ARE FINANCIAL STATEMENTS MORE COMPARABLE WHEN GAAP LIMITS MANAGERS' DISCRETION?

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ABSTRACT

This study examines whether financial statements are more comparable when accounting standards limit managers' discretion. I measure the extent to which GAAP limits managers' discretion by counting the number of times obligatory language is used in each standard. My primary findings suggest financial statements are less comparable when GAAP limits managers' discretion and that this effect is driven by firm-pairs that have dissimilar transactions. Further, using SFAS 141 and SFAS 86 as salient examples, I provide evidence that accounting for similar transactions using similar accounting treatments, as required by SFAS 141, is associated with more comparable financial statements, yet accounting for dissimilar transactions using similar accounting treatments, as required by SFAS 86, is associated with less comparable financial statements. Last, I examine how the impact of limiting discretion on comparability affects the cost of equity. My findings suggest that the cost of equity is higher when GAAP limits managers' discretion and that a substantial portion of this increase is driven by the decline in comparability associated with limiting managers' discretion.

CHAPTER 1. INTRODUCTION

I examine whether financial statements become more comparable when GAAP limits managers' discretion. The FASB defines comparability as "the qualitative characteristic that enables users to identify and understand similarities in, and differences among, items" (Concept Statement 8, p. 19). Concept Statement 8 and empirical evidence suggest that more comparable financial reports enhance the information environment and allow more efficient capital allocation (e.g., De Franco, Kothari, and Verdi 2011; Chen, Collins, Kravet, and Mergenthaler 2018). Hence, researchers and standard setters have a salient interest in understanding how the structure of accounting standards impacts financial statement comparability. A common assertion is that restrictive accounting standards enhance comparability because they standardize accounting treatments across firms by requiring similar accounting treatments for a specified set of transactions.[1] However, standardization does not guarantee more comparable financial reports (Schipper 2003). Comparable financial reports use similar accounting treatments for similar transactions *and* dissimilar treatments for dissimilar transactions (Concept Statement 8).

I define a GAAP limitation as a regulatory injunction that limits financial statement preparers' choice over (1) what information to provide or (2) which accounting methods to use when preparing public financial statements. These limitations are a central mechanism through which standard setters can limit opportunistic accounting choices, enhance comparability, and influence the information in mandatory disclosures. While comparability is one attribute of useful information, the primary objective of GAAP, and by association GAAP limitations, is to provide useful information to investors when "making decisions about providing resources to the entity" (Concept Statement 8, p. 1). Further, Concept Statement 8 asserts that capital will be allocated

[1] Without stating whether limiting managers' discretion improves comparability, Schipper (2003) states "I emphasize that detailed guidance is intended to achieve comparability" (p. 71).

more efficiently and the cost of equity will be lower if this objective is achieved. In this study, I also examine whether GAAP limitations yield a lower cost of capital via enhanced comparability.

It is unclear whether FASB-mandated GAAP limitations improve comparability. Arguably, managers are best positioned to determine the appropriate accounting treatment because they have intimate knowledge of the details of each transaction. Hence, limiting managers' judgment could impede their ability to capture the underlying economics of a transaction faithfully. On the other hand, the FASB can play a coordinating role by requiring that similar transactions are accounted for using similar accounting treatments. Ultimately, the relation between GAAP limitations and comparability relies on the relative abilities and incentives of managers and the FASB to account for similar transactions using similar treatments and to account for dissimilar transactions using dissimilar treatments.

To test the relation between GAAP limitations and comparability, I measure GAAP limitations as the number of regulatory injunctions that apply to a firm in a given year. To measure the number of limitations in each GAAP standard, I follow Hribar, Mergenthaler, Roeschley, Young, and Zhao (2020) by counting the number of times the verbs *shall*, *should*, and *must* appear in each standard. The intuition behind this measure is that standards that use more of these restrictive verbs limit the manager's discretion to a greater extent. This measure varies over time as standards are amended or as implementation guidance is issued by standard setters or regulators. Hribar et al. (2020) conduct several tests to validate this measure (see Section 3). Next, since firms differ in their exposure to various accounting standards, I follow Folsom et al. (2017) and measure a firm's reliance on each standard by counting the number of times firms mention keywords associated with each standard in their 10-K. My GAAP limitations measure, which I calculate for

each firm-year, is the sum of all standard level GAAP limitations, weighted by each firm's Folsom et al. (2017) reliance measure.

I measure comparability following De Franco et al. (2011). For a comprehensive set of firms from 1993 to 2016, I find GAAP limitations are associated with less comparable financial statements. This result is robust to controlling for time-invariant firm characteristics. Further, additional analyses suggest that this result is not driven by year-over-year changes in firms' underlying economics. Given that firms are subject to many GAAP limitations and that the nature of these limitations varies from one limitation to the next, this result suggests that the average limitation imposed by GAAP inhibits comparability.

Perhaps more importantly, I examine whether the effect of GAAP limitations on comparability varies with transaction dissimilarity. I find that GAAP limitations inhibit financial statement comparability to a greater extent when firm pairs have dissimilar transactions (Hoberg and Phillips 2016 product-space similarity score serving as proxy). This result provides evidence consistent with the central premise of this paper, which is that comparability is enhanced when "like things…look alike and different things…look different" (Concept Statement 8, p. 19).

To provide stronger identification around this result, I examine two standard changes (i.e., SFAS 141 and SFAS 86) that serve as salient examples. First, I focus on SFAS 141 and examine whether GAAP limitations improve comparability when the underlying transactions are similar. In this setting, I examine whether comparability was inhibited by the availability of multiple accounting treatments for similar transactions. Under APB 16 "similar business combinations were accounted for using different methods [i.e., pooling or purchase method] that produced dramatically different financial statement results" (SFAS 141, p. 5). In contrast, SFAS 141 required firms to account for business combinations with a single treatment (i.e., purchase

method). To test whether the use of similar accounting methods for similar transactions improves comparability, I measure treatment similarity, within each industry-year, as the number of firm pairs where both firms had a merger and used similar treatments divided by the number of firm pairs where both firms had a merger. I find comparability is higher when firm pairs use similar accounting treatments for business combinations, *as required by* SFAS 141, than when firm pairs use dissimilar treatments.

Next, I focus on SFAS 86 and examine whether limiting managers' discretion can reduce comparability when the underlying transactions are dissimilar. Prior to SFAS 86, certain firms elected to capitalize software development costs (see Appendix A of SFAS 86) but after SFAS 86 became effective firms were required to expense all software development costs until technological feasibility was attained, regardless of the likelihood that such expenditures would be recoverable in future years. To test how requiring managers to expense these costs, regardless of their unique attributes, impacted comparability, I perform a difference-in-differences analysis around the passage of SFAS 86 and find that comparability was reduced more for firms that 1) engaged in research and development relative to other firms and 2) engaged in software development relative to firms engaged in research and development generally. These analyses examining SFAS 141 and SFAS 86 provide stronger evidence that limiting the number of acceptable accounting methods when transactions are similar (dissimilar) can enhance (reduce) financial statement comparability.

Finally, I examine whether the cost of capital is affected by the decline in comparability associated with GAAP limitations. Consistent with Concept Statement 8, comparability may lower the cost of capital via two related mechanisms. First, more comparable financial statements may facilitate comparisons between alternative investment decisions and thereby decrease processing

costs for investors (Barth, Clinch, and Shibano 1999; Li 2010). Second, more comparable financial statements may yield positive information externalities and thereby decrease estimation risk (Dye 1990; De Franco et al. 2011). To examine this relation, I measure the cost of equity using the average of several implied cost of equity estimates (Dhaliwal, Judd, Serfling, and Shaikh 2016; Hail and Leuz 2006). I find the cost of equity is higher when GAAP limits managers' discretion. Specifically, a one standard deviation increase in GAAP limitations is associated with a 6 to 12 basis point increase in the cost of equity. Approximately 21 percent of this relation is explained by the impact of GAAP limitations on comparability. However, I also find the marginal GAAP limitation is negatively associated with the cost of equity when managers have incentives to manipulate.

This study faces the empirical concern that firm attributes and GAAP limitations may arise because of the attributes of the underlying transaction. For example, it is plausible more complex transactions require more guidance from standard setters and hence have more limitations. I take several steps to address this concern. First, I include industry fixed effects to control for variation in attributes of transactions across industries in all analyses. Second, I follow Donelson et al. (2012) and orthogonalize the number of limitations in each standard to two measures of transaction complexity in all analyses. In additional analyses, I address this concern by (1) scaling my standard-year measure of limitations by the length of the standard, (2) controlling for disclosure complexity using the FOG index (Li 2008) and (3) controlling for the number of standards a firm relies upon in a given year. Further, in additional analyses, I include firm *and* industry-by-year fixed effects. In all of these robustness tests, I continue to find GAAP limitations are negatively associated with comparability and positively associated with the cost of equity.

The evidence presented in this study is subject to an important caveat. Before issuing accounting standards, the FASB undergoes an extensive process, considering the impact of potential standards on a diverse group of constituents. The evidence in this paper cannot speak to the effect of GAAP limitations on all stakeholders, nor can it speak to the effects of specific changes to accounting standards. This paper provides evidence of the effect of the average GAAP limitation on comparability and the cost of equity. Consistent with the evidence in this study, certain limitations may improve comparability and the cost of equity while others may not. Further research is needed to identify the costs and benefits of specific changes to GAAP.

While research has provided evidence suggesting that *within-firm* earnings relevance is lower when GAAP limits managers' discretion (Folsom et al. 2017), this study contributes to our understanding of how GAAP limitations impact the comparison of earnings *across firms* and how these across firm comparisons impact the cost of capital. First, I provide evidence that financial statement comparability decreases as GAAP limitations increase and that this relation varies predictably with pair-wise transaction dissimilarity. However, I also find that requiring consistent accounting treatments for similar transactions can enhance comparability. Second, I find the cost of equity is higher when GAAP is more limiting. Third, I find the decline in comparability associated with GAAP limitations drives a substantial portion of the relation between GAAP limitations and the cost of equity. Finally, I provide evidence GAAP limitations effectively decrease the cost of equity when managers have incentives to bias financial reports. These results have implications for standard setters who are interested in how accounting standards can help provide useful information to investors.

CHAPTER 2. HYPOTHESES DEVELOPMENT

2.1 GAAP LIMITATIONS AND COMPARABILITY

I review the academic literature and statements by the FASB to distill two competing theories regarding the relation between comparability and limiting managers' discretion. The crux of these theories hinges on why managers select different accounting treatments and whether or not these differences in accounting treatments make "like things…look alike and different things…look different" (Concept Statement 8, p. 19). I label the first the "standardization" theory, which argues that standard setters improve comparability because they minimize diverse practices and play a coordinating role. This theory also assumes that standard-setters' proscriptions are not biased because they have no incentive to manipulate financial information. I label the second the "information" theory, which argues that managers are better suited to achieve comparability. This theory is based on the premise that managers' have more intimate knowledge regarding their firm's transactions and the assumption that the benefits of managers conveying this information outweighs the costs of any potential managerial opportunism. Both theories assume certain managers are accounting for certain classes of transactions differently but ultimately the theoretical cause of these differences plays a central role in determining the predicted relationship between limiting managers' discretion and comparability. In the following sections, I first provide a discussion of why two managers with similar transactions might select different accounting treatments and then I provide the arguments for each respective theory.

Differences in accounting treatments

Managers select different accounting treatments when 1) financial statement users have different preferences (e.g., equity or debt holder information preferences), 2) managers' incentives are different, or 3) when managers differ in professional judgment. Regarding user preferences,

different financial statement users have distinct incentives and abilities, which may cause them to demand different accounting treatments for transactions that are similar. For example, while many comment letters favored the elimination of multiple accounting treatments for mergers and acquisitions, the American Bankers Association wrote a comment letter opposing the elimination of alternative accounting treatments (i.e., the pooling method) for mergers and acquisitions. The requirements to use the pooling method focus largely on issues most relevant to banks (i.e., how the deal is financed) and do not focus on differences in the economics of the merger or acquisition. Ultimately, comparability is the attribute of information that allows the *user* to "to identify and understand similarities in, and differences among, items", (Concept Statement 8, p. 19) and therefore user preference driven differences in accounting treatments may increase comparability for those users.

If user preferences do not drive the use of different accounting treatments, these differences may be driven by differences in manager's incentives. Theory and empirical evidence suggest that managers have incentives to misrepresent financial information to meet a variety of thresholds that benefit the manager, various stakeholders, or both (e.g., Graham, Harvey, and Rajgopal 2005; Leuz, Nanda, and Wysocki 2003; Degeorge, Patel, and Zeckhauser 1999; Burgstahler and Dichev 1997; Jones 1991). Research also suggests that directional goals (e.g., meeting a threshold) increase the likelihood that individuals will reach conclusions that are consistent with their goal (e.g., Kunda 1990; Kadous, Kennedy, and Peecher 2003). If the attributes of the underlying transactions are similar, but different accounting treatments are used only because managers differ in their incentives, then comparability is likely diminished.

Last, managers may differ in how they exercise professional judgment due to differences in 1) the underlying transaction, 2) managerial expertise or 3) unintentional biases. With respect

to the first point, Concept Statement 8 suggests that comparability is enhanced when "different things…look different" (p. 19) and thus different transactions justify the use of different treatments. Regarding managerial expertise, two managers with different areas and/or levels of expertise may focus more heavily on certain transaction attributes and make different financial reporting decisions (e.g., Badolato, Donelson, and Ege 2014; Ashraf, Michas, and Russomano 2019). While some might initially argue that comparability will be diminished unless differences in the underlying transactions justify these differences in professional judgment, this likely is only the case if transactions are so simple that there is a "correct" accounting treatment to represent the given transaction. For example, the literature examining divergent and convergent thinking suggests that divergent thinking can serve to produce novel and potentially more efficient solutions, and convergent thinking can serve to evaluate divergent thoughts and select the most efficient solution (Cropley, 2006). Thus, a healthy degree of divergent accounting treatments in the current period may lead to more efficient and comparable financial statements in the long term.

With respect to unintentional biases, two managers could exhibit distinct decision-making biases that could lead them to select distinct financial statement outcomes. These biases include anchoring bias (Tversky and Kahneman 1974), confirmation bias (Snyder and Swann 1978), social comparison bias (Festinger 1954), and biases arising from the use of heuristics (Ashton and Ashton 1995), to name only a few. For example, social comparison theory suggests that individuals receive intrinsic rewards by making decisions that are similar to their comparison group (e.g., Festinger 1954; Pyszczynkski, Greenberg, and LaPrelle 1985). Thus, if managers use different comparison groups to evaluate their own performance, then they may be induced to use accounting treatments that are similar to their comparison group but dissimilar from one another. Research also suggests that expertise and unintentional biases are interdependent. For example, Maines and

Wahlen (2006) discuss how heuristic-driven biases can be partly overcome by expertise (Smith and Kida 1991). If these biases are truly unintended, then it is difficult to predict that they yield anything but reduced comparability.

Standardization theory

Standardization theory is based on the premise that managers, at times, account for similar transactions using dissimilar accounting treatments. As discussed above, these differences can be due to variety of factors. Whether and how limiting managers' discretion improves comparability depends on the underlying cause of these differences. Standardization theory generally focuses on reducing diverse practices that relate to differences caused by professional judgment or incentives. With respect to differences caused by professional judgment, limiting discretion may "reduce the effects of differences in professional judgment" (Schipper 2003, p. 67) but whether this is desirable depends on whether these differences contain information (i.e., the attributes of the transaction merit different treatments) or merely reflect noise in managers' decision making processes (i.e., the attributes of the transaction do not merit different treatments).[2]

Regarding noise in managers' decision making process, two managers could arrive at different conclusions based on the same information when 1) the problem is complex enough that more than one conclusion is plausible (i.e., there must be alternatives in the managers' choice set) and 2) the managers must *differ* in expertise, incentives, or some form of unintentional bias.[3] If differences in professional judgment are primarily due to noise and do not contain information that allows investors "to identify and understand similarities in, and differences among, items", (Concept Statement 8, p. 19) then limiting these differences via standards could improve

[2] Information disclosed by managers is often modeled as containing both an information component and a noise component (e.g., Verrecchia 1983).

[3] Multiple criteria decision-making research suggests that complex decisions may be path dependent (Korhonen, Moskowitz and Wallenius 1990). Unless managers use identical decision-making processes, path dependence may also cause managers' to reach divergent conclusions.

comparability. Some have argued that standard setters are well positioned to implement standards that accomplish this because "standard setters amass a great deal of knowledge about the mapping between economic constructs and accounting constructs" and can "enhance the [usefulness] of accounting information by incorporating this information in new standards" (Maines and Wahlen, p. 410). Ultimately, however, whether restrictive accounting standards limit differences in professional judgment that arise from information, noise, or both, is an empirical question.

Further, if managers' *differ* in their incentives to bias financial reports, then limiting managers discretion may prevent similar transactions from intentionally being presented dissimilarly in financial statements. This logic is reflected in the FASB's most frequently cited reason for issuing standards, which is to "reduce diverse practices" (Jiang et al. 2018). If the standard setter is not beholden to similar incentives to bias financial reports (e.g., their wealth does not increase when individual firms meet or beat certain thresholds), then they may implement accounting standards that more faithfully represent the similarities and differences of each transaction, and thereby enhance comparability.[4]

An important feature of this theory is that the standard setter can mandate perfect coordination across firms, and as long as the scope of the standard is set appropriately, this coordination will lead to similar transactions being accounted for similarly. Holding all else equal, this coordinating role of the standard setter provides a strong argument in favor of standardization because managers are unlikely to coordinate perfectly. For example, one firm could elect to account for inventory on a First-In, First-Out (FIFO) basis, while another firm with similar inventory flows could use a Last-In, First-Out (LIFO) approach. While there are tax incentives to

[4] However, it is an empirical question whether standards setters implement such standards. Watts and Zimmerman (1978) theorize that managers' will attempt to influence the standard setting process for personal benefit. If such attempts are successful it is unclear how the resulting standards will impact comparability.

choose one approach or the other, this use of discretion likely causes some similar inventory flows to appear dissimilar. Coordinating accounting treatments such that similar accounting methods are always used for similar transactions should improve comparability.[5]

Last, Gonedes (1980) discusses the relationship between public disclosure rules and disclosure of private information and asserts that "the effectiveness of the rules cannot, in general, be assessed independently of private information-production activities" (p. 442). Recent evidence suggests that accounting standards that limit managers' discretion are associated with an increase in supplementary voluntary disclosure (Hribar et al. 2020). Limiting managers' discretion could lead to improved comparability to the degree the information provided in GAAP-restricted disclosures and voluntary disclosures better allows financial statement users "to identify and understand similarities in, and differences among, [transactions]" (Concept Statement 8, p. 19), as compared to the information that unrestricted disclosures would provide.[6] Overall, these arguments lead to the following directional hypothesis.

Hypothesis 1a: Limiting managers' discretion leads to enhanced comparability.

Information theory

Information theory is based on the notion that managers possess superior information regarding each transaction (Holthausen and Leftwich 1983: Holthausen 1990). Given that managers almost certainly possess this information advantage, differences in accounting choices could reflect information that can enable investors "to identify and understand similarities in, and

[5] In the LIFO versus FIFO example the benefits of improved comparability may not outweigh the costs.

[6] For example, consider the following two scenarios. First, assume management is restricted by GAAP and issues a GAAP earnings number (X) and a management preferred Non-GAAP earnings number (Y). Assume also that X contains some information that management prefers not to disclose but that enhances comparability. Second, assume management is entirely unrestricted by GAAP and issues her preferred number (Y) within mandatory disclosures and may withhold (X) due to proprietary costs or opportunistic incentives. In the first scenario investors possess both Y and X and in the second case they possess only Y. See Hanlon, LaPlante, and Shevlin (2005) for an example of information loss caused by the issuance of only one of two informative signals.

differences among, [transactions]" (Concept Statement 8, p. 19). If accounting standards prevent managers from conveying this information, standards could decrease comparability by requiring dissimilar transactions to be treated similarly (e.g., SFAS 2) or requiring highly similar but superficially distinct transactions to be treated dissimilarly (e.g., APB 16).[7,8]

Because differences in accounting choice could be due to managers' incentives, which may or may not align with stakeholder incentives, proponents of the information theory argue that the prevalence and/or magnitude of managerial opportunism are such that the information benefits outweigh the costs of opportunism. Consequently, the prevalence and magnitude of managerial incentives to deviate from faithful representation will, in part, determine whether unrestrained managerial discretion would improve comparability, notwithstanding managers' information advantage. Regarding the prevalence of managerial opportunism, economic models suggest intentional managerial deception is not equilibrium behavior, but rather an exploitation of a reputation built in prior periods of truthful disclosure (Sobel 1985). Christie and Zimmerman (1994) provide evidence consistent with this notion by examining the pervasiveness of opportunistic accounting choices in a sample of takeover targets. They find "while the frequency of opportunism is relatively small, the dollar effect on retained earnings…is large" (Christie and Zimmerman, p. 540). Whether the effect of infrequent yet large opportunistic accounting choices on comparability outweighs the benefits of allowing managers to convey private information is an empirical question.

Additionally, the degree to which managers can coordinate similar accounting treatments

[7] Comment letters written to the FASB reflect this possibility. For example, a constituent was concerned that "the proposed standard (FAS 98) would stipulate a difference in accounting treatment" for equivalent transactions and stated, "for this reason, it appears that the proposed standard creates an undesirable lack of comparability between two sets of financial statements containing similar economic conditions."

[8] I take no stance on the optimality of these standards. Given the costs and benefits of each, it is plausible these standards are optimal. However, SFAS 141 superseded APB 16, demonstrating the FASB's revealed preference. SFAS 2 is now codified.

for similar transactions is relevant to whether limiting managers' discretion improves comparability. Concept Statement 8 suggests "some degree of comparability is likely to be attained by satisfying the fundamental qualitative characteristics [(i.e., relevance and faithful representation)]" (p. 20). However, "a single economic phenomenon can be faithfully represented in multiple ways [and] permitting alternative accounting methods for the same economic phenomenon diminishes comparability" (Concept Statement 8, p. 20).[9] Without considering incentives to intentionally bias financial reports, managers could select different treatments for similar transactions because of financial statement user preferences or differences in professional judgment. Again, these differences in professional judgement could be informative or could merely contain noise. If managers at different reporting entities choose distinct accounting treatments that faithfully represent the same economic transaction, then comparability is diminished.[10] Thus, the enhancements to comparability gained from managers exercising discretion depend upon the degree of selection of similar treatments that naturally would occur without accounting standards. Decision making literature has discussed the costs and benefits of convergent and divergent decision making. Divergent decision-making serves to provide novel alternatives while convergent decision making serves to evaluate the value of novel alternatives and create an efficient "orthodoxy" (Cropley 2006). Thus, a degree of divergent accounting treatments (i.e., less than perfect coordination) may be desirable because it can provide novel reporting avenues to allow managers' to convey the similarities and differences of transactions. Once novel accounting treatments have arisen, theory and evidence in Brown (2011) and McMullin (2016) suggest that firms mimic the accounting methods and disclosures of other firms

[9] In the strictest sense, there can never be two complete, neutral, and errorless treatments for the same transaction. However, if managers have imperfect information regarding the nature of the transaction, two managers with heterogeneous beliefs may choose distinct neutral and errorless disclosures that are complete with regards to the information available.

[10] Because user preferences may drive the decision to use different accounting treatments for similar transactions, the benefits of requiring comparable accounting treatments across firms may not outweigh the cost of neglecting users' preferences.

(i.e., they converge), and thus, a substantial degree of naturally occurring selection of similar accounting methods is plausible. Indeed, experimental evidence suggests that managers "exhibit more agreement and are less likely to report aggressively under a less precise (more principles-based) standard" (Agoglia, Doupnik, and Tsakumis 2011, p. 747). Thus, even if perfect coordination were optimal, the coordinating advantage of the standard setter may be slight compared to managers' information advantage.

Last, Nelson, Elliott, and Tarpley (2002) provide evidence that managers are more likely to manage earnings via transaction structuring and auditors are less likely to adjust these attempts to manage earnings when standards are precise. However, they also find that managers are more likely to use unstructured earnings management and auditors are less likely to adjust these attempts when standards are imprecise. If structured earnings management under restrictive standards provide more justification for treating similar transactions dissimilarly, then limiting managers' discretion could lead to reduced comparability. This may be the case if unstructured earnings management attempts are at the margin (e.g., a slight change in estimate) and structured earnings management attempts are wholesale changes in the accounting treatment (e.g., capitalizing versus expensing a lease). These arguments lead to the following directional hypothesis.

Hypothesis 1b: Limiting managers' discretion leads to reduced comparability

Hypothesis 1a and transaction similarity

Hypothesis 1a predicts that limiting managers' discretion will improve comparability. However, inherent in the FASB's frequently stated intention of reducing diverse practices (Jiang et al. 2018) is the assertion that managers are using dissimilar accounting treatments for similar transactions. This suggests that the limitations of managers' discretion imposed by GAAP are focused on making transactions appear similar and not on making transactions appear dissimilar.

To the degree that transactions are indeed similar, comparability should be improved as similar transactions appear similarly in financial statements. However, to the degree transactions are dissimilar, "the result will be *surface comparability*, and dissimilar arrangements will be forced into the same accounting treatment" (Schipper 2003, p. 67). These arguments lead to the following directional hypotheses.

> *Hypothesis 2a: Limiting managers' discretion when transactions are similar leads to enhanced comparability*
>
> *Hypothesis 2b: Limiting managers' discretion when transactions are dissimilar leads to reduced comparability.*

2.2 COMPARABILITY AND USEFULNESS

The impact of limiting managers' discretion on comparability could ultimately impact the overall usefulness of financial information to investors. This could occur through two related channels. First, as investors select investment opportunities from a set of alternatives, comparability aids the decision-making process by decreasing investors' information processing costs (Barth et al. 1999; Li 2010). Second, comparability may also enhance information spillovers between firms (Dye 1990). Consistent with these arguments, prior literature provides evidence that analyst accuracy and acquisition profitability are greater when financial reports are more comparable (De Franco et al. 2011; Chen et al. 2018). Further, Imhof, Seavey, and Smith (2017) provide evidence that comparability lowers the cost of equity. As long as limiting managers' discretion has an impact on comparability, I predict limiting managers' discretion will impact usefulness via comparability. Consistent with this prediction, I state my third hypothesis in the alternative form below.

> *Hypothesis 3: Limiting managers' discretion will impact usefulness via comparability.*

CHAPTER 3. RESEARCH DESIGN

3.1 SAMPLE

I obtain data from Compustat, CRSP, and the Institutional Brokers' Estimate System (IBES) to construct my financial statement, returns, and cost of equity variables. Additionally, I gather the data from the text of U.S. GAAP standards to measure the extent to which each standard limits managers' discretion and from the text of 10-K documents to determine which standards each firm relies on following Folsom et al. (2017).[11] The intersection of these databases results in a final sample of 16,204 firm-years between the years 1993-2016. This sample is limited in cross-sectional tests that require additional data. Table 1 presents the sample selection process. I winsorize all continuous variables at the 1st and 99th percentiles.

3.2 MEASUREMENT

3.2.1 GAAP LIMITATIONS

I follow Hribar et al. (2020) and measure GAAP limitations within a given standard as the number of obligations a standard places upon financial statement preparers orthogonalized to the complexity of the underlying transaction. I gather the number of obligations from accounting standards by counting the occurrences of obligatory language. To identify obligatory language, I follow Hribar et al. (2020) and use the modal verbs *shall, should,* and *must*.[12] Hribar et al. (2020) use these verbs because they are identified in the legal literature as words that impose "a high degree of obligation on the addressee" (Trosborg 1995, p. 34). Further, the obligatory use of these words has been discussed in hundreds of court cases and accounting standards specifically (Federal Plain Language Guidelines; Accounting Standards Update 2011-04).

[11] My sample of 10-K documents includes unamended 10-K documents available on the SEC EDGAR website.
[12] Obligatory language in accounting standards can (1) require recognition or disclosure of certain information, (2) require the use of a certain accounting method, and (3) define the requirements of each accounting method. A manual reading of GAAP limitations suggests that a majority of these obligations fall into the third category.

Hribar et al. (2020) validate the limiting nature of these words by (1) benchmarking their limiting nature against other potentially limiting modal verbs, (2) examining whether standards that are commonly considered to be more limiting contain more limiting words, and (3) examining whether comment letter writers discuss limited discretion more often when standards are limiting.[13] To benchmark the limiting nature of *shall*, *should*, and *must*, two independent readers with accounting expertise examined the use of these verbs within accounting standards and reported if the use of the word, in context, limited the discretion of the financial statement preparer. These words were identified as limiting in over 98 percent of the sentences examined. The next most restrictive word was "lowest" and was found to be limiting in 62 percent of the sentences examined. No other words were rated above 50 percent limiting. Last, they find that standards that are anecdotally considered restrictive contain more limiting words and that comment letter writers discuss limited discretion more often when a standard is limiting.

Variation in the complexity of the underlying transaction is important to account for when measuring GAAP limitations. For example, standards with many limitations could be related to complex transactions, and thus limitations may be correlated with transaction complexity. Like Hribar et al. (2020), I follow Donelson et al. (2012) and orthogonalize my standard-level measure of limitations (i.e., the word count of *shall, should,* and *must*) to the complexity of the underlying transaction, measured in two ways. First, I measure transaction complexity as the length of the sentence in the FASB glossary describing the transaction, and second, as the number of times the standard describes the transaction as complex, complicated, or elaborate in the standard. Like Donelson et al. (2012), I scale the orthogonalized number of limitations to lie between zero and one. Explicitly the measure of limitations ($Restrict_{it}$) is calculated as shown in Equation 1.

[13] Modal verbs are a class of verbs that imply obligation, restriction, or necessity. Hribar et al. (2020) obtain the benchmark list of modal verbs from Bill McDonald's website. This list originates from words in 10-K documents (Loughran and McDonald 2011).

$$Restrict_{it} = \sum_s [Rel\_Imp_{its} * Limitations_{ts}] \quad \text{(Eq. 1)}$$

$$Rel\_imp_{its} = \frac{(firm\_count_{its} - avg\_firm\_count_{ts})}{std\_dev(firm\_count_{ts})} + minimum_{st} \left[\frac{(firm\_count_{its} - avg\_firm\_count_{ts})}{std\_dev(firm\_count_{ts})}\right] \quad \text{(Eq. 2)}$$

Where Rel_Imp_{its} is a validated measure of the degree to which firm *i* relies on standard *s* in year *t* and is calculated following Equation 2.[14] I follow Folsom et al. (2017) and create Rel_Imp_{its} in the following steps. First, I count the number of words in each 10-K that relate to an individual standard ($firm\_count_{its}$), using the keyword list provided by Folsom et al. (2017). To standardize this measure, I subtract the standard-year average word count and scale by the standard-year standard deviation. I then add the standard-year minimum to ensure standards that do not impact the firm receive a zero weight. $Limitations_{ts}$ is the orthogonalized and scaled count of *shall, should,* and *must* in standard *s* in year *t.* To create $Limitations_{ts}$, I sum these word counts across the original standard, amending standards, and interpretive guidance documents. I treat Accounting Standards Updates as amendments to pre-codification standards, using the FASB's reference tool to determine which updates amend which standards. The product of standard-year limitations ($Limitations_{ts}$) and the degree to which a firm relies on a standard (Rel_Imp_{its}) is a measure of the extent to which standard *s* limits managers' discretion in year *t*. The sum of these products ($Restrict_{it}$) is a measure of the extent to which GAAP limit managers' discretion in a given year. For further details on the construction of $Restrict_{it}$, see Hribar et al. (2020).

3.2.2 COMPARABILITY

To measure comparability, I follow De Franco et al. (2011) and consider two firms to have comparable accounting systems if similar economic events result in similar financial statements.

[14] Folsom et al. (2017) examined the construct validity of Rel_Imp_{its} in three ways. First, for each standard, an expert from a Big Four audit firm evaluated the accuracy and completeness of the word lists used to create Rel_Imp_{its}. Second, Folsom et al. (2017) find strong positive correlations between keyword counts and the size of the corresponding line item in the financial statements, where possible. Third, Folsom et al. (2017) provide evidence that keyword counts are low in industries where certain standards are unlikely to apply to a firm and are high in industries where certain standards are likely to apply to a firm.

Similar to De Franco et al. (2011), I operationalize this definition of comparability by using returns and earnings as proxies for economic events and financial statements, respectively. Specifically, I estimate the relation between earnings and returns for firm *i* by estimating the following time-series regression model over the previous 16 quarters, requiring a minimum of 14 quarters with available data.

$$Earnings_{it} = \alpha_i + \beta_i * Return_{it} + \varepsilon_{it} \quad \text{(Eq. 3)}$$

Earnings is net income before extraordinary items scaled by market value of equity for firm *i* in quarter *t*. *Return* is the buy-and-hold stock return for firm *i* in quarter *t*. The coefficient estimates (i.e., α_i and β_i) represent the firm-specific accounting system that maps economic events to financial statement outcomes. The accounting system for firm *j* is similar to the accounting system for firm *i* if similar economic events map to similar financial statement outcomes. Following this logic, I use two accounting functions (i.e., α_i, β_i and α_j, β_j) to estimate earnings for a single economic event (i.e., $Return_{it}$) as follows.

$$E_i(Earnings_{it}) = \hat{\alpha}_i + \hat{\beta}_i * Return_{it} \quad \text{(Eq. 4)}$$

$$E_i(Earnings_{jt}) = \hat{\alpha}_j + \hat{\beta}_j * Return_{it} \quad \text{(Eq. 5)}$$

Subscripts on the expectations operator represent which firms returns were used to estimate earnings, and subscripts on *Earnings* indicate which firm's estimated coefficients were used. Using these estimates of earnings, I measure pair-wise comparability as follows.

$$Comp_{ijt} = -\frac{1}{T}\sum_t^T \left| E_i(Earnings_{it}) - E_i(Earnings_{jt}) \right| \quad \text{(Eq. 6)}$$

T is the number of quarters with available data. Greater values of *CompAcct* represent more comparable financial statements. I follow De Franco et al. (2011) and create a firm-year measure

of *Comparability* by taking the average of *CompAcct* for firm *i* in period t within the same 2-digit SIC industry.[15]

3.2.3 IMPLIED COST OF EQUITY CAPITAL AS A PROXY FOR USEFULNESS

I proxy usefulness using the cost of equity because the Concept Statement 8, supported by theory and empirical evidence, suggests that more useful information reduces the cost of capital.[16,17] I estimate the implied cost of equity capital using stock prices and IBES analyst forecast data that are available as of June in the subsequent year. Like Dhaliwal et al. (2016) I follow the models presented in Gebhardt, Lee, and Swaminathan (2001), Claus and Thomas (2001), Easton (2004), and Ohlson and Juettner-Nauroth (2005). Hereafter, these models are respectively referred to as GLS, CT, MPEG, and OJN. I describe the estimation of these variables in Appendix A.

There has been significant debate surrounding whether any of the available cost of equity measures capture the cross-sectional variation in expected returns. Several studies perform validation tests of cost of equity estimates by either (1) examining their covariance with theorized determinants of the cost of equity or (2) examining their relation with realized returns. Guay, Kothari, and Shu (2011) argue ex post returns are the appropriate benchmark to validate cost of equity estimates. Further, they "note that several studies in the accounting literature perform

[15] Inferences remain similar in additional analyses that use two alternate measures of comparability that account for the asymmetric timeliness of earnings and the lead-lag relation between earnings and returns (see Chen et al. 2018 for details).

[16] For reviews of this topic area, see De George, Li, and Shivakumar (2016) and Leuz and Wysocki (2016). See also Lambert, Leuz, and Verrecchia (2007); Lambert, Leuz, and Verrecchia (2011); Barry and Brown (1985); Coles and Loewenstein (1988); Diamond and Verrecchia (1991); Amihud and Mendelson (1986); Bhattacharya, Daouk, and Welker (2003); Francis, Lafond, Olsson, and Schipper (2004); Ashbaugh-Skaife, Collins, Kinney, and LaFond (2009); Kim and Qi (2010); Li (2010); Armstrong, Core, Taylor, and Verrecchia (2011); Bhattacharya, Ecker, Olsson, and Schipper (2012); Barth, Konchitchki, and Landsman (2013).

[17] Early literature in this area debated whether imperfect information presents undiversifiable risks to investors (see Barry and Brown 1985). Lambert et al. (2007) present a model in which accounting information has an undiversifiable impact on the cost of equity via systematic risk. A separate stream of literature suggests certain market characteristics allow idiosyncratic risk to impact market prices (Lintner 1965; Taylor and Verrecchia 2015; Malkiel and Xu 2002; Spiegel and Wang 2005; Fu 2009).

validation tests of cost of capital measures by correlating them with risk proxies, such as beta, size, and growth. However, these tests are not necessarily conclusive" (p. 129).

Gode and Mohanram (2003) provide evidence the OJN and GLS measures vary as predicted with risk factors and ex-post returns. On the other hand, Botosan and Plumlee (2005) provide evidence the OJN and GLS measures are not associated with risk factors as predicted by theory. Easton and Monahan (2005) find that seven of the most commonly used measures (including those used in this paper) of the implied cost of capital do not exhibit positive associations with realized returns. Botosan, Plumlee, and Wen (2011) provide evidence this result is due to misspecification. After correcting for this misspecification, they find the OJN, MPEG, GLS, and CT measures, as well as the average of these measures, are significantly positively associated with ex post returns. In summary, the measures used in this study have been validated in multiple studies though there is disagreement on which individual measure is the best estimate. Due to the continuing debate about which model best captures the cost of equity, I follow prior literature (Hail and Leuz 2006; Li 2010; Dhaliwal et al. 2016) and use the mean from these four models to diminish the effect of measurement error in any one measure. Using the average of these estimates will reduce measurement error to the degree the measurement error in each measure is orthogonal to the measurement error in the other measures.[18]

Panel A of Table 2 presents the descriptive statistics for the implied cost of equity estimates, and Panel B of Table 2 presents the correlation coefficients among the individual cost of equity estimates. The MPEG model generates the highest average cost of equity estimate with a mean estimate of 11.4%. The GLS model generates the lowest average cost of equity estimate

[18] In robustness analyses, I confirm that inferences are similar using the individual estimates of the cost of equity. I further alleviate concerns about measurement error by documenting that bank loan spreads, which are measured without error, are higher when limitations are higher. Banks have different incentives and abilities than equity investors and thus may be less sensitive to information quality concerns. Notwithstanding these differences, I find loan spreads are positively associated with limitations.

with a mean of 6.7%. Panel B of Table 2 demonstrates the OJN and MPEG models are the most highly correlated with a Pearson (Spearman) correlation coefficient of 0.81 (0.87). Further, the GLS and MPEG models exhibit the lowest Pearson (Spearman) correlation with a coefficient of 0.28 (0.27). The average cost of equity estimate is 9.4 percent. This measure ranges between roughly 6 and 14 percent. These estimates are comparable to those in prior studies (Dhaliwal et al. 2016; Botosan and Plumlee 2005; Chen, Chen, and Wei 2011).

3.3 EMPIRICAL SPECIFICATION

I use the following panel regression models to examine how GAAP limitations impact comparability and the cost of equity.

$$Comparability_{it} = \alpha_0 + \alpha_1 Restrict_{it} + \Sigma_{j=2}^{n} \alpha_j X + \lambda_k + \Delta_t + \epsilon_{it} \quad \text{(Model 1)}$$

$$CostofEquity_{it} = \beta_0 + \beta_1 Restrict_{it} + \Sigma_{j=2}^{n} \beta_j X + \lambda_k + \Delta_t + \varepsilon_{it} \quad \text{(Model 2)}$$

$$CostofEquity_{it} = \theta_0 + \theta_1 Restrict_{it} + \theta_2 Comparability_{it} + \Sigma_{j=3}^{n} \theta_j X + \lambda_k + \Delta_t + \mu_{it} \quad \text{(Model 3)}$$

CostofEquity$_{it}$, *Restrict*$_{it}$, and *Comparability*$_{it}$ are measures of the implied cost of equity, GAAP limitations, and comparability, respectively. Industry-fixed effects (λ_k) control for time-invariant industry characteristics and year-fixed effects (Δ_t) control for time-varying (i.e., annual) characteristics. X represents a vector of controls which includes factors impacting the earnings-return relation that the De Franco et al. (2011) measure relies upon and measures commonly used in implied cost of equity models (Chen et al. 2011; Campbell, Dhaliwal, Schwartz 2012; Dhaliwal et al. 2016).[19] For purposes of my mediation tests, the controls across these models must be held constant. Industry and firm characteristics likely determine the mapping of transactions to treatments and thus to a large degree industry and firm characteristics drive comparability.

[19] In theory, only the quality of the mapping between transactions and treatments should determine comparability. However, the De Franco et al. (2011) measure of comparability likely is determined in part by factors impacting the earnings-return relation.

Incremental adjusted-R^2 statistics from my analyses suggest firm, industry, and year fixed effects account for a substantial portion of the variation in comparability.

I include the following controls: *Long-TermGrowth, ReturnOnAssets, MarketToBook, Leverage, Momentum, and MktValueofEquity.* All variable definitions are provided in Appendix A. *Long-TermGrowth* proxies for expected earnings growth. Prior research suggests earnings derived from growth opportunities are riskier than other earnings and thus I predict *Long-TermGrowth* will vary negatively with *Comparability* and positively with the *CostofEquity* (Beaver, Kettler, and Scholes 1970; La Porta 1996; Botosan and Plumlee 2005). *ReturnOnAssets* proxies for profitability which I predict will lead to more stable earnings-return relations and increases in *Comparability*. *ReturnOnAssets* also could reflect internal hurdle rates, thus prior literature documents both positive and negative relations with the cost of equity. *MktValueofEquity* proxies for firm size. The De Franco et al. (2011) measure assumes market efficiency. For small firms, this assumption is less likely to be satisfied and the earnings-return relations may exhibit greater noise. Consequently, I predict that *Comparability* will be higher for large firms. Prior literature argues unless a model controls for all risk factors, *MktValueofEquity* will exhibit a mechanically negative association with the cost of equity (Berk 1995; Botosan and Plumlee 2005). *MarketToBook* is related to firm growth prospects and, similar to *Long-TermGrowth,* is expected to vary negatively with *Comparability*. Market prices are higher relative to book amounts as the cost of equity decreases, thus *MarketToBook* is expected to be negatively associated with *CostofEquity*. *Leverage* increases default risk and consequently may introduce additional noise into a firm's earnings-return relation. Thus, I predict leverage will be negatively associated with *Comparability*. Due to increased default risk, I predict *Leverage* is positively associated with *CostofEquity* (Modigliani and Miller 1958; Botosan and Plumlee 2005). Research has posited that

stock price momentum may be due to market inefficiency (Jegadeesh and Titman 2002).[20] If this inefficiency introduces additional noise in the earnings-return relation, I predict *Momentum* will be negatively related to *Comparability.* Consistent with prior research, I predict the cost of equity is negatively associated with *Momentum* (Dhaliwal et al. 2016). I define industry groups using the Fama-French 48 industry classifications (Fama and French 1997). I cluster heteroscedasticity-corrected standard errors at the firm level. All continuous independent variables in multivariate analyses are standardized to have a mean of zero and a standard deviation of one to aid in interpretation.

I present descriptive statistics for the sample in Panel A of Table 3. These estimates are comparable with prior research. Panel B of Table 3 presents correlation (Pearson above the diagonal and Spearman below the diagonal) coefficients. The cost of capital is significantly negatively correlated with $Restrict_{it}$, which is consistent with GAAP limitations lowering the cost of capital. The correlation between $Restrict_{it}$ and the market value of equity is 0.27. This result is unsurprising because GAAP limitations are partially determined by the number of standards upon which a firm relies. That is, a firm cannot be limited by a standard that does not apply to the transactions in which a firm is engaged. Larger firms generally rely on more standards (i.e., they engage in a more diverse set of transactions), and thus I expect a positive correlation.

CHAPTER 4. TESTS OF HYPOTHESES

4.1 TESTS OF HYPOTHESIS 1A AND HYPOTHESIS 1B

I begin my analysis of whether GAAP limitations impact comparability by examining the coefficients on $Restrict_{it}$ estimated via Model 1. Table 4 presents the coefficients estimated via Model 1. The coefficient on $Restrict_{it}$ is significantly negative at less than the five percent level

[20] Momentum may also be explained by additional risk factors not captured by the CAPM (Jegadeesh and Titman 2002). To the degree this risk increases the noise in the earnings-return relation my predictions would be similar.

across all columns in Panel A. These results suggest financial statements are less comparable when GAAP limits managers' discretion. Given that firms are subject to many GAAP limitations and that the nature of these limitations varies from one limitation to the next, this result suggests that the average limitation imposed by GAAP inhibits comparability. Importantly, this result is robust to controlling for time-invariant firm characteristics (i.e., firm fixed effects) *and* time-variant industry characteristics (i.e., industry-by-year fixed effects) which to a large degree determine comparability between firms.[21,22]

4.2 TESTS OF HYPOTHESIS 2A AND HYPOTHESIS 2B: TRANSACTION SIMILARITY

Hypotheses 2a and 2b predict that GAAP limitations improve comparability when firms engage in similar transactions and reduce comparability when firms engage in dissimilar transactions. For example, managerial incentives may lead firms to account for similar transactions using multiple treatments. Thus, regulatory intervention in the form of GAAP limitations could improve financial statement comparability. Conversely, firms whose transactions are dissimilar from other firms may not benefit from GAAP limitations because dissimilar transactions may be forced into similar treatments. Consistent with this logic, the effect of GAAP limitations on comparability may be more negative (i.e., comparability inhibiting) when firms engage in dissimilar transactions. To test these hypotheses, I alter Model 1 as follows:

$$Comp_{ijt} = \alpha_0 + \alpha_1 Restrict_{it} + \alpha_2 Dissim_{ijt} + \alpha_3 Restrict_{it} * Dissim_{ijt} + \Sigma_{j=4}^{n} \alpha_j X + \lambda_k + \Delta_t + \epsilon_{it}$$

[21] In untabulated analyses, I document that the adjusted r-squared is roughly 7 percent when no fixed effects are included. Thus, industry-by-year and firm fixed effects jointly account for roughly 70 percent of the variation in comparability.

[22] While these results are robust to controlling for stable firm characteristics, one could argue that year-over-year changes in underlying economics (e.g., an acquisition) could lead to higher limitations and lower comparability. In an untabulated analysis, I examine whether my results are concentrated among firms with large year-over-year changes in their set of transactions. To do so, I create a "non-stickiness" measure that is increasing in the degree the underlying transactions changed from one year to the next. I create this measure in two steps. First, I decile rank each firm's relative importance weight for each standard each year. I then create a measure, Nonstick, by taking the sum of the absolute differences in these ranks from one year to the next across all standards. I validate this measure by demonstrating that it is positively associated with infrequent one-time transactions (i.e., M&A activity and restructuring). While controlling for time-invariant firm characteristics, I find that the relationship between comparability and GAAP limitations does not vary with year-over-year changes in firms' transaction set. This suggests that these results are not driven by variation in the transactions engaged in but rather are driven by variation in GAAP limitations.

Where $Comp_{ijt}$ is calculated via equation six and represents the pair-wise comparability for each firm-pair *i* and *j* in year *t*. $Dissim_{ijt}$ is measured as negative one multiplied by the Hoberg and Phillips (2016) product-space similarity measure and is calculated via textual analysis of firms' 10-K product descriptions (See also Hoberg and Phillips 2010). I use this measure as a proxy for pair-wise transaction dissimilarity. My sample for this test includes all firm pairs where firm *i* is included in my original sample and where a transaction dissimilarity score is available. The results of this analysis are presented in Table 5. The coefficient on $Restrict_{it}$ is negative and significant, which suggests GAAP limitations decrease comparability when firms have average transaction dissimilarity. Consistent with Hypothesis 2b, the coefficient on the interaction term is negative and significant. This suggests GAAP limitations are more likely to inhibit comparability when pair-wise transaction dissimilarity is high. Note also that the total effect of GAAP limitations is insignificant when *Dissimilarity* is set to one standard deviation below the mean. This is directionally consistent with, though not directly supportive of, Hypothesis 2a.

Further, to examine whether GAAP limitations improve comparability when transactions are similar, I examine how comparability is impacted by a standard allowing dissimilar treatments for business combinations (i.e., pooling v. purchase method) and a standard that allows only a single treatment (i.e., purchase method). To do so, I create a measure of accounting treatment similarity for business combinations (*SimilarMethod*) that is calculated as the number of firm pairs in an industry-year that had mergers (i.e., both firms had mergers) and used the same accounting treatments divided by the number of firm pairs in an industry-year that had mergers.[23] I expect comparability will be greater when a larger proportion of firm pairs use similar methods. I add this measure to Model 1 and present the coefficient estimates in Table 6. The coefficient on

[23] The data necessary to identify the occurrence of and accounting method for public mergers and acquisitions was obtained from the Securities Data Company (SDC). I exclude minority interest and remaining interest mergers.

SimilarMethod is positive and significant in both columns.[24] This result suggests that comparability is enhanced when GAAP requires consistent accounting treatments for similar transactions.

Next, to examine whether GAAP limitations improve comparability when transactions are dissimilar, I examine how comparability is impacted by SFAS 86 which requires uniform treatment for software development costs that arguably are not entirely similar. Prior to the passage of SFAS 86, certain firms elected to capitalize software research and development costs. SFAS 86 required all software development costs to be expensed prior to technological feasibility regardless of the likelihood that such costs would result in future cash flows. Thus, this standard required similar accounting treatments for transactions that certainly vary in their likelihood of producing future cash flows. I test whether requiring uniform treatment for dissimilar transactions enhanced firm-pair comparability by performing a difference-in-difference analysis around the passage of SFAS 86. I do so using the following models.

$$Comp_{ijt} = \alpha_0 + \alpha_1 Both_{ijt} + \alpha_2 Either_{ijt} + \alpha_3 Post_{it} + \alpha_4 Post_{it} * Both_{ijt} + \alpha_5 Post_{it} * Either_{ijt} + \lambda_k + \epsilon_{it}$$

Where $Comp_{ijt}$ is calculated via equation six and represents the pair-wise comparability for each firm-pair *i* and *j* in year *t*. $Post_{it}$ is equal to one if a firm's year began after December 15, 1985 (i.e., the effective date of SFAS 86). $Both_{ijt}$ is equal to one if both firms *i* and *j* are in the treatment group (see below) and is equal to zero otherwise. $Either_{ijt}$ is equal to one if either firm *i* or *j*, but not both, is in the treatment group and is equal to zero otherwise. The sample of firms is limited to two years prior to the effective date, and two years after the effective date and 1986 is excluded to avoid the effects of anticipation of the standard becoming effective (i.e., 1984, 1985, 1987, and 1988). I use two different sets of control and treatment firms to test this question. First, I use firm-pairs that did

[24] This result, however, is sensitive to the comparability proxy used. The fact that the De Franco et al. (2011) measure is positively associated with the use of similar accounting methods for similar transactions supports the validity of this measure.

(not) engage in research and development activities as the treatment (control) group. Second, I use firm-pairs that engaged in research and development and were (not) in industries likely to engage in software development (i.e., electronics [SIC 3600-3674] and computers [SIC 3570-3577; 7370-7374]) as the treatment (control) group.

If requiring uniform accounting treatment for dissimilar software development transactions led to lower comparability, I predict that coefficients on *Post*Both* and *Post*Either* will be negative. The results of this analysis are presented in Table 7. The control sample in Column 1 consists of firms that did not engage in research and development and the control sample in Column 2 consists of firms that engaged in research and development but were not in electronics or computers. Consistent with my predictions, I find that the interaction coefficients on *Post*Both* and *Post*Either* in Column 1 and Column 2 are significantly negative. This suggests that firm-pairs where both firms were treated became less comparable in the post period and firm-pairs where only one firm was treated also exhibited a decline in comparability as well. These results highlight that GAAP limitations can reduce comparability if those limitations are structured such that dissimilar transactions are accounted for similarly.

4.3 TESTS OF HYPOTHESIS 3

I begin my analysis of whether GAAP limitations impact usefulness (the cost of equity serving as proxy) via comparability by examining the coefficients on $Restrict_{it}$ estimated via Model 2. Table 8 presents the coefficients estimated via Model 2. I present estimates from Model 2 in Column 1 as a benchmark, without controlling for variables that capture information quality (i.e., *AnalystDisp*, *SystematicRisk,* and *IdiosyncraticRisk*).[25] The coefficient on $Restrict_{it}$ is significantly positive at the five percent level across all columns in Table 8, which suggests a greater number

[25] Research suggests market characteristics and investor idiosyncrasies can impede diversification and thereby allow idiosyncratic risk to impact the cost of equity (e.g., Malkiel and Xu 2002; Spiegel and Wang 2005; Fu 2009; Taylor and Verrecchia 2015).

of GAAP limitations increases the cost of equity.[26] These results are robust to the inclusion of firm *and* industry-by-year fixed effects. The magnitudes of the coefficients on $Restrict_{it}$ indicate that a one standard deviation increase in $Restrict_{it}$ is associated with a cost of equity that is 6 to 12 basis points higher. The signs of control variables are consistent with the predicted and observed signs in prior studies (e.g., Dhaliwal et al. 2016; Botosan and Plumlee 2005; Campbell et al. 2012). In untabulated analyses I find that the positive association between GAAP limitations and the cost of equity is not present and in some cases is negative when managers have incentives to manipulate financial reports (i.e., near the zero earnings threshold, during periods of extreme growth, and when discretionary accruals are large). This suggests that GAAP limitations are more valuable to investors when managers are incentivized to manipulate.

4.3.1 MEDIATION VIA COMPARABILITY

My third hypothesis predicts GAAP limitations impact the cost of equity via comparability. In the following section, I perform a mediation analysis using coefficients from models 1, 2, and 3 to examine whether $Restrict_{it}$ impacts the cost of equity via comparability. The coefficients from Model 1 verify that a relation exists between comparability and $Restrict_{it}$. Model 2 verifies that a relation exists between the cost of equity and $Restrict_{it}$ and provides the total effect of $Restrict_{it}$ on the cost of equity. Model 3 provides an estimate of the effect of $Restrict_{it}$ on the cost of equity when comparability is controlled for and thus provides the direct effect of $Restrict_{it}$ on the cost of equity. Following Baron and Kenny (1986), I estimate the indirect effect by multiplying α_1 from Model 1 with θ_2 from Model 3. Mackinnon, Warsi, and Dwyer 1995 show that this approach is

[26] This analysis gives equal weight to each firm and hence does not speak to whether GAAP limitations increase the cost of equity "for the economy as a whole" (Concept Statement 8, p. 22) because GAAP limitations may impact large and small firms differently. To address this question, I perform a weighted least squares analysis and weight firms by their market value of equity. In this untabulated analysis, I find a positive association between the cost of equity and GAAP limitations.

equivalent to taking the difference between β_1 from Model 2 and θ_1 from Model 3. Table 9 presents the coefficient estimates from Model 3 and the indirect effects necessary to test for mediation.

Panel A of Table 9 confirms GAAP limitations are positively associated with the cost of equity. Panel B presents evidence that comparability is negatively associated with GAAP limitations.[27] In Panel C I provide evidence that significant mediation occurs (i.e., the indirect effect is significant at the one percent level).[28] The percent of the relation between GAAP limitations and the cost of equity that is mediated by comparability is substantial (i.e., 21.42 percent). These results provide support for my final hypothesis and confirm that the relation between $Restrict_{it}$ and the cost of equity is mediated by financial statement comparability. However, I find comparability does not fully mediate this relation; this is expected because theory predicts the relation between $Restrict_{it}$ and the cost of equity is also mediated by the usefulness of financial reports generally and by non-information factors (i.e., cash flow effects).

4.4 ADDITIONAL ANALYSES

My measure of GAAP limitations is created from text-based attributes of standards and 10-K documents. Specifically, variation in $Restrict_{it}$ comes from (1) the number of restrictive words in each standard, (2) the number of standards relied upon, and (3) the firm's degree of reliance on each standard. This creates the empirical concern that $Restrict_{it}$ may be correlated with standard- and firm-level attributes other than GAAP limitations. For example, transaction complexity is plausibly associated with longer standards which are respectively related to higher restrictive word counts within standards. In additional analyses, I take several additional steps to address these concerns. First, I control for the number of standards (i.e., the number of non-zero relative

[27] Comparability is used as both an independent and dependent variables in my mediation analysis and is standardized to have a mean zero and a standard deviation of one in all cases (i.e., in Table 4 and Table 6).

[28] I use Sobel (1982) t-statistics to evaluate the significance of the indirect effect. Mackinnon et al. 1995 suggest Sobel (1982) standard errors are conservative and thus have low power. Bootstrapped standard errors have also been used to estimate the significance of the indirect effect (Bollen & Stine, 1990; Shrout & Bolger, 2002) but are less conservative.

importance weights) each firm relies upon each year. Second, I control for the FOG index (Li 2008). Third, I scale the standard-year measure of limitations by the length of the standard. Fourth, I control for firm and industry-by-year fixed effects. I continue to find that limited discretion is negatively associated with comparability and positively associated with the cost of equity.

4.4.1 TRANSACTION INNOVATIONS

One plausible explanation for the results in this paper is that restrictive standards may lead managers to innovate in how they structure transactions (e.g., Nelson et al. 2002) and this could lead to more divergent earnings return relationships. To test this notion, I examine how the relation between GAAP limitations and comparability varies with the volume of Emerging Issues Task Force (EITF) pronouncements that the FASB issues in year *t-1*, *t*, and *t+1*. I examine EITFs because novel transaction structures are likely to lead to EITF pronouncements. Further, EITF pronouncements in *t* or *t*-1 (*t*+1) are (not yet) effective, thus they proxy for transaction structuring that has (not) been limited by GAAP. If my results are driven by transaction structuring that has yet to be limited by GAAP and not limitations of managers' discretion then I expect that my results will be concentrated in years where EITF pronouncements are high in t+1 but not driven by high volume of EITFs issued in the current or prior year. I expect this because discretion related to transaction structuring related to current and past EITFs has already been limited. In untabulated analyses I find that my results are stronger when current and past EITF volume (i.e., the number of EITFS issued) is high but are no different when future EITF volume is high. This result is inconsistent with transaction structuring explaining my results.

CHAPTER 5. CONCLUSION

In this study, I examine whether financial statements are more comparable when GAAP limits managers' discretion. I provide evidence regarding a common assertion that suggests

restrictive standards will improve comparability (Schipper 2003). Further, I provide evidence on how restrictive standards impact the cost of equity via comparability.

I find that financial statements are less comparable when GAAP is restrictive. Perhaps more importantly, I find that this decline in comparability is concentrated in firm-pairs with dissimilar transactions. While this finding is contrary to the assertion that restrictive standards always enhance comparability, it is fully consistent with the FASB's discussion of comparability in Concept Statement 8 and the FASB's frequently stated intent to reduce diverse accounting practices. Also consistent with the FASB's logic, I find comparability is enhanced (reduced) when GAAP requires a consistent accounting treatment for similar (dissimilar) transactions. Further, I find that the cost of equity is higher when GAAP is restrictive and that the decline in comparability associated with GAAP limitations drives a substantial portion of this relation. These results are robust to a variety of tests controlling for standard and firm characteristics.

My results suggest the average GAAP limitation may not improve comparability or the cost of equity. While this evidence is relevant for standard setters, the FASB evaluates the costs and benefits of accounting standards across a diverse set of constituents, and thus further research is needed to examine (1) the impact of the average GAAP limitation on other resource providers and (2) the impact of potential changes to specific limitations on resource providers.

APPENDIX A:
VARIABLE DEFINITIONS

Independent variable:	
Restrict	$Restrict_{it} = \sum_{s} [Rel\_Imp_{its} * Limitations_{ts}]$
Limitations	The count of "shall," "should," and "must" within a standard, amending standards, and interpretive guidance, orthogonalized to Complexity1 and Complexity2 and scaled to between zero and one.
Rel_Imp	Relative importance weight (Rel_Imp) is equal to the count of words related to a specific standard present in a firm's 10-K less the annual average divided by the standard deviation of this word count for the given standard in a given year. After this calculation, the annual minimum is added to ensure a minimum weight of 0. See Folsom et al. (2017) for the word list used to create this variable.
Complexity1	This measure counts the number of times within the standard the FASB refers to the underlying transaction as complex, complicated or elaborate.
Complexity2	This measure calculates the length of the definition of the underlying transaction provided in the FASB glossary.
Dependent variable:	
CostofEquity	The average of the implied cost of equity estimated using the following models: Gebhardt et al. (2001), Claus and Thomas (2001), Easton (2004), and Ohlson and Juettner-Nauroth (2005). Additional details can be found in Appendix B.
$Comparability_{it}$	Comparability between a firm and all other firms in the same 2-digit SIC industry, as measured by De Franco et al. (2011).
$Comparability_{ijt}$	Pair-wise comparability as calculated by De Franco et al. (2011). See equation six.
Control variables:	
AnalystDisp	The natural log of the standard deviation of analyst forecast estimates of earnings in the following period scaled by the median forecast for the following period's earnings.
IdiosyncraticRisk	IdiosyncraticRisk is the annual standard deviation of the error term from the regression used to calculate SystematicRisk.
Leverage	A firm's total long-term debt divided by assets.
Long-TermGrowth	The median of analyst estimates of firm-level earnings growth.
MarketToBook	The market value of equity divided by the book value of equity.
MktValueofEquity	The natural log of the product of share price and shares outstanding.
Momentum	The stock return over the fiscal year.
ReturnOnAssets	Net income divided by total assets.
SystematicRisk	SystematicRisk is estimated by regressing daily returns on contemporaneous value-weighted market returns with appropriate Scholes and Williams (1977) corrections for non-synchronous trading. Where indicated "equal-weighted", SystematicRisk is estimated using equal-weighted market returns.

APPENDIX B:
COST OF EQUITY MEASUREMENT

In this appendix I outline my estimation procedures used to calculate the cost of equity using the following models: Gebhardt et al. (2001), Claus and Thomas (2001), Easton (2004), and Ohlson and Juettner-Nauroth (2005). I follow the approach outlined in Dhaliwal et al. (2016) when estimating the cost of equity using these models. I provide definitions of variables used in the models below.

P_t= the price of common stock in period t. I use the price of common stock in June after the latest fiscal year-end.
BVE_t= Book value of equity (ceq) reported in Compustat. I use the value from the most recent financial statements available at time t.
$FORECAST\_EPS_{t+i}$= the median IBES earnings per share forecast or derived forecast for year t+i. Forecasts are derived using prior period forecasts and long-term growth forecasts.
DIVPAY= the forecast of the dividend payout ratio. I calculate this variable as IBES variable iadiv divided by $FORECAST\_EPS_{t+1}$. When $FORECAST\_EPS_{t+1}$ is negative, I use a six percent return on assets to calculate earnings. I winsorize DIVPAY to be between zero and one.

B.1. Gebhardt et al. (2001)

$$P_t = BVE_t + \sum_{i=1}^{T-1} \frac{(FUTURE\_ROE_{t+i} - COC_{GLS}) * BVE_{t+i-1}}{(1+COC_{GLS})^i} + \frac{(FUTURE\_ROE_{t+T} - COC_{GLS}) * BVE_{t+T-1}}{COC_{GLS} * (1+COC_{GLS})^{T-1}} \quad \text{(B–1)}$$

For the next three years I proxy for expected earnings ($FUTURE\_ROE_{t+i}$) using analysts forecasts. Beyond the third year I proxy for expected earnings by assuming the return on equity will linearly decline to a long-run equilibrium return on equity from year four to year T, assuming T=12. Long-run return on equity is measured as the industry-specific, median return on equity (ROE) measured over the previous ten years. ROE is calculated as income available for common shareholders dived by book value of equity from the prior period. Industries are classified using the Fama-French 48 industry classifications (Fama and French 1997). Similar to Botosan and Plumlee (2005) I do not drop observations with negative values for return on equity. Following Liu, Nissim, and Thomas (2002), the industry return on equity is set to the risk-free rate if the return on equity is lower than the risk free rate. I assume the clean surplus equation holds and estimate BVE_{t+1} as follows: BVE_{t+1}= BVE_t+EPS_{t+1}–(EPS_{t+1}* DIVPAY). This approach assumes future dividends per share equal future earnings per share multiplied by the dividend payout ratio. I numerically approximate COC_{GLS} using a computer-based approximation algorithm that solves for COC_{GLS} such that equation B–1 is satisfied within a tenth of a cent.

B.2. Claus and Thomas (2001)

$$P_t = BVE_t + \sum_{i=1}^{5} \frac{(FORECAST\_EPS_{t+i} - COC_{CT} * BVE_{t+i-1})}{(1+COC_{CT})^i} + \frac{(FORECAST\_EPS_{t+5} - COC_{CT} * BVE_{t+4}) * (1+G_{lt})}{(COC_{CT} - G_{lt}) * (1+COC_{CT})^5} \quad \text{(B–2)}$$

APPENDIX B (CONT.):
COST OF EQUITY MEASUREMENT

To estimate abnormal earnings for the next five years, I use IBES earnings forecasts. When missing, earnings forecasts are derived using the forecast for the prior year and the forecasted long-term earnings growth rate. When the long-term growth rate is missing, I estimate the long-term growth rate using earnings per share in years two and 3. I estimate the abnormal long-term growth rate (G_{lt}) as the risk-free rate less three percent. I use the 10-year Treasury bond yield as the risk-free rate. I assume the clean surplus equation holds and estimate BVE_{t+1} as follows: $BVE_{t+1}= BVE_t+EPS_{t+1}–(EPS_{t+1}*$ DIVPAY). This approach assumes future dividends per share equal future earnings per share multiplied by the dividend payout ratio. I numerically approximate COC_{CT} using a computer-based approximation algorithm that solves for COC_{CT} such that equation B–2 is satisfied within a tenth of a cent.

B.3. Gode and Mohanram (2003) as derived in Ohlson and Juettner-Nauroth (2005)

$$COC_{OJN} = X + \sqrt{X^2 + \left(\frac{FORECAST\_EPS_{t+1}}{P_t}\right) * (G_2 - G_{lt})} \quad \text{(B–3)}$$

where

$$X = 0.5 * (G_{lt} + \frac{DIV\_PER\_SHARE_{t+1}}{P_t}).$$

G_2 is the mean of the growth rate implied by earnings per share in period one and period two and the long-term growth rate forecasted by analysts. This model requires that earnings per share in period one and two are positive. I estimate the abnormal long-term growth rate (G_{lt}) as the risk-free rate less three percent. I use the 10-year Treasury bond yield as the risk-free rate.

B.4. The PEG ration as modified by Easton (2004)

$$P_t = \frac{FORECAST\_EPS_{t+2} + COC_{MPEG} * DIV\_PER\_SHARE_{t+2} - FORECAST\_EPS_{t+1}}{{COC_{MPEG}}^2} \quad \text{(B–4)}$$

I numerically approximate COC_{MPEG} using a computer-based approximation algorithm that solves for COC_{MPEG} such that equation B–4 is satisfied within a tenth of a cent. $FORECAST\_EPS_{t+1}$ and $FORECAST\_EPS_{t+2}$ must be greater than zero and $FORECAST\_EPS_{t+1}$ must be less than or equal to $FORECAST\_EPS_{t+2}$. This approach assumes the change in abnormal earnings growth equals zero (see Equation (9) on page 80 of Easton 2004).

TABLE 1
SAMPLE SELECTION

Main Sample	
Firm-year observations in Compustat from 1993 - 2016	290,303
Less: firm-years with missing data for all cost of equity measures	(241,218)
Less: firm-years with missing data for comparability measure	(22,904)
Less: firm-years with missing data for limitations measure	(8,811)
Less: firm-years with missing data for control variables	(1,166)
Final sample of firm-year observations	**16,204**
Total number of firms	3,074

Notes: This table presents the sample selection procedure.

TABLE 2
DESCRIPTIVE STATISTICS – COST OF EQUITY

Panel A: Descriptive Statistics - Cost of Equity

Variable	N	Mean	Std Dev	P5	P25	Median	P75	P90	P95	P99
CostofEquity-Mean	16,204	0.094	0.026	0.059	0.066	0.077	0.091	0.107	0.127	0.143
CostofEquity-GLS	16,204	0.067	0.032	0.023	0.030	0.044	0.068	0.086	0.102	0.113
CostofEquity-CT	16,204	0.087	0.028	0.048	0.057	0.071	0.085	0.100	0.119	0.134
CostofEquity-OJN	16,204	0.110	0.031	0.072	0.079	0.091	0.105	0.122	0.147	0.165
CostofEquity-MPEG	16,204	0.114	0.046	0.065	0.074	0.087	0.104	0.130	0.168	0.198

This table presents descriptive statistics for the CostofEquity estimates used in this paper. CostofEquity is the average of four proxies for the Cost of Equity from Gebhardt et al. (2001), Claus and Thomas (2001), Ohlson and Juettner-Nauroth (2005), and Easton (2004), which are respectively titled CostofEquity-GLS, CostofEquity-CT, CostofEquity-OJN, CostofEquity-MPEG. These measures are calculated using current stock prices and IBES analyst forecast data that is available as of June in the subsequent year. Additional details regarding the construction of these variables is available in Appendix B.

Panel B: Correlations - Cost of Equity

Variable		**1**	**2**	**3**	**4**	**5**
CostofEquity-Mean	**1**	1.00	**0.63**	**0.78**	**0.90**	**0.81**
CostofEquity-GLS	**2**	**0.65**	1.00	**0.48**	**0.36**	**0.28**
CostofEquity-CT	**3**	**0.79**	**0.54**	1.00	**0.69**	**0.46**
CostofEquity-OJN	**4**	**0.91**	**0.36**	**0.69**	1.00	**0.81**
CostofEquity-MPEG	**5**	**0.82**	**0.27**	**0.47**	**0.87**	1.00

This table presents Pearson correlation coefficients above the diagonal and Spearman correlation coefficients below the diagonal. Cost of Equity variable definitions are given in Appendix B.

TABLE 3
DESCRIPTIVE STATISTICS

Panel A: Descriptive Statistics - Independent Variables

Variable	N	Mean	Std Dev	P5	P25	Median	P75	P90	P95	P99
Comparability	16,204	-2.568	1.476	-5.100	-4.320	-3.200	-2.290	-1.620	-1.040	-0.670
Restrict	16,204	6.963	4.859	0.368	0.689	3.513	6.408	9.712	13.302	15.819
Long-TermGrowth	16,204	0.147	0.088	0.043	0.060	0.100	0.130	0.180	0.250	0.300
ReturnOnAssets	16,204	0.115	0.165	-0.096	-0.005	0.063	0.115	0.169	0.248	0.332
MarketToBook	16,204	3.059	2.874	0.877	1.079	1.493	2.200	3.479	5.687	8.125
Leverage	16,204	0.668	0.945	0.000	0.000	0.062	0.394	0.868	1.524	2.317
MktValueofEquity	16,204	7.282	1.670	4.649	5.166	6.088	7.183	8.363	9.570	10.255
Momentum	16,204	0.194	0.460	-0.423	-0.290	-0.080	0.134	0.373	0.707	1.017
AnalystDisp	16,204	0.074	0.127	0.005	0.009	0.016	0.031	0.073	0.167	0.275
VW SystematicRisk	16,204	1.093	0.611	0.208	0.383	0.670	1.014	1.429	1.906	2.238
IdiosyncraticRisk	16,204	0.346	0.176	0.139	0.162	0.215	0.304	0.434	0.588	0.704
NetIncome	16,204	302.409	776.461	-28.077	-1.196	13.008	55.990	210.390	723.399	1,572.600
IncomeGrowth	16,204	-0.018	2.244	-2.549	-1.310	-0.349	0.073	0.360	1.095	2.249
DiscAccruals	13,517	0.090	0.104	0.004	0.009	0.024	0.056	0.114	0.207	0.301

This table presents descriptive statistics for the independent variables used in my analyses. All variables are defined in Appendix A.

TABLE 3 (CONT.)
DESCRIPTIVE STATISTICS

Variable		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Comparability	**1**		**-0.03**	**-0.14**	**-0.17**	**0.12**	**-0.06**	0.01	**0.05**	**-0.11**	**-0.18**	**-0.31**	**-0.27**	0.00	**0.05**	**-0.08**
CostofEquity	**2**	**0.05**		**-0.06**	**0.11**	**-0.13**	**-0.24**	**0.12**	**-0.29**	**-0.14**	**0.24**	**0.10**	**0.25**	**-0.09**	**-0.04**	**0.03**
Restrict	**3**	**-0.17**	**-0.10**		**-0.10**	**-0.09**	**-0.06**	**0.18**	**0.27**	**-0.02**	**0.06**	**0.10**	**-0.12**	**0.11**	**-0.03**	**-0.08**
Long-TermGrowth	**4**	**-0.22**	**0.04**	**-0.11**		**-0.10**	**0.23**	**-0.14**	**-0.18**	**0.16**	**0.18**	**0.30**	**0.40**	**-0.15**	-0.01	**0.15**
ReturnOnAssets	**5**	**0.10**	**-0.13**	**-0.16**	**-0.05**		**0.43**	**0.13**	**0.25**	**0.07**	**-0.25**	**-0.12**	**-0.22**	**0.25**	**0.16**	**-0.05**
MarketToBook	**6**	**-0.05**	**-0.37**	**-0.10**	**0.31**	**0.52**		**0.21**	**0.27**	**0.22**	**-0.06**	**0.06**	**0.03**	**0.12**	**0.04**	**0.11**
Leverage	**7**	**0.17**	**0.13**	**0.18**	**-0.33**	**0.03**	**-0.09**		**0.08**	**-0.03**	**0.05**	**-0.07**	**-0.09**	**0.03**	**-0.02**	**-0.07**
MktValueofEquity	**8**	**0.03**	**-0.29**	**0.26**	**-0.19**	**0.29**	**0.31**	**0.19**		**0.04**	**-0.19**	**-0.07**	**-0.50**	**0.63**	**0.05**	**-0.11**
Momentum	**9**	**-0.05**	**-0.17**	-0.01	**0.11**	**0.12**	**0.28**	**-0.03**	**0.11**		**-0.08**	**0.08**	**0.07**	**-0.02**	**0.06**	**0.04**
AnalystDisp	**10**	**-0.20**	**0.26**	**0.11**	**0.11**	**-0.37**	**-0.22**	**-0.03**	**-0.22**	**-0.15**		**0.18**	**0.27**	**-0.12**	**-0.05**	**0.04**
VW SystematicRisk	**11**	**-0.32**	**0.07**	**0.14**	**0.31**	**-0.12**	**0.05**	**-0.18**	**-0.05**	0.00	**0.25**		**0.39**	**-0.13**	**-0.03**	**0.09**
IdiosyncraticRisk	**12**	**-0.29**	**0.24**	**-0.14**	**0.45**	**-0.21**	**-0.03**	**-0.26**	**-0.53**	**-0.08**	**0.32**	**0.34**		**-0.27**	**-0.07**	**0.20**
NetIncome	**13**	**0.13**	**-0.19**	**0.14**	**-0.28**	**0.54**	**0.19**	**0.18**	**0.81**	**0.06**	**-0.32**	**-0.16**	**-0.56**		**0.06**	**-0.06**
IncomeGrowth	**14**	**0.11**	**-0.09**	**-0.07**	**0.04**	**0.38**	**0.15**	**-0.03**	**0.09**	**0.16**	**-0.20**	**-0.03**	**-0.12**	**0.27**		-0.01
DiscAccruals	**15**	**-0.11**	**0.03**	**-0.08**	**0.20**	**0.02**	**0.11**	**-0.16**	**-0.13**	0.01	**0.05**	**0.11**	**0.22**	**-0.15**	-0.01	

This table presents Pearson correlation coefficients above the diagonal and Spearman correlation coefficients below the diagonal. Variable definitions are given in Appendix A.

TABLE 4
COMPARABILITY

		Comparability		Comparability		Comparability		Comparability	
Intercept		1.3734	***	0.2238		0.3048	***	0.0126	
		[18.44]		[0.57]		[4.57]		[0.04]	
Restrict	**+/-**	**-0.0748**	***	**-0.0375**	***	**-0.0812**	***	**-0.0317**	**
		-[5.51]		**-[2.89]**		**-[4.94]**		**-[2.36]**	
Long-TermGrowth		-0.0299	***	-0.0282	***	-0.0139		-0.0113	
		-[2.93]		-[2.81]		-[1.08]		-[0.94]	
ReturnOnAssets		0.0950	***	0.0814	***	0.0681	***	0.0459	***
		[7.04]		[6.12]		[4.43]		[3.09]	
MarketToBook		-0.0085		-0.0003		-0.0821	***	-0.0749	***
		-[0.60]		-[0.02]		-[3.78]		-[3.46]	
Leverage		-0.0821	***	-0.1075	***	0.0125		-0.0223	
		-[6.37]		-[8.59]		[0.64]		-[1.23]	
MktValueofEquity		0.1236	***	0.1136	***	0.4965	***	0.4234	***
		[9.82]		[9.47]		[11.56]		[10.22]	
Momentum		-0.0836	***	-0.0836	***	-0.0694	***	-0.0639	***
		-[9.95]		-[9.74]		-[7.97]		-[7.37]	
Firm Fixed Effects		No		No		Yes		Yes	
Industry by Year Fixed Effects		No		Yes		No		Yes	
Industry Fixed Effects		Yes		No		No		No	
Year Fixed Effects		Yes		No		Yes		No	
Adjusted-R^2		0.433		0.547		0.671		0.771	
Observations		16,204		16,204		16,204		16,204	

Notes: This table presents regression estimates of financial statement comparability on GAAP limitations (Restrict) and control variables. These results examine the average relation between comparability and GAAP limitations. All variables are defined in Appendix A. The only difference being that all continuous variables in this table are standardized to have a mean of zero and a standard deviation of one. T-statistics (presented in parentheses) are calculated using heteroscedasticity robust standard errors clustered at the firm level. ***, **, * represent statistical significance at the 1, 5, and 10 percent levels respectively.

TABLE 5
CROSS-SECTIONAL VARIATION IN COMPARABILITY: TRANSACTION DISSIMILARITY

	Comparability		Comparability	
Intercept	0.7941	***	0.7941	***
	[17.85]		[17.86]	
Restrict	-0.0185	**	-0.0179	**
	-[2.50]		-[2.37]	
Dissimilarity	-0.0064		-0.0056	
	-[1.41]		-[1.23]	
Restrict*Dissimilarity	**-**		**-0.0071**	*****
			-[1.73]	
Total Effect at Dissimilarity= -1 [t-stat]			-[1.06]	
Controls Included	Yes		Yes	
Industry Fixed Effects	Yes		Yes	
Year Fixed Effects	Yes		Yes	
Adjusted-R^2	0.214		0.214	
Observations	882,184		882,184	

Notes: This table presents regression estimates of pair-wise financial statement comparability for firms *i* and *j* on GAAP limitations for firm *i*, dissimilarity between firms *i* and *j*, and the interaction of these variables. These results examine how the relation between comparability and GAAP limitations varies with transaction dissimilarity. All variables are defined in Appendix A. The only difference being that all continuous variables in this table are standardized to have a mean of zero and a standard deviation of one. T-statistics (presented in parentheses) are calculated using heteroscedasticity robust standard errors clustered at the firm level. ***, **, * represent statistical significance at the 1, 5, and 10 percent levels respectively. All columns include the following controls: Long-TermGrowth, ReturnOnAssets, MarketToBook, Leverage, MktValueofEquity, and Momentum.

TABLE 6
MERGERS AND ACQUISITIONS: TREATMENT SIMILARITY WITH SIMILAR TRANSACTIONS

		Comparability		Comparability	
Intercept		-3.1657	***	-3.2283	***
		-[13.30]		-[13.63]	
SimilarMethod	+	0.1398	**	0.1450	**
		[2.38]		[2.45]	
Restrict				-0.1223	***
				-[5.08]	
Controls Included		Yes		Yes	
Industry Fixed Effects		Yes		Yes	
Year Fixed Effects		Yes		Yes	
Adjusted-R^2		0.394		0.398	
Observations		13,212		13,212	

Notes: This table presents regression estimates of financial statement comparability on accounting treatment similarity. SimilarMethod is the industry-year number of firm pairs that used the same accounting method for mergers and acquisitions (i.e., both used pooling or purchase method) divided by the number of firm pairs that had mergers in that industry-year (i.e., both firms had mergers). These results examine the relation between comparability and accounting method similarity. All variables are defined in Appendix A. The only difference being that all continuous variables (excluding SimilarMethod) in this table are standardized to have a mean of zero and a standard deviation of one. T-statistics (presented in parentheses) are calculated using heteroscedasticity robust standard errors clustered at the firm level. ***, **, * represent statistical significance at the 1, 5, and 10 percent levels respectively. All columns inlude the following controls: Long-TermGrowth, ReturnOnAssets, MarketToBook, Leverage, MktValueofEquity, and Momentum.

TABLE 7
SOFTWARE DEVELOPMENT: TREATMENT SIMILARITY WITH DISSIMILAR TRANSACTIONS

		Comparability		Comparability	
Intercept		0.1388	**	0.0947	
		[2.00]		[0.80]	
Both		**0.0650**	***	**0.0711**	**
		[3.06]		**[2.54]**	
Either		-0.0318	*	0.0355	**
		-[1.87]		[2.06]	
Post		-0.0474	***	-0.0681	***
		-[4.20]		-[4.30]	
Post*Both	**-**	-0.0817	***	-0.1525	***
		-[4.71]		-[5.38]	
Post*Either	**-**	-0.0531	***	-0.1007	***
		-[3.21]		-[5.42]	
Industry fixed effects		Yes		Yes	
Adjusted-R^2		0.018		0.030	
Observations		690,997		322,191	

Notes: This table presents difference-in-difference regression estimates of pair-wise financial statement comparability for firms i and j on GAAP restrictions for firm i, a post indicator, *Both*, *Either*, the interactions of post with *Both* and separately, with *Either.* Post is equal to one if SFAS 86 was effective for that fiscal year (i.e., fiscal years ended after December 15, 1986) and zero otherwise. *Both* is equal to one if firm *i* and firm *j* both are in the treatment group (see below), otherwise *Both* is equal to zero. *Either* is equal to one if firm *i* or firm *j*, but not both, are in the treatment group, otherwise *Either* is equal to zero. A firm is included in the treatment group in Column 1 if the firm engages in research and development activities. A firm is included in the sample of Column 2 if the firm engages in research and development activities. A firm is included in the treatment sample of Column 2 if the firm is in an industry that is likely to engage in software development (i.e., electronics [SIC 3600-3674] and computers [SIC 3570-3577; 7370-7374]). The sample is limited to fiscal years between 1984, 1985, 1987 and 1988. T-statistics (presented in parentheses) are calculated using heteroscedasticity robust standard errors clustered at the firm level. ***, **, * represent statistical significance at the 1, 5, and 10 percent levels respectively. (i.e., research and development expense is not equal to zero)

TABLE 8

COST OF EQUITY

		CostofEquity		CostofEquity		CostofEquity		CostofEquity	
Intercept		0.0778	***	0.0798	***	0.0815	***	0.0841	***
		[45.70]		[47.67]		[48.34]		[47.77]	
Restrict	+/-	**0.0012**	***	**0.0011**	***	**0.0009**	***	**0.0006**	**
		[3.62]		**[3.14]**		**[2.60]**		**[1.99]**	
AnalystDisp	+			0.0039	***	0.0037	***	0.0034	***
				[11.35]		[10.88]		[9.89]	
Long-TermGrowth	+	0.0050	***	0.0044	***	0.0041	***	0.0036	***
		[15.12]		[13.20]		[12.02]		[10.62]	
ReturnOnAssets	?	-0.0005	*	0.0003		0.0004		0.0008	***
		-[1.68]		[1.09]		[1.55]		[2.62]	
MarketToBook	-	-0.0047	***	-0.0049	***	-0.0051	***	-0.0054	***
		-[12.66]		-[13.23]		-[13.59]		-[14.47]	
Leverage	+	0.0047	***	0.0044	***	0.0044	***	0.0044	***
		[15.17]		[14.27]		[14.43]		[14.63]	
MktValueofEquity	-	-0.0052	***	-0.0045	***	-0.0047	***	-0.0029	***
		-[15.68]		-[13.69]		-[14.23]		-[7.99]	
Momentum	-	-0.0022	***	-0.0018	***	-0.0019	***	-0.0021	***
		-[9.81]		-[8.33]		-[8.59]		-[9.92]	
SystematicRisk	+					0.0021	***	0.0008	***
						[8.15]		[2.84]	
IdiosyncraticRisk	+							0.0042	***
								[10.26]	
Ind. and Year Fixed Effects		Yes		Yes		Yes		Yes	
Adjusted-R^2		0.337		0.356		0.361		0.369	
Observations		16,204		16,204		16,204		16,204	

Notes: This table presents regression estimates of the implied cost of equity on GAAP limitations (Restrict) and control variables. These results examine the average relation between the implied cost of equity and GAAP limitations. All variables are defined in Appendix A. The only difference being that all continuous independent variables in this table are standardized to have a mean of zero and a standard deviation of one. T-statistics (presented in brackets) are calculated using heteroscedasticity robust standard errors clustered at the firm level. ***, **, * represent statistical significance at the 1, 5, and 10 percent levels respectively.

TABLE 9
MEDIATION ANALYSIS

Panel A: Model 1	CostofEquity	
Restrict	0.0012	***
	[3.62]	
Adjusted-R^2	0.337	
Panel B: Model 2	Comparability	
Restrict	-0.0748	***
	-[5.51]	
Adjusted-R^2	0.433	
Panel C: Model 3	CostofEquity	
Restrict	0.0010	***
	[2.90]	
Comparability	-0.0036	***
	-[10.12]	
Indirect Effect:	**0.0003**	***
	[4.84]	
Percent Mediated	21.42%	
Controls Included	Yes	
Ind. Fixed Effects	Yes	
Year Fixed Effects	Yes	
Adjusted-R^2	0.348	
Observations	16,204	

Notes: The panels of this table present regression estimates of 1) the implied cost of equity on GAAP limitations (Restrict), 2) comparability on GAAP limitations, and 3) the implied cost of equity on GAAP limitations and comparability. Indirect effects are calculated by multiplying the coefficient on Restrict in Panel B with the coefficient on the information measure in Panel C. T-statistics are calculated using heteroscedasticity robust standard errors clustered at the firm level. ***, **, * represent statistical significance at the 1, 5, and 10 percent levels respectively. Controls include Long-TermGrowth, ReturnOnAssets, MarketToBook, Leverage, MktValueofEquity, and Momentum.

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