit and loss firms. Specifically, the coefficient on *Public×Post-FIN48* for both loss firms (coefficient = -0.476, t-statistic = -2.569, $p < 0.05$) and profit firms (coefficient = -0.215, t-statistic = -2.754, $p < 0.01$) is negative and significant, and the coefficients are statistically similar for the two groups. Thus, our results are not attributable to the differences across profit and loss firms, i.e., the recession.[28]

4.4 Alternative Control Group: Canadian Public Firms

Private firms provide an excellent control group because FIN 48 disclosures do not apply to them. At the same time, they are subject to the same patent application requirements

[27] In untabulated analysis, we separately examine incremental and radical patent counts, and we document that our findings are driven by the incremental patent count.
[28] In untabulated analysis, we find that the results for both profit and loss firms are attributable to incremental patent count.

by the U.S. Patent and Trademark Office as U.S. public firms. Private firms could also be affected by other factors related to innovation that are different from public firms for which we do not control. For this reason, we consider an alternative control group that consists of Canadian public firms.[29]

Using Canadian public firms as the control group has the advantage of being able to control for several additional factors that have been shown in the prior literature to be associated with corporate innovation and could be correlated with the time-varying effects of FIN 48. Specifically, in addition to the control variables in Equation (1), we follow Guo et al. (2019) and control for debt (*Leverage*), cash holdings (*Cash*), Property, Plant & Equipment (*PPE*), Capital expenditures (*CAPEX*), R&D expenses (*R&D*), performance-matched discretionary accruals (*DACC*), the Kaplan and Zingales index (*KZ Index*), Tobin's Q (*BTM*), firm age (*Age*), and the Herfindahl-Hirschman Index (*HHI*) to control for industry competition (also squared HHI to control for any non-linear effects). See Appendix B for the definition of these variables.[30]

Table 6, Panel A, presents a breakdown of our sample size when Canadian public firms are the control group.[31] Table 6, Panel B, presents the univariate test of the hypothesis that U.S. public firms display significantly lower patent application activity after FIN 48 using the alternative control group of Canadian public firms. The mean *Patent_Count* for Canadian public firms decreases from 0.746 before FIN 48 to 0.663 after FIN 48, which is statistically insignificant from zero (t-statistic = -0.919), while the mean *Patent_Count* for U.S. public firms significantly decreases from 1.468 before FIN 48 to 1.309 after FIN 48 (t-

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[29] We only consider Canada as a control group in our alternative analysis, rather than our primary analysis because, like the U.S., Canadian patent laws are often subject to change which could affect our inferences. While no significant changes that would affect our results occurred during our sample period, the use of cross-country analysis may have drawbacks, e.g., institutional differences between the U.S. and Canada. Despite these concerns, the use of Canadian public firms as our control group helps provide assurance that our primary inferences are not biased by our control group.
[30] We replace all missing continuous control variables with zero.
[31] In untabulated analysis, we also examine a comparison of the control variables for this sample in a manner similar to Table 1, Panel D. We note no unusual patterns.

statistic = -4.222, $p < 0.01$), as also shown in Table 2, Panel A. This evidence supports our hypothesis.

Table 6, Panel C, provides the result of estimating the modified Equation (1) with all the additional control variables in Column (1). The coefficient on the interaction term *USA×Post-FIN48* is -0.168 (t-statistic = -2.062, $p < 0.05$), which supports our hypothesis that U.S. public firms display significantly lower patent application activity after FIN 48.[32]

4.5 Entropy Balancing and Coarsened Exact Matching

Private and public firms are likely to be intrinsically different because becoming a publicly listed firm is a choice that likely depends on various unobservable factors that could generate non-linearity. Even though the difference-in-difference design mitigates such concerns, we use the entropy balancing method (Hainmuller 2012, Shipman et al. 2017) to weight the covariates. Furthermore, private firms represent about 8.5% of the sample, and as such, the parallel trend assumption could be seemingly satisfied because of the low statistical power of the control group. To mitigate this concern, we use the coarsened exact matching procedure to match private and public firms on the covariates (Iacus et al. 2012).

First, we entropy balance on all covariates for the first two moments, i.e., mean and variance. Table 7, Panel A provides the mean and variance of the control variables for public and private firms before and after entropy balancing. After entropy balancing the mean and variance of control variables for the private firm observations are similar to those of the public firms. Table 7, Panel B, column (1) provides the results of estimating Equation (1) after the private firm observations are entropy balanced. We find that the coefficient of the interaction term (*Public × Post-FIN48*) remains negative and statistically significant (-0.346,

[32] In untabulated analysis, we document a parallel-trend in the pre-period when examining Canadian firms as our control group.

t-statistic = -2.785, $p < 0.01$). More importantly, all covariates (other than firm-size) are not statistically significant, showing that the weighting of the private firm's observations "matches" those of public firms in profitability and growth dimensions.

Second, we employ a coarsened exact matching procedure (Iacus et al. 2012). Specifically, we coarse exact match on our non-transformed and winsorized control variables, i.e., total assets, return on assets, lagged change in sales and the dummy variables for a negative return on assets and a lagged change in sales, and choose the strata based on the terciles of each variable. This technique reduces the L1 imbalance distance in covariates between U.S. public and U.S. private firms by 20.45% from 0.44 to 0.35. The coarsened exact matching deletes 156 unmatched U.S. private observations and 5,266 U.S. public observations. This removal lowers our sample size to 2,164, consisting of 412 private firm observations and 1,752 public firm observations. Thus, private firm observations represent roughly 20% of the sample size, mitigating concern about the control group's relatively smaller sample size driving our results. Table 7, Panel B, column 2, provides the results of estimating Equation (1) after balancing our sample's covariates by coarsened exact matching. We find that the coefficient of the interaction (*Public* × *Post-FIN48*) is negative and statistically significant (-0.438, t-statistic = -2.293, $p < 0.05$), which supports our hypothesis.

Third, we combine the previous matching approaches, as suggested by Hainmuller (2012). Specifically, we first employ the coarsened exact matching procedure and then entropy balance all control variables for U.S. public and U.S. private firms on the first two moments. Table 7, Panel B, column 3, provides the results of estimating Equation (1) after balancing our sample's covariates by coarsened exact matching and entropy balancing the control variables. We find that the coefficient on the interaction term (*Public* × *Post-FIN48*) is negative and statistically significant (-0.271, t-statistic = -2.806, $p < 0.01$). This finding supports our hypothesis.[33]

4.6 Robustness Tests

4.6.1 Expanded Sample and Firm-Year Fixed Effects

In Equation (1), we include control variables that prior studies associate with innovation activity. We do so to control for firm-specific factors that could change from the pre-FIN 48 to the post-FIN 48 periods; however, the shortage of financial data available for private firms limits the set of such control variables. As such, instead of financial data, we consider firm fixed effects.[34] The inclusion of firm and year fixed effects allows for only within-firm and within-year variation to affect the relation between innovation and the interaction of *Public* and *Post-FIN48*. It thus mitigates concerns that time or firm-specific invariant factors drive the results. Not requiring the firm-specific control variables increases the sample size substantially: the number of firm-year observations is 319,018, representing 40,350 unique firms. Table 8, Panel A reports the results of estimating a modified Equation (1) for this sample. The results are qualitatively similar to those discussed earlier: specifically, the coefficients on the interaction of *Public*×*Post-FIN48* are -0.212 (t-statistic = -8.247) and -0.0694 (t-statistic = -3.001, $p < 0.01$) with no fixed Effects and with year and firm fixed effects, respectively.

[33] In untabulated analysis, we examine the analysis where the Canadian public firms are our control group using the entropy balanced and coarsened exact matching procedures. We document that our inferences are qualitatively the same.
[34] Because firms in our sample are either always public and always private, the inclusion of both the year and firm fixed effects perfectly correlated with the main effect of *Public* and *Post-FIN48.* As such, in this research design we include only the interaction between *Public* and *Post-FIN48.*

4.6.2 Alternative Definition of Patent Activity

Even though we control for firm-size in our analysis, it is possible that scale effects, as documented in Cohen and Klepper (1996) or Ciftci and Cready (2011) bias the inferences. To mitigate this concern, we consider scale effects directly: We compute *ScaledPatent_Count* by scaling the number of patent applications by total assets. We replace *Patent_Count* with *ScaledPatent_Count* and estimate Equation (1) and find qualitatively similar results (see Table 8, Panel B).

4.6.3 Alternative Testing Window

Instead of using the four-years before and after the onset of FIN 48, we use two years before (2005-2006) and two years after (2008-2009) the onset of FIN 48. The sample decreases to 1,947 firm-year observations. We re-estimate Equation (1), and report the results in Table 8, Panel C, and find qualitatively similar albeit statistically weaker results.

5. Conclusion

We exploit the onset of the FIN 48 disclosure requirements for UTBs to examine whether disclosures affect corporate innovation, as measured through patent applications. We hypothesize that under FIN 48, firms are less likely to allocate towards incremental innovation projects, which will decrease the number of patent applications. As our identification strategy, we use a control group of private firms subject to FIN 48's changes to measure the UTB reserve but are not subject to the enhanced UTB disclosure requirements. Using a difference-in-difference research design, we provide evidence consistent with our expectations. Specifically, we find that following the onset of FIN 48, public firms decrease patent applications, and this decrease is more attributable to patent applications about incremental innovation than radical innovation.

These findings are robust to numerous alternative specifications, e.g., an alternative definition of innovation activities, a shorter time window, and using Canadian listed firms as an alternative control group. We provide evidence consistent with our proposed mechanism that the change in disclosures drives our findings. We also provide evidence that our results are more concentrated amongst firms less likely to be audited by the IRS before FIN 48 and firms that more likely face greater IRS scrutiny after FIN 48 due to having a large UTB reserve. Taken together, our evidence suggests that UTB disclosures under FIN 48 adversely affect corporate innovation.

Even though the number of patents decreases due to the disclosure, we are agnostic about its welfare effects. While corporate innovation is encouraged by the U.S. government via the R&E tax credit, it can also lead to wasteful spending by firms. Moreover, we document a decline in incremental innovation following FIN 48 while radical innovation is unaffected. Future research can examine the long-term effects of these actions.

APPENDIX A

Examples of Disclosures following FIN 48

The Boeing Company: 10 –K 12/31/2009:

A reconciliation of the beginning and ending amount of unrecognized tax benefits is as follows:

	2009	2008
Unrecognized Tax Benefits – January 1,	**$1,453**	$1,272
Gross increases – tax positions in prior periods	**219**	88
Gross decreases – tax positions in prior periods	**(31)**	(28)
Gross increases – current-period tax positions	**148**	132
Settlements		(10)
Lapse of statute of limitations	**(2)**	(1)
Unrecognized Tax Benefits – December 31,	**$1,787**	$1,453

As of December 31, 2009 and 2008, the total amount of unrecognized tax benefits was $1,787 and $1,453, of which $1,452 and $1,171 would affect the effective tax rate, if recognized. These amounts are primarily associated with U.S. federal tax issues such as the tax benefits from the Foreign Sales Corporation/Extraterritorial Income (FSC/ETI) tax rules, the amount of research and development tax credits claimed, U.S. taxation of foreign earnings, and valuation issues regarding charitable contributions claimed. Also included in these amounts are accruals for domestic state tax issues such as the allocation of income among various state tax jurisdictions and the amount of state tax credits claimed.

Celgene Corporation: 10-K 12/31/2010:

	2010	2009
Balance at beginning of year	$442,489	$385,840
Increases related to prior year tax positions	9,131	16,322
Decreases related to prior year tax positions	—	—
Increases related to current year tax positions	118,012	76,110
Settlements	(29,292)	(35,783)
Lapse of statute	—	—
Balance at end of year	$540,340	$442,489

These unrecognized tax benefits relate primarily to issues common among multinational corporations. If recognized, unrecognized tax benefits of approximately $504.7 million would have a net impact on the effective tax rate. The Company accounts for interest and penalties related to uncertain tax positions as part of its provision for income taxes. Accrued interest at December 31, 2010 and 2009 is approximately $32.5 million and $21.2 million, respectively.

The Company effectively settled examinations with various taxing jurisdictions in 2010 and 2009. These settlements resulted in decreases in the liability for unrecognized tax benefits related to tax positions taken in prior years of $29.3 million in 2010 and $35.8 million in 2009. The Company has recorded increases in the liability for unrecognized tax benefits for prior years related to ongoing income tax audits in various taxing jurisdictions.

The Company's tax returns are under routine examination in many taxing jurisdictions. The scope of these examinations includes, but is not limited to, the review of our taxable presence in a jurisdiction, our deduction of certain items, our claim for research and development credits, our compliance with transfer pricing rules and regulations and the inclusion or exclusion of amounts from our tax returns as filed. Certain of these examinations are scheduled to conclude within the next 12 months. It is reasonably possible that the amount of the liability for unrecognized tax benefits could change by a significant amount during the next 12-month period. Finalizing examinations with the relevant taxing authorities can include formal administrative and legal proceedings and, as a result, it is difficult to estimate the timing and range of possible changes related to our unrecognized tax benefits. An estimate of the range of the possible change cannot be made until issues are further developed or examinations close.

Cisco Systems Inc.: 10-K 7/30/2011

(b) Unrecognized Tax Benefits

The aggregate changes in the balance of gross unrecognized tax benefits were as follows (in millions):

Years Ended	July 30, 2011	July 31, 2010	July 25, 2009
Beginning balance	**$ 2,677**	$ 2,816	$ 2,505
Additions based on tax positions related to the current year	**374**	246	190
Additions for tax positions of prior years	**93**	60	307
Reductions for tax positions of prior years	**(60)**	(250)	(17)
Settlements	**(56)**	(140)	(109)
Lapse of statute of limitations	**(80)**	(55)	(60)
Ending balance	**$ 2,948**	$ 2,677	$ 2,816

As of July 30, 2011, $2.6 billion of the unrecognized tax benefits would affect the effective tax rate if realized. During fiscal 2011, the Company recognized $38 million of net interest expense and $9 million of penalties. During fiscal 2010, the Company recognized $167 million of net interest income and $5 million of penalties. The Company's total accrual for interest and penalties was $214 million and $167 million as of the end of fiscal 2011 and 2010, respectively. The Company is no longer subject to U.S. federal income tax audit for returns covering tax years through fiscal 2001. With limited exceptions, the Company is no longer subject to state and local or foreign income tax audits for returns covering tax years through fiscal 1997.

During fiscal 2010, the Ninth Circuit withdrew its prior holding and reaffirmed the 2005 U.S. Tax Court ruling in *Xilinx, Inc. v. Commissioner*. As a result of this final decision in fiscal 2010, the Company decreased the amount of gross unrecognized tax benefits by approximately $220 million and decreased the amount of accrued interest by $218 million, which effectively reversed a similar amount that was recorded as an increase in the unrecognized tax benefits during fiscal 2009 as a result of the Ninth Circuit's initial decision.

The Dow Chemical Company: 10-K 12/31/2015

Total Gross Unrecognized Tax Benefits			
In millions	*2015*	*2014*	*2013*
Balance at January 1	$ 240	$ 266	$ 409
Increases related to positions taken on items from prior years	92	42	385
Decreases related to positions taken on items from prior years	(6)	(57)	(137)
Increases related to positions taken in the current year	10	10	10
Settlement of uncertain tax positions with tax authorities	(56)	(13)	(393)
Decreases due to expiration of statutes of limitations	—	(8)	(8)
Balance at December 31	$ 280	$ 240	$ 266

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At December 31, 2015, the total amount of unrecognized tax benefits was $280 million ($240 million at December 31, 2014), of which $206 million would impact the effective tax rate, if recognized ($233 million at December 31, 2014).

Interest and penalties are recognized as components of the "Provision for income taxes," and totaled a charge of $80 million in 2015, a charge of $15 million in 2014 and a benefit of $71 million in 2013. The Company's accrual for interest and penalties associated with uncertain tax positions was $159 million at December 31, 2015 and $109 million at December 31, 2014.

During 2013, court rulings on two separate tax matters resulted in the adjustment of uncertain tax positions. In February 2013, the U.S. District Court for the Middle District of Louisiana issued a ruling that disallowed, for tax purposes, transactions and partnerships associated with Chemtech, a wholly owned subsidiary. In March 2013, the U.S. Supreme Court denied *certiorari* in Union Carbide's research tax credit case. Through denial of *certiorari*, the decision issued by the U.S. Court of Appeals denying Union Carbide's tax credit claim for supplies used in process-related research and development at its manufacturing facilities became final. As a result of these rulings, the Company adjusted uncertain tax positions related to these matters, resulting in a tax charge of $276 million in 2013.

A rollforward of our unrecognized tax benefits was as follows:

(In millions)	2010	2009	2008
Unrecognized tax benefits, beginning of year	**$ 469**	$415	$342
Additions based on current year tax positions	**14**	20	36
Additions based on prior year tax positions	**32**	34	38
Reductions based on prior year tax positions	**(317)**	—	—
Settlements with taxation authorities	**(10)**	—	(1)
Unrecognized tax benefits, end of year	**$ 188**	$469	$415

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NOTES TO CONSOLIDATED FINANCIAL STATEMENTS (CONTINUED)

We generally account for our state income tax expense as a deferred contract cost, as we can generally recover this expense through the pricing of our products and services to the U.S. Government. We include this deferred contract cost in contracts in process until allocated to our contracts, which generally occurs upon payment or when otherwise agreed as allocable with the U.S. Government. Net state income tax expense allocated to our contracts was $59 million, $25 million and $122 million in 2010, 2009 and 2008, respectively. We include state income tax expense allocated to our contracts in administrative and selling expenses.

The American Jobs Creation Act of 2004 provides a deduction for income derived from qualifying domestic production activities (the Domestic Manufacturing Deduction under Section 199 of the Internal Revenue Code (IRC)) that is phased in over the 2005–2010 period. The deduction was equal to 9% of qualified income in 2010, and 6% in 2009 and 2008.

In December 2010, the Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010 was enacted. This legislation retroactively reinstated the research and development tax credit for 2010 and extended it through December 31, 2011. As a result, we recorded a benefit of approximately $26 million in the fourth quarter of 2010 representing the benefit of the research and development tax credit for the full year.

APPENDIX B

Variable Definitions

Variable Name	Description
Dependent Variables	
Patent_Count	Natural logarithm of one plus the number of unique patents based on data from the OECD Orbis database.
Radical_Count	Natural logarithm of one plus the number of unique patents most likely to be considered radical innovation. A radical innovation is a patent application (as obtained from the OECD Orbis database) with zero backward citations.
Incremental_Count	Natural logarithm of one plus the number of unique patents most likely to be considered incremental innovation. An incremental innovation is a patent application (as obtained from the OECD Orbis database) with one or more backward citations.
ScaledPatent_Count	Number of unique patents per year scaled by total assets of the firm (in US$ billions).
Independent Variables	
Public	Dummy variable that is equal to 1 for publicly held firms and 0 otherwise.
Post-FIN 48	Dummy variable that is equal to 1 for years after 2006 and 0 otherwise.
USA	Dummy variable that is equal to 1 for U.S. publicly held firms and 0 otherwise. We use this dummy variable in the analysis with Canadian public firms as an alternative control group instead of the *Public* dummy variable.
Control Variables Primary Analysis	
Size	Natural logarithm of total assets (AT). We winsorize size at the 1 and 99 percentiles.
ROA	Return on assets, defined as operating income before depreciation (OIBDP) scaled by average total assets (AT) for public firms and earnings over average total assets for private firms. We winsorize *ROA* at -0.5 and 0.5.
ΔSales	Growth, defined as change in sales (SALE) scaled by prior year sales. We winsorize sales at -1 and 1.
NegROA	Dummy variable that equals 1 if ROA is negative and 0 otherwise.
NegΔSales	Dummy variable that equals 1 if ΔSales is negative and 0 otherwise.
Control Variables Additional Analysis	
R&D	R&D expenses (XRD) divided by book value of total assets (AT)
Age	Natural logarithm of 1 plus the age of the firm based on the year the firm was founded from the OECD Orbis database, missing values are replaced with 0
Leverage	Book value of debt (DLC + DLTT) divided by book value of total assets (AT)
Cash	Cash (CHE) at the end of fiscal year divided by book value of total assets (AT)
PPE	Property, plant, and equipment (PPENT) divided by book value of total assets (AT)

CAPEX	Capital expenditure (capx) divided by book value of total assets (AT)
BTM	Book-to-market, defined as total assets (AT) over market value (MKVALT)
KZ Index	Kaplan and Zingales Index calculated as -1.002 × cash flow [(IB + DP) /PPENT] plus 0.283 × Tobin's Q plus 3.139 × leverage minus 39.368 × dividends [(DVC + DVP) /PPENT] minus 1.315 × cash holdings (CHE/PPENT), where PPENT is lagged
DACC	Performance matched discretionally accruals, according to (Kothari et al. 2005)
HHI	Herfindahl-Hirschman-Index, calculated as the relative squared sum of revenue (SALE) for industries based on SIC four-digit codes per year.
HHI^2	Squared Herfindahl–Hirschman Index
Additional Table 4 Variables	
Disclosure	An indicator variable equal to 1 if the observation has high tax footnote disclosures and 0 otherwise. We measure disclosure in four ways: (1) *Sentence* = above the median sentence count in the tax footnote (2) *Paragraph* = above the median in average words per paragraph (3) *ComplexParagraph* = above the median in average words per paragraph and complex word count (4) *ComplexSentence* = above the median in average words per sentence and complex word count Data obtained to determine *Disclosure* was formed using textual analysis and obtained directly from firm's tax footnotes in the annual 10-K SEC filing.
IncreaseUTB	An indicator variable equal to 1 if the observation incurs an increase in UTBs as a result of their FIN 48 adoption, and 0 otherwise. Data obtained to determine *IncreaseUTB* is obtained directly from Audit Analytics and ties to the Audit Analytics (2008) report.
DecNoChangeUTB	An indicator variable equal to 1 if the observation incures a decrease or no change in UTBs as a result of their FIN 48 adoption, and 0 otherwise. Data obtained to determine *DecNoChangeUTB* is obtained directly from Audit Analytics and ties to the Audit Analytics (2008) report.
Additional Table 5 Variables	
SmallFirm	Dummy variable equal to 1 if the observation is below the median for *Size*, and 0 otherwise
LowUTB	Dummy variable that is equal to 1 for mean Uncertain Tax Benefits (UTB) per firm below the sample median UTB and 0 otherwise, where we define UTBs as end-year UTB (txtubend) over total assets (AT)

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TABLE 1: Sample and Descriptive Statistics

Panel A: Sample Selection Procedure

	Firms	Obs.
Firms with OECD Orbis patent data matched to Compustat and Capital IQ from 2003 to 2011	4,205	20,383
Less firms with missing or total assets less than 10 million	2,704	14,014
Less financial firms	2,610	13,357
Less firms in fiscal-year 2007	2,576	11,864
Less firms that entered the sample in 2007 or later	2,386	11,168
Less U.S. private firms with listed U.S. parent corporations during sample period	2,380	11,141
Less firms with missing control variables and firms with control variables not at least once in post-FIN 48 period	1,424	8,480
Final Sample	1,150	7,586

Panel B: Sample Composition by Treatment and Control Groups

	Pre-FIN 48 = 2003 To 2006		**Post-FIN 48 = 2008 To 2011**	
	# of Firms	**Total # of Obs**	**# of Firms**	**Total # of Obs**
Public	885	3,339	975	3,679
Private	107	165	175	403
Total	992	3,504	1,150	4,082

Panel C: Descriptive Statistics of Explanatory Variables in Equation (1)

	N	**Mean**	**Std. Dev.**	**P25**	**P50**	**P75**
Public	7,586	0.925	0.263	1	1	1
Post-FIN48	7,586	0.538	0.499	0	1	1
Size	7,586	6.147	2.256	4.303	5.966	7.803
ROA	7,586	0.067	0.194	0.021	0.107	0.17
ΔSales	7,586	0.125	0.319	-0.016	0.088	0.227
NegROA	7,586	0.219	0.413	0	0	0
NegΔSales	7,586	0.280	0.449	0	0	1

Panel D: Descriptive Statistics of Control Variables for Public and Private Firms

	Pre-FIN 48 = 2003 To 2006		**Post-FIN 48 = 2008 To 2011**		**DIFFERENCE = Post-FIN 48 Minus Pre-FIN 48**	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Difference**	**t-Statistic**
Public:						
Size	6.241	2.186	6.365	2.266	**0.124****	**2.328**
ROA	0.070	0.196	0.059	0.197	**-0.011****	**-2.364**
ΔSales	0.166	0.304	0.084	0.320	**-0.081*****	**-10.904**
NegROA	0.216	0.412	0.228	0.420	**0.012**	**1.246**
NegΔSales	0.189	0.391	0.363	0.481	**0.174*****	**16.566**
Private:						
Size	4.052	1.539	4.243	1.574	**0.191**	**1.321**
ROA	0.114	0.156	0.087	0.152	**-0.027***	**-1.891**
ΔSales	0.178	0.315	0.136	0.380	**-0.042**	**-1.264**
NegROA	0.133	0.341	0.191	0.394	**0.058**	**1.648**
NegΔSales	0.170	0.377	0.323	0.468	**0.153*****	**3.730**

Notes to Table 1:
Panel A provides the number of firms and firm-years, i.e., observations.
Panel B provides the sample composition by public, i.e., treatment group and private, i.e., control groups.
Panel C presents the descriptive statistics of explanatory variables.
Panel D presents the differences in means for the explanatory variables across pre- and post-FIN 48 periods, separately for public and private firms.
Variable definitions of the variables are in Appendix B. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 2: Patent Activity for Public Firms and Private Firms, Before and After FIN 48

Panel A: *Patent_Count,* Univariate Tests

	Pre-FIN 48 = 2003 To 2006		Post-FIN 48 = 2008 To 2011		DIFFERENCE = Post-FIN 48 Minus Pre-FIN 48	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Difference**	**t-Statistic**
Public	1.468	1.622	1.309	1.534	**-0.159*** **	**-4.222**
Private	0.500	0.759	0.613	0.807	**0.114**	**1.553**
DIFFERENCE Public Minus Private	**0.969*** **		**0.696*** **			
t-Statistic	**7.631**		**8.970**			

Panel B: Results of Estimating Equation (1)

Dependent Variable = *Patent_Count*	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**
Public	0.969***	0.219**
	(10.74)	(2.077)
Post-FIN48	0.114**	0.026
	(2.084)	(0.387)
Public×Post-FIN48	**-0.273*** **	**-0.224*** **
	(-4.590)	**(-3.229)**
Size		0.321***
		(11.630)
ROA		-0.594***
		(-2.698)
ΔSales		0.129*
		(1.925)
NegROA		0.282***
		(2.974)
NegΔSales		-0.000
		(-0.002)
Observations	7,586	7,586
Adjusted R-Squared	0.021	0.185

Notes to Table 2:

Panel A presents the difference in means of *Patent_Count.*

Panel B presents the results of estimating Equation (1) without control variables in column (1) and with control variables in column (2).

Equation (1) is given below.

$$Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + \varepsilon_{i,t}$$

Variable definitions of the variables are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 3: Radical and Incremental Patent Activity for Public and Private Firms, Before and After FIN 48

Panel A: Radical and Incremental *Patent_Count*, Univariate Tests

	Pre-FIN 48 = 2003 To 2006		**Post-FIN 48 = 2008 To 2011**		**DIFFERENCE = Post-FIN 48 Minus Pre-FIN 48**	
	Radical	**Incremental**	**Radical**	**Incremental**	**Difference Radical (t-Statistic)**	**Difference Incremental (t-Statistic)**
Public	0.574	1.543	0.568	1.359	**-0.006 (-0.120)**	**-0.183*** (-4.597)**
Private	0.102	1.160	0.112	1.248	**0.010 (0.262)**	**0.088 (0.740)**
Difference Public Minus Private (t-Statistic)	**0.472*** (8.035)**	**0.383* (1.812)**	**0.456*** (10.157)**	**0.111 (0.943)**		

Panel B: Results of Estimating Equation (1) for Radical and Incremental Patents

Dependent Variable =	***Radical_Patent_Count***	***Incremental_Patent_Count***
	Coefficient (t-Statistic) (1)	**Coefficient (t-Statistic) (2)**
Public	-0.015	0.234**
	(-0.770)	(2.186)
Post-FIN48	0.000	0.025
	(0.020)	(0.373)
Public×Post-FIN48	**-0.007**	**-0.218*** **
	(-0.287)	**(-3.073)**
Size	-0.002	0.323***
	(-1.343)	(11.568)
ROA	0.016	-0.611***
	(0.513)	(-2.738)
ΔSales	-0.004	0.133*
	(-0.299)	(1.952)
NegROA	0.011	0.272***
	(0.900)	(2.831)
NegΔSales	-0.012	0.011
	(-1.411)	(0.212)
Observations	7,586	7,586
Adjusted R-Squared	0.001	0.184

Notes to Table 3:

Panel A presents the difference in means of *Radical_Patent_Count* and *Incremental_Patent_Count.*

Panel B presents the results of estimating Equation (1) separately for *Radical_Patent_Count* and *Incremental_Patent_Count.* Equation (1) is given below.

$$X\_Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + \varepsilon_{i,t}$$

for *X = {Radical, Incremental}.* Column (1) presents the results with *Radical_Count* and Column (2) presents the results with *Incremental_Count.* Variable definitions of the variables are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 4: Patent Activity for Before and After FIN 48: UTB Disclosure vs. UTB Measurement Tests

Panel A: High versus Low Disclosure, Public Firms Only

Dependent Variable = ***Patent_Count***	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**	**Coefficient (t-Statistic) (3)**	**Coefficient (t-Statistic) (4)**
	Disclosure = Sentence	***Disclosure = Paragraph***	***Disclosure = ComplexParagraph***	***Disclosure = ComplexSentence***
Disclosure	0.760***	0.153*	0.210**	0.234***
	(7.696)	(1.764)	(2.101)	(2.585)
Post-FIN48	-0.0913**	-0.102**	-0.137***	-0.120***
	-(2.105)	(-2.153)	(-3.550)	(-2.816)
Disclosure×Post-FIN48	**-0.211*** **	**-0.192** **	**-0.188** **	**-0.163** **
	(-2.817)	**(-2.301)**	**(-1.980)**	**(-2.101)**
Controls	Yes	Yes	Yes	Yes
Observations	6,609	6,609	6,609	6,609
Adjusted R-Squared	0.180	0.179	0.179	0.180

Panel B: Increases versus Decreases/No Change in UTBs due to FIN 48, Public Firms Only

Dependent Variable = ***Patent_Count***	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**
	Increase UTB	**Decrease/No Change UTB**
Post-FIN48	**-0.173*** **	**-0.299*** **
	(-3.546)	**(-4.819)**
Controls	Yes	Yes
Observations	2,431	1,385
Adjusted R-Square	0.161	0.226

Notes to Table 4:

Table 4 presents the results of estimating Equation (1) for various cross-sectional tests.

Panel A presents our analysis when examining firms with high versus low disclosure. Our proxy for disclosure is an indicator variable that indicates if the observation has an above median sentence count (*Sentence*), above median words per paragraph (*Paragraph*), above the median average words per paragraph and complex word count *(ComplexParagrapgh*), and above the median average words per sentence and complex word count (*ComplexSentence)*.

Panel B partitions the sample by whether firms increased their UTB reserve in response to FIN 48 or did not increase their UTB reserve in response to FIN 48 (Decrease / No Change UTB)

Equation (1) is given below:

$$Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + \varepsilon_{i,t}$$

For Panel A, we modify Equation (1) to replace *Public* with *Disclosure* and the interaction term as *Disclosure* × *Post-FIN 48* to capture the cross-sectional variation in disclosure pre vs. post-FIN 48. For Panel B, we do not include *Public* because all observations in our sample are publicly traded firms.

Variable definitions are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 5: Patent Activity for Public and Private Firms, Before and After FIN 48: Cross-Sectional Tests

Panel A: Small versus Large Firms

Dependent Variable = *Patent_Count*	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**
	Small Firms (*SmallFirm* = 1)	**Large Firms (*SmallFirm* = 0)**
Public	0.139**	0.540
	(2.000)	(1.525)
Post-FIN48	0.051	-0.144
	(0.730)	(-1.136)
Public×Post-FIN48	**-0.194****	**-0.113**
	(-2.583)	**(-0.885)**
Controls	Yes	Yes
Observations	3,799	3,787
Adjusted R-Squared	0.146	0.077

Panel B: Low versus High UTBs, Public Firms Only

Dependent Variable = *Patent_Count*	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**
	Low UTBs (*LowUTB* = 1)	**High UTBs (*LowUTB* = 0)**
Post-FIN48	**-0.165*****	**-0.302*****
	(-4.016)	**(-6.414)**
Controls	Yes	Yes
Observations	2,869	2,854
Adjusted R-Square	0.134	0.206

Panel C: Profit versus Loss Firms

Dependent Variable = *Patent_Count*	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**
	Loss Firms (*NegROA* = 1)	**Profitable Firms (*NegROA* = 0)**
Public	0.965***	0.057
	(3.981)	(0.538)
Post-FIN48	0.181	0.044
	(1.024)	(0.600)
Public×Post-FIN48	**-0.476****	**-0.215*****
	(-2.569)	**(-2.754)**
Controls	Yes	Yes
Observations	1,660	5,926
Adjusted R-Squared	0.203	0.183

Notes to Table 5:

Table 5 presents the results of estimating Equation (1) for various cross-sectional partitions.
Panel A partitions the sample by the median *Size* for public and private firms separately (*SmallFirm = 1* vs. *SmallFirm = 0)*.
Panel B partitions the sample by the median level of Uncertain Tax Benefit scaled by Total Assets (*LowUTB = 1* vs. *LowUTB =0)*.
Panel C partitions the sample by whether the firm is in a loss state. (*NegROA* = 1 vs. *NegROA* = 0).
Equation (1) is given below:

$$Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + \varepsilon_{i,t}$$

Variable definitions are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 6: Patent Activity for U.S. and Canadian Public Firms, Before and After FIN 48

Panel A: Sample Composition by Treatment and Control Groups

	Pre-FIN 48 = 2003 To 2006		Post-FIN 48 = 2008 To 2011	
	# of Firms	**Total # of Obs**	**# of Firms**	**Total # of Obs**
U.S.	885	3,339	975	3,679
Canadian	69	244	86	323
Total	954	3,583	1,061	4,002

Panel B: *Patent_Count,* Univariate Tests

	Pre-FIN 48 = 2003 To 2006		Post-FIN 48 = 2008 To 2011		DIFFERENCE = Post-FIN 48 Minus Pre-FIN 48	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Difference**	**t-Statistic**
U.S.	1.468	1.622	1.309	1.534	**-0.159*** **	**-4.222**
Canadian	0.746	1.086	0.663	1.058	**-0.083**	**-0.919**
DIFFERENCE U.S. Minus Canadian (t-Statistic)	**-0.722*** (-6.844)**		**-0.647*** (-7.421)**			

Panel C: Patent Activity for U.S. Public and Canadian Public Firms

Dependent Variable = *Patent_Count*	Coefficient (t-Statistic) (1)
USA	0.543***
	(4.146)
Post-FIN48	-0.139*
	(-1.761)
USA×Post-FIN48	**-0.168****
	(-2.062)
Size	0.394***
	(14.425)
ROA	0.446*
	(1.690)
ΔSales	0.133**
	(2.008)
NegROA	0.218**
	(2.432)
NegΔSales	0.128**
	(2.577)
Leverage	-0.323***
	(-2.666)
Cash	0.645***
	(4.064)
PPE	-2.163***
	(-8.198)
CAPEX	4.283***
	(4.916)
R&D	2.281***
	(8.329)
DACC	-0.000
	(-0.008)
KZ Index	0.000***
	(3.174)
BTM	-0.021
	(-0.832)
Age	0.165***
	(3.247)
HHI	-0.017
	(-0.028)
HHI^2	0.427
	(0.700)
Observations	7,585
Adjusted R-Squared	0.295

Notes to Table 6:
Panel A provides the sample composition by U.S. public, i.e., treatment group, and Canadian public, i.e., control group.
Panel B presents the difference in means of *Patent_Count.*
Panel C presents the results of estimating a modified version of Equation (1) for U.S. public and Canadian public firms. The modified version of Equation (1) is given below:

$$Patent\_Count_{i,t} = \alpha + \beta_1 USA_i + \beta_2 Post\text{-}FIN48_t + \beta_3 USA_i \times Post\text{-}FIN48_t + Controls + \varepsilon_{i,t}$$

USA is an indicator variable that is 1 for U.S. public firms and zero for Canadian public firms. Variable definitions are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 7: Patent Activity for Public and Private Firms, Before and After FIN 48: Entropy Balancing and Coarsened Exact Matching

Panel A: Entropy Balancing on Two Moments, Descriptive Statistics Before and After Entropy Balancing

	Treatment: Public Firms		Control: Private Firms	
Before Entropy Balancing	**Mean**	**Variance**	**Mean**	**Variance**
Size	6.306	4.969	4.187	2.450
ROA	0.065	0.039	0.095	0.024
ΔSales	0.123	0.099	0.148	0.131
NegROA	0.222	0.173	0.174	0.144
NegΔSales	0.280	0.202	0.278	0.201
After Entropy Balancing				
Size	6.306	4.969	6.305	4.970
ROA	0.065	0.039	0.065	0.039
ΔSales	0.123	0.099	0.123	0.099
NegROA	0.222	0.173	0.222	0.173
NegΔSales	0.280	0.202	0.280	0.202

Panel B: Results of Estimating Equation (1) and Matching Methods

Dependent Variable = *Patent_Count*	**Entropy Balancing**	**Coarsened Exact Matching**	**Coarsened Exact Matching & Entropy Balancing**
	Coefficient (t-Statistic)	**Coefficient (t-Statistic)**	**Coefficient (t-Statistic)**
Public	0.852***	0.593***	0.548***
	(4.849)	(3.047)	(3.956)
Post-FIN48	0.187	0.228	0.064
	(1.482)	(1.245)	(0.802)
Public×Post-FIN48	**-0.346*** **	**-0.438** **	**-0.271*** **
	(-2.785)	**(-2.293)**	**(-2.806)**
Size	0.164***	0.252***	0.153***
	(4.526)	(9.235)	(4.303)
ROA	0.193	-0.703***	-0.265
	(0.451)	(-3.214)	(-0.787)
ΔSales	0.081	0.099	-0.003
	(0.447)	(0.991)	(-0.025)
NegROA	-0.177	0.234**	0.011
	(-0.575)	(2.340)	(0.081)
NegΔSales	-0.052	-0.088	-0.019
	(-0.549)	(-1.082)	(-0.188)
Observations	7,586	2,164	2,164
Adjusted R-Squared	0.147	0.164	0.098

Notes to Table 7:
Panel A provides the results of our balancing procedure for the entropy balancing.
Panel B presents the results of estimating Equation (1) using entropy balancing in column (1), coarsened exact matching in column (2), and entropy balancing and coarsened exact matching in column (3).
Equation (1) is given below.

$$Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + \varepsilon_{i,t}$$

Variable definitions are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

TABLE 8: Patent Activity for Public and Private Firms, Before and After FIN 48: Additional tests

Panel A: All Orbis Firms

Dependent Variable = ***Patent_Count***	**Coefficient (t-Statistic) (1)**	**Coefficient (t-Statistic) (2)**
Public	1.796***	
	(35.531)	
Post-FIN48	-0.034***	
	(-16.029)	
Public×Post-FIN48	**-0.212*** **	**-0.069*** **
	(-8.247)	**(-3.001)**
Controls	No	No
Year FE	No	Yes
Firm FE	No	Yes
Observations	319,018	319,018
Adjusted R-Squared	0.122	0.583

Panel B: Patent Activity Scaled Firm Assets

Dependent Variable = ***ScaledPatent_Count***	**Coefficient (t-Statistic)**
Public	0.505***
	(2.975)
Post-FIN48	0.249
	(1.434)
Public×Post-FIN48	**-0.483*** **
	(-2.736)
Controls	Yes
Observations	7,586
Adjusted R-Squared	0.123

Panel C: Results of Estimating Equation (1) with Two Year in Pre and Post Period Only

Dependent Variable = ***Patent_Count***	**Coefficient (t-Statistic) (1)**
Public	0.186
	(1.550)
Post-FIN48	-0.030
	(-0.301)
Public×Post-FIN48	**-0.177***
	(-1.728)
Controls	Yes
Observations	1,947
Adjusted R-Squared	0.183

Notes to Table 8:

Panel A provides the results of estimating Equation (1) without control variables and without and with year and firm fixed effects in Columns (1) and (2), respectively.

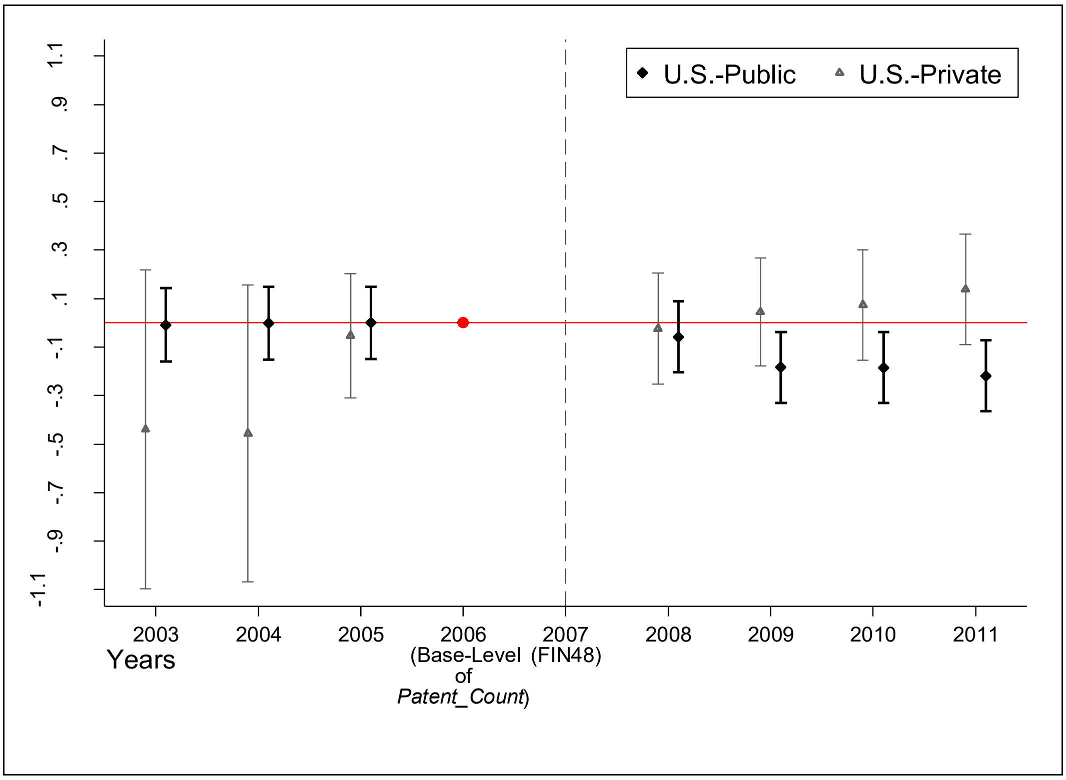
Panel B provides the results of estimating Equation (1) using the dependent variable Patent Count scaled by Firm Assets.

Panel C provides the results of estimating Equation (1) using only two years before and after FIN 48. Equation (1) is given below:

$$Patent\_Count_{i,t} = \alpha + \beta_1 Public_i + \beta_2 Post\text{-}FIN48_t + \beta_3 Public_i \times Post\text{-}FIN48_t + \beta_4 Size_{i,t} + \beta_5 ROA_{i,t} + \beta_6 \Delta Sales_{i,t} + \beta_7 NegROA_{i,t} + \beta_8 Neg\Delta Sales_{i,t} + \varepsilon_{i,t}$$

Variable definitions are in Appendix B. Standard errors are clustered at firm-level. ***, **, and * indicates statistical significance at the 1%, 5%, and 10% level using two-tailed p-values.

FIGURE 1: Parallel Trend Pattern of Patent Activity for U.S. Public and U.S. Private Firms by Year



Notes to Figure 1:
Figure 1 presents the graph of parallel trends for both U.S. public and U.S. private firms where we plot the estimated results of regressing *Patent_Count* on year dummy variables for each year from 2003 to 2011 with year 2006 as the baseline year of innovation activity, as measured through patent applications, for each group before FIN 48 was introduced as well as the two-tailed 95% confidence intervals of the estimates.