THE ACCOUNTING REVIEW
Vol. 89, No. 2
2014
pp. 605–633

American Accounting Association
DOI: 10.2308/accr-50642

Auditor Style and Financial Statement Comparability

Jere R. Francis
University of Missouri

Matthew L. Pinnuck
The University of Melbourne

Olena Watanabe
Iowa State University

ABSTRACT: The term "audit style" is used to characterize the unique set of internal working rules of each Big 4 audit firm for the implementation of auditing standards and the enforcement of GAAP within their clienteles. Audit style implies that two companies audited by the same Big 4 auditor, subject to the same audit style, are more likely to have comparable earnings than two firms audited by two different Big 4 firms with different styles. By comparable we mean that two firms in the same industry and year will have a more similar accruals and earnings structure. For a sample of U.S. companies for the period 1987 to 2011, we find evidence consistent with audit style increasing the comparability of reported earnings within a Big 4 auditor's clientele.

Keywords: *earnings; comparability; Big 4 accounting firms*.

Data Availability: *All data are publicly available from the sources identified in the text*.

I. INTRODUCTION

Comparability is defined by the Financial Accounting Standards Board (FASB) as the quality of information that enables users to identify similarities and differences in the financial performance of two firms. The joint conceptual framework project of FASB and the International Accounting Standards Board (IASB) emphasizes that comparability is a basic property of financial information that enhances its usefulness (Financial Accounting Standards Board 2010). Indeed the FASB states that comparability in financial reporting is the primary reason

We thank the editors, Michael L. Ettredge and John Harry Evans III, and the two reviewers for their guidance and constructive suggestions. We also appreciate the comments of workshop participants at The University of Arizona, Bocconi University, Bond University, HEC Montreal, Laval University, Michigan State University, University of Missouri, Washington University in St. Louis, and the comments of participants at the 2011 American Accounting Association Annual Meeting and the 2011 International Symposium on Audit Research Annual Meeting. Professor Francis also holds an appointment as Distinguished Professor, University of Technology, Sydney.

Editor's note: Accepted by Michael L. Ettredge.

Submitted: March 2012
Accepted: October 2013
Published Online: October 2013

for developing accounting standards (FASB 1980, para. 112), and the centrality of comparability is stressed in accounting textbooks, particularly financial statement analysis texts (Revsine, Collins, Johnson, and Mittelstaedt 2011; Phillips, R. Libby, and P. Libby 2013).

The primacy of comparability as a qualitative characteristic of accounting makes it important to understand the factors that give rise to this characteristic. The emerging research into the determinants of comparability has focused on the role of accounting standards such as the adoption of IFRS (Barth, Landsman, Lang, and Williams 2012; Lang, Maffett, and Owens 2010). However, accounting standards on their own do not fully determine financial reporting outcomes; economic agents and institutional incentives also play an important role (Ball, Robin, and Wu 2003; Leuz, Nanda, and Wysocki 2003). This motivates our investigation of the role that auditors play in the implementation of comparability in the United States. An advantage of studying financial statement comparability in a single-country setting is that we avoid the potential confounding effect of institutional differences in cross-country studies.

The concept and use of the word *comparability* differs in the literature. For the purpose of our study we define accounting comparability as the closeness of two firms' reported earnings due to the consistency with which rules are applied across firms. In our empirical context, this means that firm-pairs in the same industry and fiscal year, and therefore subject to the same general economic shocks, are expected to have a similar accruals and earnings structure, all things being equal. However, there are frictions in the interpretation, implementation, and enforcement of accounting standards that can reduce inter-company comparability. Our study focuses on the role of the auditor and, following Kothari, Ramanna, and Skinner (2010), we argue that each Big 4 audit firm has its own unique set of internal working rules that guide and standardize the auditor's application of auditing and accounting standards. These working rules give rise to what we term *audit style*, with the consequence that audit firms have systematic differences in their audit approaches, and in their interpretation and enforcement of accounting standards. As a result, we expect reported accruals and earnings to be more consistent and comparable within an audit firm's clientele than between audit firm clienteles. This leads to our main hypothesis: two companies audited by the same Big 4 auditor, and therefore subject to the same *audit style*, are more likely to have comparable earnings than two companies audited by two different Big 4 auditors and subject to different *audit styles*.[1]

We measure accounting comparability in three ways. The first approach is to examine differences in year-specific total and abnormal accruals between pairs of firms in the same industry using the same Big 4 auditor versus firm-pairs with two different Big 4 auditors. The second approach measures the degree to which the earnings of a pair of firms in the same industry, and audited by the same Big 4 auditor, covary across time (Barth et al. 2012; De Franco, Kothari, and

[1] Some recent examples provide anecdotal evidence of auditor style effects. In a field study, Dichev, Graham, Harvey, and Rajgopal (2013, 32) cite CFOs on the effect of auditors on earnings:

"The big [accounting] firms are not passing authority downstream to the regional headquarters or onto the actual auditors like they used to . . . Interpretation of these rules in the accounting firms comes from high above now rather than from the field."

". . . earlier you could work with your local accounting firm, your local partner and accomplish things. Now pretty much everything goes up to their think tank at national."

The implication is that the work of accounting firms is standardized across their clienteles, consistent with a "style" effect. Another example is from Blacconiere, Frederickson, Johnson, and Lewis (2011) who study firms making voluntary disclosures that disavow the reliability of mandated fair value information. Their descriptive statistics show that Ernst & Young (E&Y) clients are four times more likely to disavow the reliability of mandated fair value disclosures than are the clients of other national audit firms. E&Y included a disavowal as an illustrative disclosure in its SFAS No. 123 implementation guidance (E&Y 1995), and their clients used this wording almost verbatim in their disavowal disclosures.

Verdi 2011; Lang et al. 2010). The third approach follows the CEO/CFO style literature and uses an auditor fixed effects model to examine the commonality of accruals for auditor clienteles (Bamber, Jiang, and Wang 2010; Ge, Matsumoto, and Zhang 2011).

The primary tests are based on pairs of firm-year observations from Compustat in the same industry-year for the period 1987 to 2011. Consistent with our main hypothesis, we find that two firms in the same industry-year and audited by the same Big 4 auditor have more comparable earnings than two firms audited by two different Big 4 auditors. These results are consistent across the three empirical tests: pairs of firms in the same industry-year with the same Big 4 auditor have more similar total and abnormal accruals; firm-pairs with the same Big 4 auditor have a higher covariation in earnings over time; and auditor fixed effects are a statistically significant determinant of accruals. These results are robust to a set of controls for other factors that might create frictions or otherwise affect the comparability of accruals and earnings for firm-pairs. The findings are consistent with each Big 4 audit firm having a unique style that increases the comparability of earnings within its clientele relative to firm-pairs with two different Big 4 auditors. Economic magnitudes of these effects are reported in Section IX.

We also examine whether a pair of companies audited by the same Big 4 auditor will have more comparable earnings than a pair of companies audited by non-Big 4 auditors. The test is based on the argument that Big 4 auditors, due to their size and economies of scale, will have a greater capacity than non-Big 4 auditors to incur the fixed costs in developing standardized in-house rules for implementing auditing standards and the enforcement of GAAP. Consistent with this prediction we report evidence that Big 4 auditors have a greater effect on accounting comparability than non-Big 4 auditors.

The study makes several contributions to the literature. This is the first study to hypothesize and test the role of economic institutions within a country in the production of comparability. The existing debate and empirical evidence with regard to the production of comparability has focused almost exclusively on the role of standards themselves, especially FASB versus IFRS. Our study provides evidence that an economic institution—the auditor—is also an important factor in the production of financial statement comparability. Consistent with the joint FASB/IASB conceptual framework, our results suggest that accounting standards alone may not necessarily lead to comparability, but that the effects of standards are also dependent upon audit firms involved in the enforcement of GAAP. As such, we document a new channel through which auditor characteristics affect audited financial statements.

Second, we contribute to the debate on principles versus rules in the development of accounting standards by regulators. Kothari et al. (2010) argue that regulators should not be concerned with the potential for non-comparability if accounting standards are principle-based, because accountants and auditors who are involved in the day-to-day application of principles will develop "working rules" to standardize accounting practice. Our results suggest that this standardization process occurs within the clientele of one auditor; however, there are significant "style differences" between audit firms that reduces inter-auditor comparability.

Third, we contribute to the broader literature that examines the auditor's role in the production of financial reports. This literature has mainly examined the role of auditing in curbing earnings management, which is related to the qualitative characteristic of "representational faithfulness" (FASB 2010). We show that the unique style of each Big 4 auditor affects the qualitative characteristic of comparability, and this is another source of variation within the Big 4 group of auditors. Our study and results are therefore related to Bamber et al. (2010) who report that individual corporate managers have their own individual style in choice of voluntary corporate financial disclosure, and Ge et al. (2011) who find that CFO style affects the choice of accounting policies. We extend the concept of unique styles in the production of financial reports from

individuals to accounting firms. Our finding is analogous to the finance literature that examines mutual funds and documents that funds have their own unique styles (Barberis and Shleifer 2003).

The next section develops the study's two hypotheses. The research design is presented in Section III, and sample selection and data are summarized in Section IV. Empirical results are reported in Sections V through VIII. Section IX discusses the economic magnitudes of the results, and Section X concludes the study.

II. HYPOTHESIS DEVELOPMENT

Two lines of research are relevant to this study: research that examines financial statement comparability, and research linking auditor characteristics with earnings attributes. While the importance of comparability has long been recognized by standard-setters, and discussed in the academic literature at a conceptual and normative level, there is much less empirical research on comparability. Recent empirical papers have emerged in response to the development of new methodologies to measure comparability, and to the widespread adoption of IFRS. These papers examine how the adoption of IFRS affects financial statement comparability, and how improved comparability affects decisions by investors. For example, Barth et al. (2012) examine the comparability of financial statements of non-U.S. firms that adopt IFRS with that of U.S. firms, and find that IFRS adoption by non-U.S. firms enhances their financial statement comparability with U.S. firms. Lang et al. (2010) examine changes in cross-country financial statement comparability around mandatory IFRS adoption and document that IFRS adoption increases comparability, measured as cross-country earnings co-movement. Other recent papers have examined whether comparability affects the decisions of participants in the capital market. De Franco et al. (2011) find that earnings comparability within an industry is positively related to analysts' following and accuracy, and negatively related to analysts' optimism and dispersion in earnings forecasts. Bradshaw, Miller, and Serafeim (2011) also study analysts and find that the commonality of accounting policy choices, their measure of comparability, affects analyst coverage and behavior. Last, DeFond, Hu, Hung, and Li (2011) show that mutual funds increase their foreign investment in countries with mandatory IFRS adoption, which they argue is due to improved cross-country earnings comparability.

Turning to the auditing literature, a large body of research has examined the association of auditor characteristics with clients' audited earnings. The seminal studies linking auditors and earnings attributes are Becker, DeFond, Jiambalvo, and Subramanyam (1998) and Francis, Maydew, and Sparks (1999), who document that Big 4 clients have smaller abnormal accruals than non-Big 4 clients. This stream of research has also examined other earnings attributes such as benchmark beating (Burgstahler and Dichev 1997; Frankel, Johnson, and Nelson 2002), accruals quality (Dechow and Dichev 2002; Doyle, Ge, and McVay 2007), and timely loss recognition (Basu 1997; Krishnan 2005). Francis (2004, 2011) reviews the empirical auditing literature and describes auditor characteristics associated with earnings quality, including the Big 4/non-Big 4 dichotomy, the auditor's industry expertise (Reichelt and Wang 2010), and engagement-specific factors such as client size (Reynolds and Francis 2000), auditor tenure (Johnson, Khurana, and Reynolds 2002), auditor-provided nonaudit services (Frankel et al. 2002), and the presence of audit firm alumni in executive positions of clients (Menon and Williams 2004).

We bring the comparability and audit research streams together to investigate the role of the auditor in comparability. While prior comparability research has examined the capital market effects from the global harmonization of standards, we know of no attempts to empirically document the role of economic agents such as auditors on financial statement comparability. Barth et al. (2012) recognize that accounting reports are the result of a complex interaction of the features of the financial reporting system that include accounting standards, their interpretation,

enforcement, and litigation, all of which can affect comparability. Apart from the actual accounting standards themselves, which are exogenously given, the auditor is actively involved in all of these features of the financial reporting system. The audit research literature, in turn, has focused on the role of the auditor in facilitating the reporting of high-quality earnings, with the primary emphasis on accruals quality and earnings management behavior. We extend this line of research to investigate the role the auditor plays in facilitating comparability.

Our argument is that each Big 4 audit firm has its own unique audit testing approach for implementing GAAS along with in-house working rules for interpreting and applying GAAP. The policies of each Big 4 firm will give rise to what we term an "audit style," and we expect auditor style to have a systematic effect on clientele earnings. It is well-known that each Big 4 accounting firm has its own unique audit methodology and testing procedures. For example, Kinney (1986) classified the then Big 8 accounting firms based on their use of unstructured, intermediate, and structured audit technologies. While audit methods/procedures must comply with generally accepted audit standards (GAAS), the audit standards are themselves rather general in nature and much more principles-based than is U.S. GAAP. This means that each accounting firm must devise its own in-house working rules for the efficient and consistent implementation of GAAS across its client base (Cushing and Loebbecke 1986). Auditors also attempt to differentiate themselves from one another based on their methodologies. For example, in the 1980s there was a dichotomy between auditors that used a quantitative approach versus those that used a qualitative methodology (Kaplan, Williams, and Menon 1990). In the 1990s, KPMG promoted its "business risk" audit as an innovation (Bell, Marrs, Solomon, and Thomas 1997). These divergent practices are also illustrative of the kinds of technical innovation that Kothari et al. (2010) argue is more likely to occur when standards, in this case auditing standards, are principles-based rather than rule-based. The unique character of audit methodologies implies that each firm's audit approach will systematically detect or not detect the same client errors, including GAAP implementation errors. The implication is that financial statements will be more similar for firm-pairs with the same auditor, *ceteris paribus*, than for firm-pairs with two different auditors each having different styles.[2]

Turning to style effects that arise from GAAP interpretation, it may not be as well-known, but each of the Big 4 accounting firms also has in-house rules for interpreting and implementing GAAP, just as it has for implementing auditing standards (GAAS). Kothari et al. (2010) develop the general argument that a principles-based approach to GAAP does not eliminate the role of "rules." Instead a principles-based approach to GAAP will result in economic agents such as auditors developing in-house "working rules" for the consistent interpretation and implementation of standards. Kothari et al. (2010, 277) state that:

> It is not likely to be cost effective for accountants and auditors to work with principles on a day-to-day basis. Authority on interpreting and implementing GAAP in an economy has to be delegated to thousands of rank-and-file accountants and auditors (for reasons of efficiency); this is possible only if working rules are formulated out of principles.

While there is more guidance in U.S. GAAP than in the relatively more principles-based international financial reporting standards (IFRS), the Big 4 accounting firms will still find it advantageous to develop their own in-house working rules because U.S. GAAP still requires

[2] Vera-Muñoz, Ho, and Chow (2006), Banker, Chang, and Kao (2002), Dowling (2009) report that accounting firms also use information technology systems to standardize the implementation of audit methodologies and that firms also standardize the documentation of audits using electronic templates for working papers that embed the audit firm's methodology. Zerni (2012) identifies the following software products: KPMG's KWorldTM, PricewaterhouseCoopers' TeamAssetTM and KnowledgeCurveTM, and Ernst & Young's KnowledgeWebTM.

considerable judgment in interpreting and implementing accounting standards. As a result, each Big 4 firm has its own in-house GAAP guide that is used internally by its auditors in the field.

To illustrate this point, we have identified the following current products developed by each Big 4 accounting firm for internal use by audit staff:

- Deloitte: Deloitte Technical Library (http://www.deloitte.com/us/techlibrary); Deloitte Roadmap (http://www.deloitte.com/view/en_US/us/Services/audit-enterprise-risk-services/Financial-Statement-Internal-Control-Audit/Accounting-Standards-Communications/980bef5fe91fb110VgnVCM100000ba42f00aRCRD.htm).
- Ernst & Young: Global Accounting and Auditing Information Tool (GAAIT) (http://www.ey.com/GL/en/Services/Assurance/Assurance-Key-A-A-Guidance-On-Ernst—Young-Online—Global-Accounting—Auditing-Information-Tool).
- KPMG: Accounting Research Online (http://www.kpmg.com/global/en/topics/accounting-research-online/pages/default.aspx).
- PricewaterhouseCoopers: Accounting Guides (http://www.cfodirect.pwc.com/CFODirectWeb/Controller.jpf?NavCode=MSRA-777JJY).

Each Big 4 firm explicitly states that their product represents a guide for the interpretation and application of GAAP. For example, Ernst & Young characterize their product as a global online resource for accounting and auditing standards, which represents Ernst & Young's interpretative guidance for U.S. GAAP, international GAAP, and other GAAP systems. Deloitte says its Technical Library provides interpretative guidance for GAAP, and KPMG says its guide contains regulatory pronouncements and KPMG's guidance on new pronouncements including illustrative examples to facilitate the practical application of standards.

While accounting firms originally developed these materials for internal use by their audit staff, they also provide some of the same information to their clients. In other words, clients are likely to be using their auditor's GAAP guidance products in preparing financial statements. Deloitte's Technical Library has a subscription price of $2,000, and Ernst & Young's Global Accounting and Auditing Information Tool (GAAIT) has a base price of $750. KPMG's Accounting Research Online is also available by subscription, and PricewaterhouseCoopers provides their accounting guides by subscription through its CFOdirect network. In addition, our discussions with Big 4 practitioners reveal that when a complex accounting issue arises in the preparation of financial reports, the CFO will often seek guidance from the Technical Department of the firm's Big 4 auditor.[3]

The working rules of Big 4 auditors are an important mechanism through which GAAP is operationalized and implemented by both auditors and their clients, even within the United States with its arguably more explicit rule-based standards. The result is that two companies with the same Big 4 accounting firm as their auditor are more likely to interpret and implement GAAP in the same way, including the role of the auditor in enforcing GAAP and detecting GAAP misapplications for its clientele through the firm's standardized audit methodology.

Thus, if there are auditor style effects on their clienteles' financial reports, we should observe greater consistency in the financial statements of two companies in the same industry-year audited

[3] The existence of in-house rules is supported by Acito, Burks, and Johnson (2009) who investigate accounting for operating leases and subsequent restatements for the misreporting of these leases. Acito et al. (2009, Table 4) use an auditor fixed effects model and finds systematic differences between auditor clienteles in the frequency of restatements that corrected lease accounting errors. This finding suggests that auditors had different GAAP interpretations with respect to lease accounting, which resulted in different rates of subsequent client restatements.

by the same Big 4 audit firm relative to firm-pairs with two different Big 4 audit firms where the effect of style is randomized away. Our first hypothesis, in alternative form, is:

H1: A pair of companies audited by the same Big 4 audit firm will have more comparable earnings than a pair of companies audited by two different Big 4 audit firms.

DeFond et al. (2011) point out that while comparability is the desired outcome of adopting a set of uniform accounting standards, uniformity alone does not necessarily result in comparability. In particular, the standards and in-house rules must also be faithfully implemented. This leads us to predict that the financial statements of a pair of firms with the same Big 4 auditor will have a greater comparability than financial statements of a pair of firms with the same non-Big 4 auditor. Because Big 4 auditors are larger, they have a greater capacity to incur the fixed-cost investments in audit programs and in-house rules for interpreting and implementing GAAP through "technical guidelines." Second, because Big 4 auditors have a larger and more dispersed staff, they have greater incentive/need for staff controls than smaller firms. For these reasons we predict that Big 4 audit firms will have a greater style effect than non-Big 4 audit firms. Finally, higher quality auditors are more likely to correctly apply accounting standards, and prior research has found that non-Big 4 auditors are associated with lower-quality audited earnings (Teoh and Wong 1993; Becker et al. 1998; Francis et al. 1999).[4] Because managers have flexibility in the application of accounting standards, comparability will be greater among companies with Big 4 auditors because the accounting standards are applied on a more consistent and correct basis. Thus, we predict increased financial statement comparability for firms with Big 4 auditors, and the second hypothesis in alternative form is:

H2: A pair of companies audited by the same Big 4 audit firm will have more comparable earnings than a pair of companies audited by the same non-Big 4 audit firm.

III. RESEARCH DESIGN

Prior earnings comparability research has typically analyzed either (1) cross-sectional similarities in the levels of contemporaneous measures (Joos and Lang 1994); or (2) the correlation of earnings (covariation) across time (Barth et al. 2012; De Franco et al. 2011).[5] To test H1 we build on this research and use method (1) to test cross-sectional similarities of accruals for firm-pairs, and method (2) to test the correlation of earnings for firm-pairs across time. For robustness, and to provide a link to the CEO/CFO style literature, we use an auditor fixed effects model to test for systematic accounting similarities within audit firm clienteles.

Empirical Tests of H1

Differences in Accruals

Our first approach to testing accounting comparability is to examine the similarity or "closeness of accruals" for pairs of firms in the same industry, at a common point in time, conditional on audit firm. This approach is based on and extends prior comparability research that has examined similarities in cross-sectional levels of contemporaneous measures such as return on

[4] There is also evidence that mid-tier audit firms with national practices provide higher quality audits than smaller non-Big 4 firms (Francis et al. 1999), and in the post-SOX period some studies report that audit quality differences have narrowed between the Big 4 firms and mid-tier firms (Boone, Khurana, and Raman 2010; Cassell, Giroux, Meyers, and Omer 2013).

[5] An alternative approach uses the similarity of accounting policy choices (Bradshaw and Miller 2007; Bradshaw et al. 2011). We do not use this approach because of limited data availability on accounting method choices.

equity and price multiples to investigate cross-country convergence in firm-specific earnings multiples (Joos and Lang 1994; Land and Lang 2002). Our approach is conceptually similar except that we are examining auditors as an institutional factor giving rise to comparability and the convergence of earnings within a single country.

Our analysis examines accruals because it is the primary component of earnings that is subject to discretion and is the component through which economic agents such as auditors can most directly affect comparability. The premise is that two firms in the same industry and year, and audited by the same Big 4 auditor, are more likely to have the same type of accrual adjustments due to audit methodology, and to make the same set of accounting choices and judgments in implementing GAAP. Therefore, the accruals structure of these firms will be more similar than that of two firms with different auditors where the effect of audit style will be randomized away. We measure the similarity of accruals as follows:

$$Diff\_Total\_Accruals_{ijt} = abs(Total\_Accruals_{it} - Total\_Accruals_{jt}), \quad (1)$$

where $Diff\_Total\_Accruals_{ijt}$ is the absolute value of the difference between signed total accruals for firm-pairs in the same SIC two-digit industry classification in year t. We calculate this comparability metric for each firm i and firm j pairwise combination, for J firms in the same industry and fiscal year. We control for economic fundamentals and exogenous shocks because our analysis examines firm-pairs within the same industry and year where the accruals structure should be similar, *ceteris paribus*.

Total accruals are calculated as the difference between income before extraordinary items and cash flows from operations adjusted for cash flows from extraordinary items (IB − (OANCF − XIDOC)), scaled by beginning of year total assets. We use the same approach to calculate differences in abnormal accruals, which we label $Abn\_Accr\_Diff_{ijt}$. Abnormal accruals are calculated using the Jones (1991) model of discretionary accruals, with control for contemporaneous performance (Kothari, Leone, and Wasley 2005). H1 predicts that financial statements for firm-pairs in the same industry-year with the same Big 4 auditor will report a more similar accrual structure and, therefore, will have smaller differences in both total accruals and abnormal accruals.

Earnings Covariation

A second way of measuring accounting comparability is the degree to which earnings for firm-pairs in the same industry covary over time (Barth et al. 2012; De Franco et al. 2011). This approach should be less subject to omitted variables that could affect the accrual-difference metric that may capture a number of properties other than comparability. The specific approach we employ follows De Franco et al. (2011; hereafter DKV), who measure comparability as the degree to which earnings for two firms in the same industry covary over time.[6] Following DKV, we measure the level of covariance as the adjusted R^2 from the following regression:

[6] DKV suggest two approaches for measuring comparability, an approach based on the similarity of the mapping of earnings to stock returns across firms, and an approach based on the covariation in earnings across firms. We use the latter approach in our study, the earnings covariation metric, because it is a more direct test of the arguments that underpin our hypothesis. As Lang et al. (2010) point out, the DKV comparability metric of mapping earnings to stock returns measures whether earnings are similarly capturing the underlying economics, while an earnings covariation metric captures anything that creates earnings similarity, regardless of whether the underlying economics are indeed similar. H1 in our study is underpinned by the concept of auditors having in-house rules that cause them to impose the same accounting choices on their clientele, regardless of the underlying economics, which leads to covariation in earnings. For this reason we use the earnings covariation metric and do not measure comparability based on the similarity of the mapping of earnings to stock returns because it is a fundamentally different construct.

$$Earnings_{iq} = \alpha_{0ij} + \alpha_{1ij} Earnings_{jq} + \varepsilon_{ijq}, \quad (2)$$

where *Earnings* is income before extraordinary items for firm *i* and firm *j* for quarter *q* scaled by average total assets of each firm. The model in Equation (2) is estimated over 16 consecutive quarters *q* for all unique pairs of firms in the same two-digit SIC industry. We measure the accounting comparability of firm *i* and firm *j* in Equation (2) as the adjusted R^2 from the regression, hereafter referred to as earnings comparability covariation, or *ECOMP_COV*. Higher values of *ECOMP_COV* indicate greater earnings comparability between firm-pairs. H1 predicts that earning covariation is greater for firm-pairs with the same Big 4 auditor.[7]

A potential limitation of the earnings covariation metric is that it does not explicitly control for economic shocks that are crucial to isolating accounting comparability. Following DKV, we address this issue in three ways. First, we perform all our analyses on firm-pairs within industry by year, thereby controlling for common economic fundamentals and shocks. Second, we control for contemporaneous cash flow covariation for firm-pairs, which is measured analogously to *ECOMP_COV*. Specifically, *CFO_COMP_COV* is created in an identical manner to *ECOMP_COV* except that in Equation (2) we replace *Earnings* with *CFO*, which is the ratio of quarterly cash flow from operations to the beginning of period market value. Finally, we control for contemporaneous monthly stock return covariation (*RET_COV*) for firm-pairs, which is measured analogously to *ECOMP_COV*. Stock returns will reflect all economic shocks and provide a further control for the effect of underlying economic fundamentals on accruals.

Regression Models

To examine the relation between accounting comparability and auditor style we estimate the following OLS regression models:

$$Diff\_Total\_Accruals_{ijt}(Abn\_Accr\_Diff_{ijt}) = \alpha_{0ij} + \alpha_1 Same\_Big4_{jt} + \alpha_2 Controls + \varepsilon_{ijt}. \quad (3)$$

$$ECOMP\_COV_{ijt} = \alpha_{0ij} + \alpha_1 Same\_Big4_{jt} + \alpha_2 CFO\_COMP\_COV + \alpha_3 RET\_COV + \alpha_4 Controls. \quad (4)$$

All tests are based on robust t-statistics that control for heteroscedasticity and with standard errors clustered at the firm level to control for potential non-independence (Petersen 2009). Results are robust to clustering by both firm and year, and to alternative clustering by unique firm-pairs.[8]

The regression models in Equations (3) and (4) are estimated using a sample of firm-pairs that have the same Big 4 auditor versus firm-pairs with two different Big 4 auditors. Because our data

[7] The earnings covariation metric uses quarterly earnings data and the observable effects of the auditor on comparability may be weaker in quarterly data compared to annual earnings because only the annual earnings are subject to a full-scope audit (Brown and Pinello 2007). In contrast, interim earnings numbers for the first three quarters are simply reviewed by the auditor. Under the integrated reporting model, fourth quarter earnings must be equal to annual earnings less the sum of the first three quarters, so the fourth quarter earnings can be also noisy due to the implicit adjustments for prior-quarter estimation errors. While we can expect the auditor to influence the implementation of GAAP over the entire year, we do not expect the influence of "audit style" on earnings comparability to be as strong in quarterly data as it would be in fully audited annual earnings. This results in a design trade-off. While annual data would be more appropriate in our research context, it is not practical to use annual data given the long time-series required to calculate earnings covariation. Further, the testing framework requires that a firm have the same auditor over the time-series, which is also less likely when using a longer time-series of annual data.

[8] The dependent variable in regression model (3) is truncated at 0, which under certain conditions can lead to biased OLS estimators. However, this is unlikely to be an issue in our setting as we have very few 0-value observations. Nevertheless we also estimate model (3) using a Tobit regression and the results remain unchanged.

extend to the era of the Big 8 accounting firms, each accounting firm is treated as unique for the years in which it exists in the sample data. For example, a firm audited by Price Waterhouse in 1997, and by PricewaterhouseCoopers in 1998, is treated as having a different auditor in each year. For convenience, we use the term "Big 4" to refer to all of these auditors.

To test H1 we use the indicator variable, *Same_Big4*, which is coded 1 if the auditor for a pair of firms is the same Big 4 firm, and 0 if auditors in a pair of firms are two different Big 4 auditors. In Equation (3) we predict a negative coefficient on *Same_Big4* because a lower value of *Diff_Total_Accruals* indicates a smaller difference in accruals and, hence, greater cross-sectional comparability of earnings. In contrast, in Equation (4), we predict a positive coefficient on *Same_Big4* because a larger value of *ECOMP_COV* indicates greater earnings covariation over time and, hence, greater time-series comparability of earnings.

Lang et al. (2010) point out that there is no theoretical or empirical guidance concerning appropriate control variables to include in a regression that explains earnings comparability, and they include control variables for size and market-to-book on the basis that these variables are widely used to capture many unobservable firm-specific characteristics. We include these variables but also control for a wider range of other variables identified in the literature that could result in the earnings between two firms being similar due to either economic fundamentals (e.g., volatility of operations) or the propensity to manage earnings (e.g., market-to-book ratio or leverage). The full set of control variables are: size, leverage, market-to-book, cash flow from operations, losses, standard deviation of sales, standard deviation of cash flows, and sales growth.[9] Due to the absence of theory, we make no predictions as to what the signs of the coefficients on the control variables should be. In addition, in model (3) we also include the level of accruals as an independent variable to control for the finding from prior audit research that accrual levels differ across auditor clienteles (Becker et al. 1998; Francis et al. 1999). Therefore, regression model (3) examines whether auditors have an effect on the comparability of earnings that is incremental to their effect on accruals quality. We also include industry fixed effects at the two-digit SIC industry classification level as a further control for innate firm characteristics and potential omitted variables. Test variables and control variables are defined in Appendix A.

Since the dependent variable is calculated each year t for a pair of firms i and j, the control variables must also control for yearly characteristics of the firm-pair i and j. Following prior research that has used pairs of firms, we control for both the *levels* and *differences* in firm-pair characteristics (Francis, Huang, and Khurana 2009; De Franco et al. 2011). For the regressions that have $Diff\_Total\_Accruals_{ijt}$ and $Abn\_Accr\_Diff_{ijt}$ as the dependent variable we control for *levels* by entering the minimum value in each year t for the paired control variables for firm i and j.[10] The *differences* are measured by the absolute values of yearly differences in the control variable values for firm i and firm j. For the regressions that have *ECOMP_COV* as the dependent variable we follow the same approach. However, for these regressions, the dependent variable is constructed from the correlation of earnings across 16 consecutive quarters for firm-pairs. We therefore estimate the average of each control variable for each firm i and each firm j across the corresponding 16 quarters. We use averages of each firm to construct the average minimum value, and differences in these averaged values are used to construct the difference metric.

[9] In regression (3) using *Diff_Total_Accruals* as the dependent variable, we include cash flow from operations to control for cash flow fundamentals. For the regression in Equation (4) using *ECOMP_COV* as the dependent variable, we included the variable *CFO_COMP_COV* to control for cash flow fundamentals.

[10] As an alternative control, we use the average value in each year t for the paired control variables for firm i and firm j. This shows similar results to entering the minimum of each firm pair and all conclusions remain the same.

IV. SAMPLE SELECTION AND DATA

Sample Construction

We begin with all non-missing observations for Compustat firms incorporated in the U.S. with data from 1987 through 2011. We use this period because we require "Cash Flows from Operations" as a control variable and also to measure total accruals (earnings minus cash flows), and these data became available in 1987. Following De Franco et al. (2011) we retain firms with fiscal year ends in March, June, September, and December. We retain only observations with at least 20 firms in a given two-digit industry, and delete firms with names containing "HOLDING," "HOLDINGS," "ADR," "partnership," "LP," "LLP." We also delete all firms that report negative total assets or total assets less than $10 million, and firm-year observations in a year in which the firm switches its auditor. Further, the sample is constrained by the availability of "Cash Flows from Operations" as noted above. Finally, we winsorize all continuous variables at 1 percent and 99 percent.

We begin with the accruals-difference sample, in which all firms in an industry-year are exhaustively paired. For example, if there are three firms A, B, and C, then the firm-pairs would be A-B, B-C, and A-C. The earnings co-movement tests are based on a subsample of the accruals-differences sample. Specifically, we keep only those firms from the accruals-difference sample that have data for all variables for 16 consecutive quarters and that did not change auditors during the 16 quarter period. A firm-pair first enters the sample when 16 consecutive quarters of data first becomes available. To alleviate any concern in regard to the robustness of the t-statistics, we use firm-pair observations with non-overlapping four-year periods to mitigate concerns over non-independence of error terms.[11]

During the sample period there were several audit firm mergers. For all our tests we only compare pairs of firms audited by the same auditor.[12] This has no effect on the accruals tests that are based on yearly cross-sectional data. However, it does result in a reduced sample for the *ECOMP_COV* analysis because this metric requires firm *i* to have the same exact audit firm across a four-year (16-quarter) period.

Descriptive Statistics

Table 1, Panel A reports descriptive statistics for all variables in the study. The test variable *Same_Big4* is coded 1 for 22.2 percent of the sample. For the accrual-difference metrics, the mean difference in total accruals (abnormal accruals) between firm-pairs is 11.3 (10.6) percent of total assets. The mean value of *ECOMP_COV* is an adjusted R^2 of 11.4 percent, similar to the 11.2 percent reported by De Franco et al. (2011). Panel B reports the correlation between *ECOMP_COV* and the accruals-difference metrics and shows a statistically negative association as would be predicted because larger accrual differences imply lower earnings comparability, although the magnitude of the correlations is only $r = 0.05$. The low correlation reflects: (1) the fact that accrual differences are measured after removing variation in earnings due to cash flows, while *ECOMP_COV* includes variation due to cash flows; (2) the accrual metric is a yearly cross-

[11] For example, if we estimate $ECOMP\_COV_{ij}$ for the firm-pair *i* and *j* using 16 consecutive quarterly observations from 1988 to 1991, then the next estimated observation of $ECOMP\_COV_{ij}$ for the firm-pair *i* and *j* that we would include in the sample is based on the 16 consecutive quarterly observations from 1992 to 1995.

[12] Touche Ross merged with Deloitte Haskins and Sells on December 4, 1989 to form Deloitte Touche (later renamed Deloitte); Coopers & Lybrand merged with Price Waterhouse on July 1, 1998 to form PricewaterhouseCoopers, and Arthur Young merged with Ernst & Whinney on October 1, 1989 to form Ernst & Young.

TABLE 1

Descriptive Statistics

Panel A: Summary Statistics of Variables

Variable	Min.	10%	Mean	Median	90%	Max.	STD
Dependent Variables							
Diff_Total_Accruals	0.000	0.013	0.113	0.077	0.270	0.974	0.113
Abn_Accr_Diff	0.000	0.013	0.106	0.075	0.249	0.848	0.101
ECOMP_COV	0.000	0.002	0.114	0.052	0.321	0.998	0.152
Test Variables							
Both_Big4	0.000	1.000	0.960	1.000	1.000	1.000	0.201
Same_Big4	0.000	0.000	0.222	0.000	1.000	1.000	0.416
Control Variables							
TA_Min	−0.641	−0.277	−0.127	−0.097	−0.023	0.356	0.116
Abn_Accr_Min	−0.463	−0.178	−0.051	−0.028	0.041	0.386	0.098
TA_Avg	−0.641	−0.177	−0.071	−0.061	0.020	0.356	0.086
Abn_Accr_Avg	−0.463	−0.087	0.002	0.006	0.085	0.386	0.073
Size_Diff	0.000	0.300	1.987	1.653	4.174	9.007	1.524
Size_Min	2.394	2.935	4.697	4.478	6.736	11.596	1.469
Size_Avg	2.394	3.883	5.691	5.574	7.652	11.596	1.451
LEV_Diff	0.000	0.009	0.216	0.160	0.489	1.316	0.214
LEV_Min	0.000	0.000	0.098	0.031	0.290	1.316	0.134
LEV_Avg	0.206	0.432	0.169	0.013	0.173	0.000	1.316
MB_Diff	0.000	0.151	1.696	0.967	4.171	18.446	2.091
MB_Min	−2.982	0.260	1.104	0.901	2.171	17.634	0.998
MB_Avg	−2.982	0.611	1.952	1.531	3.837	17.634	1.511
CFO_Diff	0.000	0.019	0.167	0.112	0.391	1.235	0.168
CFO_Min	−0.793	−0.295	−0.036	0.022	0.126	0.500	0.187
CFO_Avg	−0.793	−0.123	0.047	0.071	0.181	0.500	0.133
LossProb_Diff	0.000	0.000	0.335	0.250	0.813	1.000	0.296
LossProb_Min	0.000	0.000	0.167	0.063	0.563	1.000	0.245
LossProb_Avg	0.000	0.031	0.335	0.281	0.719	1.000	0.260
STD_Sales_Diff	0.000	1.358	95.146	18.756	243.841	2450.380	225.808
STD_Sales_Min	0.091	0.739	14.288	4.012	29.271	2450.550	46.549
STD_Sales_Avg	0.091	2.688	61.861	17.649	154.765	2450.550	130.283
STD_CFO_Diff	0.084	0.815	8.967	3.017	17.666	1253.810	26.996
STD_CFO_Min	0.000	0.858	54.564	10.356	140.579	1253.160	125.845
STD_CFO_Avg	0.095	2.079	36.249	10.447	90.407	1253.810	73.284
STD_Sales_Grth_Diff	0.000	0.017	0.788	0.115	1.262	2343.540	11.018
STD_Sales_Grth_Min	0.021	0.047	0.163	0.103	0.248	1115.240	0.975
STD_Sales_Grth_Avg	0.021	0.082	0.557	0.179	0.903	1203.550	5.700
Ret_Diff	0.000	0.076	0.683	0.436	1.493	8.084	0.843
Ret_Min	−0.955	−0.636	−0.133	−0.161	0.355	7.316	0.441
Ret_Avg	−0.955	−0.374	0.209	0.092	0.870	7.316	0.624

(continued on next page)

TABLE 1 (continued)

Panel B: Spearman Correlation Coefficients between Comparability Metrics

		I	II	III
I	*ECOMP_COV*	1.000		
II	*Diff_Total_Accruals*	−0.053	1.000	
		< 0.0001		
III	*Abn_Accr_Diff*	−0.049	−0.701	1.000
		< 0.0001	< 0.0001	

Panel A reports descriptive statistics for all 2,471,917 firm-pairs in the study for the sample period 1987–2011. Panel B reports correlations between the comparability metrics for firm-pairs in the study for the period 1987–2011. All variables are defined in the Appendix A.

sectional measure while *ECOMP_COV* measures comparability across time; and (3) it is well documented there is noise in any earnings-attribute metric.

V. ACCRUAL DIFFERENCES

Primary Results

Table 2 reports the estimation of firm-pair differences in total and abnormal accruals. In the test of H1, the coefficient on *Same_Big4* is negative and statistically significant at $p < 0.01$ (two-tailed) for both differences in total accruals and differences in abnormal accruals. This is consistent with greater similarity in the accruals structure for firm-pairs audited by the same Big 4 auditor, and supports H1 regarding the effect of audit firm style on accounting comparability. The signs of the coefficients on the control variables are generally as expected. *Accruals_Min*, *Size_Min*, *CFO_Min* all have negative coefficients. As the minimum level of accruals, firm size, and cash flows increases in magnitude, firm-pairs are likely be more similar and therefore are more likely to have a similar accruals structure and smaller differences in accruals.

Auditor Changes

The next analysis is limited to subsamples where there is an auditor change for the firm-pair. We begin with the notion that if a pair of firms has different auditors and one of the firms changes auditor to have the same auditor as the other firm, then after the switch we should observe a more similar accruals structure resulting in smaller differences because the two firms are now subject to the style effects of the same auditor. To test this expectation, we re-estimate the regression model in Equation (3) for a subsample of firm-pairs that have switched from having two different Big 4 auditors (pre-switch) to having the same Big 4 auditor post-switch:

$$Diff\_Total\_Accruals_{ijt}(Abn\_Accr\_Diff_{ijt}) = \alpha_{0ij} + \alpha_1 S\_Switch_{jt} + \alpha_2 Controls + \varepsilon_{ijt}, \quad (5)$$

where *S_Switch* is an indicator variable that takes the value of 1 in the test years following the switch (same auditor), and the value of 0 in the benchmark years prior to switch (different auditors). Therefore the indicator variable *S_Switch* compares the differences in accruals for the same pair of firms, before and after the switch. We predict a negative coefficient if switching to the same auditor decreases the accrual differences for a pair of firms. The dependent variable and control variables are the same as previously described. We estimate this regression across three alternative periods of increasing length before and after the switch.

TABLE 2

OLS Results for Accruals Comparability Tests

	Y = Diff. Signed Total Accruals			Y = Diff. Signed Abnormal Accruals		
Variable	**Coeff.**	**t-stat.**	**p-value**	**Coeff.**	**t-stat.**	**p-value**
Intercept	0.073	17.76	0.000***	0.104	18.99	0.000***
Same_Big4	−0.001	−4.68	0.000***	−0.001	−3.26	0.001***
Accruals_Min	−0.723	−253.14	0.000***	−0.713	−303.72	0.000***
Size_Diff	−0.004	−20.76	0.000***	−0.002	−14.95	0.000***
Size_Min	−0.007	−21.26	0.000***	−0.004	−15.90	0.000***
LEV_Diff	0.000	0.01	0.990	0.003	2.78	0.006***
LEV_Min	−0.013	−6.45	0.000***	−0.002	−1.46	0.144
MB_Diff	0.001	9.11	0.000***	0.000	2.38	0.018**
MB_Min	0.003	8.25	0.000***	0.000	−0.07	0.944
CFO_Diff	−0.060	−15.71	0.000***	−0.033	−11.95	0.000***
CFO_Min	−0.181	−31.57	0.000***	−0.184	−40.20	0.000***
LossProb_Diff	−0.033	−28.83	0.000***	−0.022	−23.28	0.000***
LossProb_Min	−0.067	−28.82	0.000***	−0.027	−14.10	0.000***
STD_Sales_Diff	0.000	4.06	0.000***	0.000	2.34	0.019**
STD_Sales_Min	0.000	0.36	0.720	0.000	−0.60	0.547
STD_CFO_Diff	0.000	3.66	0.000***	0.000	3.10	0.002***
STD_CFO_Min	0.000	3.33	0.001***	0.000	4.03	0.000***
STD_Sales_Grth_Diff	0.000	3.30	0.001***	0.000	−0.74	0.457
STD_Sales_Grth_Min	0.000	1.38	0.167	0.000	1.09	0.278
Industry FE	Yes			Yes		
R^2	0.560			0.537		
n	2,471,917			2,471,917		

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
All p-values are based on robust standard errors clustered at the firm level. There are 6,044 unique firms/clusters for the t-tests.
Table 2 reports an OLS regression that examines the impact of having the same Big 4 auditor on the pairwise financial statement comparability between firms, where comparability is measured based on differences in accruals between firm *i* and firm *j*. The dependent variables are the absolute value of the differences between firm *i* and firm *j* in total accruals, *Diff_Total_Accruals*, and differences in abnormal accruals, *Abn_Accr_Diff*. Total accruals are calculated as the difference between income before extraordinary items and cash flows from operations adjusted for cash flows from extraordinary items, all scaled by beginning of year total assets. Abnormal accruals are calculated using the Jones (1991) model of abnormal accruals as modified by Kothari et al. (2005). The test variable *Same_Big4* is coded 1 if both auditors in a pair of firms are the same Big 4 firm, and 0 if auditors in a pair are two different Big 4 auditors.
Control variables are defined in Appendix A.

We begin by comparing accruals differences two years before (*t*−2 and *t*−1), and three years after the switch *t*0, *t*+1, and *t*+2, where the first year of the new auditor is denoted *t*0.[13] We then expand this to three years prior to the switch (*t*−3 through *t*−1) and four years after the switch (*t*0 through *t*+3), and finally to four years before and five years after the switch, *t*−4 to *t*+4. As reported in Panel A, Table 3, the results across all sub-periods examined show that the predicted coefficient on *S_Switch* is negative as expected, and statistically significant at $p < 0.01$, except for

[13] We allow a three-year transitional period for the new auditor to imprint their style on the grounds that it will take more than one year to do so. However, in untabulated tests we find the same result if we compare just the last year before the switch (two different audit firms) and the first year after the switch (the same audit firm).

TABLE 3

Accruals Comparability for Subsample of Auditor Switches

Panel A: Pair of Firms Switch to Have the Same Big 4 Auditor

	Variable	Coeff.	t-stat.	n
$t-2,t+2$				
$Y = Diff\_Total\_Accruals$	*S_Switch*	−0.003	−2.93***	66,405
$Y = Abn\_Accr\_Diff$	*S_Switch*	−0.003	−3.39***	66,405
$t-3,t+3$				
$Y = Diff\_Total\_Accruals$	*S_Switch*	−0.005	−4.86***	49,630
$Y = Abn\_Accr\_Diff$	*S_Switch*	−0.004	−3.72***	49,630
$t-4,t+4$				
$Y = Diff\_Total\_Accruals$	*S_Switch*	−0.003	−2.59***	35,542
$Y = Abn\_Accr\_Diff$	*S_Switch*	−0.002	−1.93*	35,542

Panel B: Pair of Firms Switch to Different Big 4 Auditors

	Variable	Coeff.	t-stat.	n
$t-2,t+2$				
$Y = Diff\_Total\_Accruals$	*D_Switch*	−0.001	−0.82	47,571
$Y = Abn\_Accr\_Diff$	*D_Switch*	0.000	0.07	47,571
$t-3,t+3$				
$Y = Diff\_Total\_Accruals$	*D_Switch*	−0.002	−1.45	33,026
$Y = Abn\_Accr\_Diff$	*D_Switch*	−0.001	−0.41	33,026
$t-4,t+4$				
$Y = Diff\_Total\_Accruals$	*D_Switch*	0.002	1.14	20,925
$Y = Abn\_Accr\_Diff$	*D_Switch*	0.001	0.86	20,925

(continued on next page)

Abn_Accr_Diff in the period *t*–4 to *t*+4, which is significant at $p < 0.10$. These results for years around auditor changes provide compelling evidence that auditor style has an effect on clientele accruals, and that a change to the same auditor leads to more similar accruals.

For completeness, we also examine the situation where firm-pairs have the same auditor, and one of them changes auditors so that the firm pair now has two different auditors. We estimate the same regression as in Equation (5) except we include an indicator variable *D_Switch* that takes the value of 1 in the test years following the switch to different auditors, and the value of 0 in the benchmark years prior to switch when the pair of companies had the same auditor. The results are reported in Table 3, Panel B. Across all test periods, the coefficient on *D_Switch* is not statistically significant from 0. Therefore, there is no evidence that a change to different auditors reduced comparability in the post-switch period. We have no explanation for why the evidence from the two sets of switching tests in Table 3 is not symmetrical.

The literature on auditor switches has proposed a number of reasons for firms switching auditors. One possibility is that the motive for an auditor change could give rise to an omitted variable that biases the accrual-difference test. However, this bias should affect auditor changes in both directions, those that give rise to a firm pair with the same auditor (*S_Switch*) and those auditor changes that give rise to a firm pair with the different auditors (*D_Switch*). Therefore, our first approach to controlling for this possible bias is to predict that an *S_Switch* decreases accrual-differences by a greater amount than a *D_Switch*. By directly comparing the relative accrual-

TABLE 3 (continued)

Panel C: Statistical Tests For the Difference between *S_Switch* and *D_Switch* Coefficients

	Difference	t-stat.	p-value
$t-2,t+2$			
$Y = Diff\_Total\_Accruals$	−0.002	1.07	0.285
$Y = Abn\_Accr\_Diff$	−0.003	2.16	0.031
$t-3,t+3$			
$Y = Diff\_Total\_Accruals$	−0.003	1.99	0.046
$Y = Abn\_Accr\_Diff$	−0.003	2.12	0.034
$t-4,t+4$			
$Y = Diff\_Total\_Accruals$	−0.005	2.48	0.013
$Y = Abn\_Accr\_Diff$	−0.003	2.00	0.046

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
All p-values are based on robust standard errors clustered at the firm level.
Panel A reports an OLS regression for a sample of firm-pairs for which one firm has changed auditors resulting in the firm-pair switching from having different Big 4 auditors to having the same Big 4 auditor. The test variable *S_Switch* is coded 1 in the years after the switch when both auditors in a pair of firms have the same Big 4 auditor, and 0 for the years prior to the switch when auditors in a pair are two different Big 4 auditors. Therefore *Switch* estimates the change in accrual-differences between a pair of firms before and after the switch from having different to having the same auditor. Panel B reports an OLS regression for a sample of firm-pairs that have switched from having the same Big 4 auditor to having different Big 4 auditors. The test variable *D_Switch* is coded 1 in the years after the switch when both auditors in a pair of firms have different Big 4 auditors, and 0 for the years prior to the switch when auditors in a pair are the same Big 4 auditors.
The dependent variables are differences in total accruals, *Diff_Total_Accruals*, and differences in abnormal accruals, *Abn_Accr_Diff*. For brevity, control variables are not reported.

differences of an *S_Switch* versus a *D_Switch*, we control for any systematic bias associated with the act of auditor switching, *per se*. We find across all the three test periods that *S_Switch* results in smaller accrual-differences than occurs in a *D_Switch*. These results are reported in Table 3, Panel C, which is at least suggestive that switching to the same auditor leads to relatively more comparability than switching to different auditors.

One of the often-cited reasons for auditor switches is opportunistic opinion shopping, although academic research has found little or no evidence of this (Chow and Rice 1982; Smith 1986; Johnson and Lys 1990; Francis and Wilson 1988; DeFond 1992; Lennox 2000). To control for the potential effect of opinion shopping we remove firms from our sample that had a qualified audit opinion in the year prior to the switch and re-estimate the auditor switch tests. The results show across all sub-periods examined that the coefficient on *S_Switch* is negative as predicted, and statistically significant. There is also evidence that auditor changes are associated with increased litigation risk and financial distress, which could bias the tests in Table 3 (DeFond and Subramanyam 1998; Shu 2000). DeFond and Subramanyam (1998) proxy for litigation risk and distress by using a variable for audit report qualifications in the prior year, which we control for in the models. As an alternative approach to jointly proxy for poor financial performance and litigation risk, we eliminate firms with extreme values of earnings (DeFond and Subramanyam 1998).[14] The

[14] Extreme values are controlled by including an indicator variable coded 1 if firm-year observations are in the upper (lower) 5 percent of the distribution of return on assets.

results for this subsample remain unchanged from the full sample; specifically, across all sub-periods examined, the coefficient on *S_Switch* is negative as predicted, and statistically significant, and *D_Switch* remains insignificant.[15]

Other Robustness Tests

The first robustness test is based on the notion that auditors can impose comparability on the accruals component of earnings, but not the cash flow component. Therefore, we re-estimate the regression model in Equation (3) using firm-pair differences in cash flows from operations as the dependent variable, and expect the auditor test variable to be insignificant in this analysis. Untabulated results confirm that the coefficient on *Same_Big4* is not statistically different from zero at the $p = 0.10$ level. This provides additional evidence that audit style affects earnings comparability only through the accruals component of earnings, which is more subjective and discretionary than the cash flow component.

We have argued that auditors impose comparability through their in-house policies for the implementation of GAAS and GAAP. These policies are more likely to exist for routine or typical transactions than for non-routine transitory transactions that, by definition, are more idiosyncratic in nature. Thus, there is little advantage in having standardized in-house rules for atypical, non-routine transactions. To test this we create a subsample of firm-pairs that report special items, and compute the absolute value of differences in the signed value of reported special items for firm-pairs *i* and *j*. This variable is more likely to reflect non-routine transitory transactions and therefore is less likely to be affected by audit style. Consistent with our expectation, when we estimate our baseline regression (3) with special items as the dependent variable, the coefficient on *Same_Big4* is not statistically significant at the $p = 0.10$ level.

Next, we address the concern that the control variables may not effectively control for differences in economic fundamentals, particularly when differences in economic fundamentals for firm *i* and firm *j* are large. Therefore, we remove all firm-pairs from the sample where the difference in sales revenue between firm *i* and firm *j* is greater than 20 percent. Untabulated results show that the coefficient on *Same_Big4* is negative and statistically significant at the $p < 0.01$ level for differences in total accruals and differences in abnormal accruals, which indicates that size differences in firm-pairs do not affect the results.

VI. EARNINGS COVARIATION

The test of H1 using earnings covariation (*ECOMP_COV*) is reported in Table 4, using the model in Equation (4). The coefficient on the test variable *Same_Big4* is positive and statistically significant at the $p < 0.01$ level, consistent with the earnings of a pair of firms audited by the same Big 4 auditor being more comparable over time than the earnings of a pair of firms audited by two different Big 4 auditors.

Signs of the coefficients on the control variables in Table 4 are largely as expected. The positive coefficient on *CFO_COMP_COV* indicates that as cash flow from operations for two firms becomes more highly correlated, so do their earnings. Similarly, firm-pairs with more similar stock return covariation (*RET_COV*) have more correlated earnings, which is consistent with the mapping of earnings to stock returns. The negative coefficients on *Size_Diff* and *LossProb_Diff* are consistent with less earnings covariation when there is a greater difference in the size of two firms or in probability of reporting a loss for the two firms. Finally, the negative coefficient on

[15] Another reason proposed for auditor switches is to align cross-temporal changes in client characteristics and differences in audit firm cost structure, resulting in growing firms switching to larger auditors (Johnson and Lys 1990). This motivation for switching does not apply to our sample, which is restricted to Big 4 auditors.

TABLE 4

OLS Results for *ECOMP_COV* Comparability Metric

Variable	Coeff.	t-stat.	p-value
Intercept	0.094	20.27	0.000***
Same_Big4	0.002	3.34	0.001***
Size_Diff	0.000	−1.18	0.238
Size_Min	0.002	5.46	0.000***
LEV_Diff	−0.005	−2.59	0.010***
LEV_Min	0.007	1.80	0.072*
MB_Diff	0.003	6.11	0.000***
MB_Min	0.000	2.23	0.026**
LossProb_Diff	−0.013	−11.27	0.000***
LossProb_Min	−0.003	−2.31	0.021**
STD_Sales_Diff	0.000	3.80	0.000***
STD_Sales_Min	0.000	2.72	0.007***
STD_CFO_Diff	0.000	−3.59	0.000***
STD_CFO_Min	0.000	−4.13	0.000***
STD_Sales_Grth_Diff	−0.001	−4.55	0.000***
STD_Sales_Grth_Min	−0.016	−8.06	0.000***
CFO_COMP_COV	0.084	22.75	0.000***
RET_COV	0.042	15.72	0.000***
Industry Fixed Effects	Yes		
R^2	0.014		
n	676,952		

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
All p-values are based on robust standard errors clustered at the firm level. There are 5,078 unique firms/clusters for the t-tests.
Table 4 reports an OLS regression in which the dependent variable *ECOMP_COV* is a pairwise measure of financial statement comparability based on earnings co-movement between firm *i* and firm *j* across 16 quarters. The test variable is *Same_Big4* that is coded 1 if both auditors in a pair of firms are the same Big 4 firm, and 0 if auditors in a pair are two different Big 4 auditors.
Control variables are defined in Appendix A.

STD_CFO_Min is consistent with a greater variation in cash flows leading to less earnings comparability.

VII. AUDITOR FIXED EFFECTS

An alternative approach to test for the effect of audit style on comparability is to determine if auditor fixed effects explain the level of accruals reported by each individual firm *i*. Rather than examining firm-pairs, this approach uses the level of accruals reported by each individual firm *i* as the dependent variable, and examines whether auditor fixed effects explain significant cross-sectional variation in accruals using the following OLS regression:

$$Total\_Accruals_{it} (\text{or } Abnormal\_Accruals_{it}) = \alpha_{0i} + \gamma + \alpha_1 Controls_{it} + \varepsilon_{it}, \quad (6)$$

where $Total\_Accruals_{it}$ and $Abnormal\_Accruals_{it}$ of firm *i* in year *t* are as previously defined, and γ is the set of individual auditor fixed effect coefficients. The control variables are firm size, market-to-book, leverage, cash flows from operations, loss, standard deviation of sales, standard deviation of cash flows from operations, and standard deviation of sales growth for each firm *i* and

year *t*. We also include industry fixed effects to control for potential omitted variables bias (Greene 2011). Thus, our estimated auditor fixed effects model captures the incremental effect of each auditor *after controlling for firm-specific variables and industry-wide fixed effects*. We predict that an F-test will reject the null hypothesis that coefficients of the individual auditor fixed effects are the same in the regression model. This approach to testing for audit style is conceptually similar to the stream of research that examines whether managers have individual styles. The seminal paper by Bertrand and Schoar (2003) finds that manager fixed effects are significant for a wide range of corporate decisions and interpret this result to be consistent with general differences in "style" across managers. Bamber et al. (2010) and Ge et al. (2011) extend this research to the accounting effects of style by CEOs and CFOs.

In untabulated results, the F-statistic testing the equality of auditor fixed effects is 14.67 for a regression with *Total_Accruals* as the dependent variable, and 4.07 for a regression with *Abnormal_Accruals* as the dependent variable, both significant at the $p < 0.01$ level. These results reject the null hypothesis that each auditor affects accruals in the identical manner, providing additional evidence that audit firms have individual styles in their approach to the interpretation and enforcement of GAAP that they impose on clients. A significant individual auditor fixed effect in the presence of an extensive set of controls and industry fixed effects requires that the audit firm exerts a systematic effect on accruals that is above or below the mean relative to other audit firms. In other words, an audit firm has a style that is unique and common across its client base, and that is systematically different from that of one or more other auditors.[16]

We extend this approach by testing individual line items in the financial statements. We focus on reported line items where there is some likelihood of differences in reported amounts due to inherent judgment that is where auditor style effects would be most expected. The accounts we examine are: inventory, capitalized leases, and pension expense. We first examine inventory because it is well documented in the audit literature that inventory is an asset subject to inherent risk that requires specific audit procedures. Consistent with this, a large volume of audit fee literature consistently finds inventory is positively related to audit fees (Hay, Knechel, and Wong 2006). Accordingly, we predict any style differences between auditors are likely to be evident in this account. In regard to capitalized leases, Acito et al. (2009) report that beginning in late 2004 through mid-2006, more than 250 U.S. firms disclosed that the operating lease accounting methods they had been using violated GAAP and that the violations were similar in nature. This suggests that there may be systematically different interpretations in the application of the rules or the materiality thresholds in applying the lease accounting standards (FASB 1976, 1988). We also examine the expected rate of return for pension assets as there is substantial flexibility in deciding the assumption that, in turn, can materially affect pension expense (Comprix and Muller 2006; Picconi 2006; Ge et al. 2011).

In untabulated results, the F-statistics for auditor fixed effects are 8.35, 10.16, and 44.28 for regressions with *inventory* (cost of goods sold/average inventory), *lease capitalization* (capitalized leases/total assets), and *pension expense* (pension interest rate assumption), respectively. These F-tests are all significant at the $p < 0.01$ level, so we reject the null hypothesis that auditors are associated with the accounting for inventory, leases, and pension expense in exactly the same manner. This analysis provides additional evidence that audit firms have individual styles in their approach to the interpretation and enforcement of GAAP in clients' financial reports, and reinforces

[16] The F-statistic only shows that at least one firm is different from the other auditors and not that each auditor is different from all other auditors. To provide more robust evidence, we conduct a Wald test of equality of coefficients between alternate combinations of two auditors (i.e., PwC versus KPMG; Ernst & Young versus Deloitte, and so on). There are 21 unique combinations of auditor pairs of which 18 are statistically different from each other, which indicates there are style differences between nearly all of the Big N auditor pairs.

the findings in Table 2 and Table 3 concerning the similarity of accruals for firm-pairs in the same industry-year.

In summary, the results from accruals differences, earnings covariation, and auditor fixed effects are all consistent with the prediction in H1 that audit style affects accounting comparability. Specifically, firm-pairs in the same industry-year with the same Big 4 auditor have greater cross-sectional similarity in accruals and greater earnings covariation over time, and the auditor fixed effects tests show that there are systematic clientele differences in accruals and financial statement line items.[17]

Self-Selection

Firms are not randomly assigned to an auditor, rather, they choose one. To the extent that the reasons for choosing an auditor are not randomly distributed across firms, but rather are correlated across firm-pairs, self-selection could have an implication for the interpretation of the results. However, the test of auditor changes in Table 3 provides compelling evidence that audit style is the causal mechanism we observe in the data. In addition, the use of industry fixed effects is a standard econometric procedure to control for omitted variables that is a source of selection bias (Greene 2011).

There are other reasons to believe self-selection is not a significant issue in this study. First, the vast bulk of the theory and the empirical evidence associated with auditor choice has been associated with the choice between a Big N and a non-Big N auditor. This self-selection decision is not relevant to our primary H1 that examines comparability within the population of Big N auditors. Second, to the extent self-selection is driven by the choice of an auditor because they have a specific audit methodology and/or interpretation of accounting standards that suits the firm, this selection-motivation is consistent with H1.

Notwithstanding these observations, we conduct some tests to alleviate self-selection concerns. We begin by considering motives for a firm choosing an auditor that could be associated with a firm's accounting production function. Some papers have documented evidence consistent with auditors being industry specialists. It is therefore possible that two firms with similar accounting production functions could choose the same auditor because they have a similar demand for an industry specialist. To guard against this we remove from the sample firm-pairs that had an auditor that meets the typical definition of an auditor specialist in the literature.[18] For this reduced sample, the coefficient on *Same_Big4* for accrual-differences (earnings covariation) metric remains negative (positive) and statistically significant at the 0.01 level. Another possibility is that due to proprietary information, two competitors in the same industry do not want to share the same auditor. To the extent competitors share a similar accounting production function, this type of self-selection creates a bias against finding a result as similar firms are selecting different auditors. Nevertheless, we address this self-selection issue by following prior literature and use measures of industry competition based on the Herfindahl index to proxy for proprietary costs of disclosure (e.g., Harris 1998; Botosan and Stanford 2005; Berger and Hann 2007). We calculate the Herfindahl index using sales by two-digit industry-year and assign an index value to a firm-pair per industry-year. To control for the similarity that might be induced by close competition, we remove yearly firm-pairs

[17] In an untabulated analysis we also looked at the closeness of the *C_SCORE* metric for firm-pairs (Khan and Watts 2009). Consistent with the above results, we find that *C_SCORE*s are closer in value for firm-pairs with the same Big 4 auditor than for firm-pairs with two different auditors, which is evidence that auditors have a systematic style with respect to the level of accounting conservatism they impose on their clientele.

[18] Consistent with the literature, an industry specialist is defined as an auditor that audits more than 30 percent of the total assets of an industry or, as an alternate definition, an industry specialist is an auditor that audits the most sales in an industry.

in the highest decile of the Herfindahl index and re-estimate the regression models. The results in Table 2 and Table 4 are unchanged. The coefficient on *Same_Big4* for the accrual-difference (earnings covariation) metric remains negative (positive) and statistically significant at the 0.01 level.

VIII. BIG 4 VERSUS NON-BIG 4 AUDITORS

This section reports the test of H2, which predicts greater earnings comparability for firm-pairs with the same Big 4 auditor than firm-pairs with the same non-Big 4 auditor. As a starting point we examine if there are non-Big 4 auditor style effects on the comparability of earnings. We re-estimate the model in Equation (3) for a subsample of firm-pairs with non-Big 4 auditors, and create an indicator variable *Same_NonBig4* that is coded 1 if two firms are both audited by the same non-Big 4 auditor, and 0 if they are audited by two different non-Big 4 auditors. The results are reported in Table 5, Panel A. For parsimony we do not report the control variables.

The results show a negative and statistically significant coefficient on the variable *Same_NonBig4* for total accruals but not abnormal accruals. The sign for the earning co-movement metric (*ECOMP_COV*) is also significant but in the opposite direction to that predicted for greater comparability. Therefore, there is no consistent evidence that two companies audited by the same non-Big 4 auditor have more comparable earnings than a pair of companies audited by two different non-Big 4 auditors. We also present the results just for a subsample of firm-pairs audited by mid-tier auditors. We examine mid-tier auditors as it is possible they have a greater investment in in-house programs than other smaller non-Big 4 auditors. The variable *Same_MidTier* is coded 1 if two firms are both audited by the same mid-tier auditor, and 0 if they are audited by two different mid-tier auditors, where the mid-tier auditors are BDO Seidman, Grant Thornton, and McGladrey and Pullen. The results reported in Table 5, Panel B show a negative and statistically significant coefficient on the variable *Same_MidTier* for total accruals (abnormal accruals are weakly significant at the 10 percent level, one-tailed), and earnings covariation is not significant. Overall we conclude there is only weak and inconsistent evidence that having the same non-Big 4 auditor affects the comparability of earnings.

We now turn to a formal test of H2. We first approach this by constructing a subsample of firm-pairs that either have the same Big 4 auditor or the same non-Big 4 auditor. We then re-estimate the model in Equation (3) with the indicator variable *Same_Big4* that is coded 1 if two firms are both audited by the same Big 4 auditor, and 0 if they are audited by the same non-Big 4 auditor. The results reported in Table 5, Panel C show a negative and statistically significant coefficient on the variable *Same_Big4* for total accruals, but a statistically insignificant coefficient for abnormal accruals. For *ECOMP_COV* the coefficient is positive (as predicted) and weakly statistically significant at the 10 percent level (one-tailed). Therefore, there is some evidence consistent with Big 4 auditors having a greater effect on earnings comparability than non-Big 4 auditors. Clients of Big 4 and non-Big 4 auditors can differ in economic fundamentals for which an OLS regression may not adequately control. To address this, we examine a subsample of firm-pairs in which the size of each firm must be between the smallest firm audited by a Big 4 auditor and the largest firm audited by a non-Big 4 auditor. The tests in Table 5, Panel D show a negative and statistically significant coefficient on the variable *Same_Big4* for total accruals, and marginally statistically significant coefficients for abnormal accruals and earnings covariation (10 percent level, one-tailed). In summary, there is support for H2 that Big 4 auditors have greater accounting comparability within their clienteles than occurs for non-Big 4 auditors.

TABLE 5

Impact of Big 4 versus Non-Big 4 Auditor on the Comparability of Earnings

Panel A: Same Non-Big 4 versus Different Non-Big 4

	Dep. Var. = *Diff_Total_Accruals*		Dep. Var. = *Abn_Accr_Diff*		Dep. Var. = *ECOMP_COV*	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Same_NonBig4	−0.004	−2.28**	−0.001	−1.05	−0.004	−1.94*
n	28,615					

Panel B: Same Mid-Tier versus Different Mid-Tier

	Dep. Var. = *Diff_Total_Accruals*		Dep. Var. = *Abn_Accr_Diff*		Dep. Var. = *ECOMP_COV*	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Same_MidTier	−0.003	−2.12**	–0.002	−1.38	0.001	0.87
n	22,809					

Panel C: Same Big 4 versus Same Non-Big 4

	Dep. Var. = *Diff_Total_Accruals*		Dep. Var. = *Abn_Accr_Diff*		Dep. Var. = *ECOMP_COV*	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Same_Big4	−0.003	−1.78*	−0.002	−1.26	0.003	1.32
n	559,516					

Panel D: Same Big 4 versus Same Non-Big 4 for Subsample of Size-Matched Sample

	Dep. Var. = *Diff_Total_Accruals*		Dep. Var. = *Abn_Accr_Diff*		Dep. Var. = *ECOMP_COV*	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Same_Big4	−0.003	−1.83*	−0.002	−1.32	0.003	1.25
n	474,352					

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
All p-values are based on robust standard errors clustered at the firm level.
Panels A and B report an OLS regression that examines the impact of non-Big 4 auditors on comparability. Panels C and D report an OLS regression that examines the difference in impact on comparability of earnings between a Big 4 auditor versus a non-Big 4 auditor. Comparability is measured based on differences in accruals between firm *i* and firm *j*. The dependent variables are the absolute value of the differences between firm *i* and firm *j* in total accruals, *Diff_Total_Accruals*, and differences in abnormal accruals, *Abn_Accr_Diff*. The test variable *Same_NonBig4* in Panel A is coded 1 if both auditors in a pair of firms are the same non-Big 4 firm, and 0 if auditors in a pair are two different non-Big 4 auditors. The test variable *Same_MidTier* in Panel B is coded 1 if both auditors in a pair of firms are the same mid-size auditor firm, and 0 if auditors in a pair are two different mid-size auditors. The test variable *Same_Big4* in Panels C and D is coded 1 if both auditors in a pair of firms are the same Big 4 firm, and 0 if auditors in a pair are the same non-Big 4 auditor.
Control variables are not reported for brevity.

IX. ECONOMIC IMPORTANCE

In this section we calculate the economic significance of auditor style effects on the comparability of earnings. In the accruals-difference test reported in Table 2, the coefficient is 0.001 on *Same_Big4* that means that the average effect of auditor style on reported earnings is approximately 0.1 percent of total assets. However, the analysis of auditor changes in Table 3, Panel A, which is arguably a stronger test, suggests a larger magnitude closer to 0.4 percent of assets. The median firm in our study has an operating ROA (operating income scaled by average total assets) of 7.33 percent. Therefore, the auditor style effect due to a 0.4 percent shift in accruals (scaled by assets) will cause a 5.5 percent change in operating ROA for the average firm in the sample (0.4/7.33). There would be a similar percentage effect on firms' profit margins because sales and total assets are nearly the same for the median firm in the sample. The median ratio of sales to assets is 1.005, and the median operating profit margin is 7.2 percent. So a 0.4 percent shift in accruals would cause a 5.6 percent change in operating margin (0.4/7.2). In both ROA and profit margin calculations, a fraction of one percentage point in accruals (scaled by assets) has a nontrivial effect.

In the auditor fixed effect model, when total accruals is the dependent variable, the individual audit firm coefficients range from a low of −0.007 to a high of +0.044. All but one coefficient is significant at the $p < 0.10$ level (two-tailed), with an equal number of positive and negative coefficients. The coefficients are interpretable as percentage effects on total accruals, so the percentage range is from −0.7 to +4.4 percent of accruals, scaled by assets. Pairwise differences in audit firms' coefficients have an average absolute value of 0.026, or 2.6 percent accruals. As demonstrated in the ROA and profit margin analysis above, a percentage point change in accruals, scaled by assets, has a nontrivial effect on financial statements and financial ratios.

In the earnings covariation test in Table 4, the coefficient is 0.002 for *Same_Big4*. This implies that auditor style increases the covariance of earnings, on average, by 2.13 percent from a base of 0.094 (0.002/0.094). The effect is more difficult to interpret because it is a change in R^2, but the percentage magnitude is in line with the above tests.

Because this is the first study to examine auditor style, we have no empirical evidence to inform our priors as to what the magnitudes should be. However, we believe the magnitudes for both the accrual and earnings covariation metrics are plausible and can be categorized as large enough to matter in an economic as well as statistical sense.

X. CONCLUSION

We expect firm-pairs in the same industry-year will have a similar accruals and earnings structure after controlling for firm-specific factors and common exogenous shocks. Our tests show that this is more likely to be the case if the firm-pair is audited by the same Big 4 accounting firm, which is evidence of an audit firm style effect in making accruals/earnings similar within an auditor's clientele. A single set of uniform accounting standards is often advocated as a means to increase comparability of financial statements, reflecting the rationale for the FASB-IASB convergence project. Our study documents that the role of an economic agent, the auditor, is also important in facilitating the production of accounting comparability. We argue that the Big 4 style effect arises from each audit firm having its own unique set of in-house rules with respect to the interpretation and implementation of GAAS (auditing standards) and the interpretation and enforcement of GAAP (accounting standards).

Our results have a number of implications. First, they provide support for Kothari et al. (2010) who conjecture that when standards are principles-based, economic agents such as auditors will develop their own in-house rules that give rise to comparability in the production of financial statements. We find support for the idea that auditors develop in-house rules to facilitate

comparability within their clientele. These auditor style effects also appear to reduce comparability between clients audited by different auditors, although we do not know if these effects are greater than what would occur in the absence of auditor style. We also contribute to the audit literature by showing that Big 4 accounting firms may have an effect on another earnings attribute that has not previously been investigated, namely, accounting comparability. We find that firms audited by Big 4 auditors have greater accounting comparability than firms audited by non-Big 4 auditors, which suggests another dimension in which the two auditor groups differ. It is also the case that each Big 4 audit firm has its own "style," which affects accounting comparability and is therefore another source of variation within the Big 4 group of auditors.

While the purpose of our investigation is not to examine whether the accounting comparability arising from audit style improves earnings quality, our findings suggest this is a useful area for future research. On one hand, the in-house working rules of audit firms are beneficial if they minimize random errors by audit staff and random errors and/or intentional biases by clients, which would improve earnings quality within an auditor's clientele. However, this effect also allows for the possibility of systematic differences between auditor clienteles. Our study reports some suggestive evidence that both effects occur. For example, Table 2 shows that firm-pairs in the same industry-year have smaller differences in abnormal accruals when audited by the same Big 4 auditor, and we know that larger abnormal accruals are associated with the likelihood of client misreporting (Dechow, Sloan, and Sweeney 1996; Dechow, Ge, Larson, and Sloan 2011). We also document in Table 1, Panel B that firm-pairs with smaller accrual-differences have more similar earnings time-series (*ECOMP_COV*), and Table 4 shows that stock returns of firm-pairs are more similar (*RET_COV*) when their earnings time-series are more similar. Together these results suggest that the accounting comparability, as defined in this study, leads to a more similar mapping of earnings to stock returns for firm-pairs and, therefore, is potentially value-relevant. However, an important alternative possibility is that "audit style" imposes consistency and uniformity at the expense of relevance and true comparability. As FASB (2010, para. Q23) cautions, uniformity may make things look the same, but uniformity is not comparability, where the objective is to make "like things" look alike, and "different things" look different. Therefore, a logical next step is to study the implications of auditor style on the quality and informativeness of earnings, and to examine if auditor-induced accounting comparability improves earnings quality or if it leads to uniformity at the expense of more informative accounting.

REFERENCES

Acito, A. A., J. J. Burks, and W. B. Johnson. 2009. Materiality decisions and the correction of accounting errors. *The Accounting Review* 84 (3): 659–688.

Ball, R., A. Robin, and J. S. Wu. 2003. Incentives versus standards: Properties of accounting income in four East Asian countries. *Journal of Accounting and Economics* 36 (1–3): 235–270.

Bamber, L. S., J. Jiang, and I. Y. Wang. 2010. What's my style? The influence of top managers on voluntary corporate financial disclosure. *The Accounting Review* 85 (4): 1131–1162.

Banker, R. D., H. Chang, and Y. C. Kao. 2002. Impact of information technology on public accounting firm productivity. *Journal of Information Systems* 16 (2): 209–222.

Barberis, N., and A. Shleifer. 2003. Style investing. *Journal of Financial Economics* 68 (2): 161–199.

Barth, M. E., W. R. Landsman, M. Lang, and C. Williams. 2012. Are IFRS-based and US GAAP-based accounting amounts comparable? *Journal of Accounting and Economics* 54 (1): 68–93.

Basu, S. 1997. The conservatism principle and the asymmetric timeliness of earnings. *Journal of Accounting and Economics* 24 (1): 3–37.

Becker, C. L., M. L. DeFond, J. Jiambalvo, and K. R. Subramanyam. 1998. The effect of audit quality on earnings management. *Contemporary Accounting Research* 15 (1): 1–24.

Bell, T., F. Marrs, I. Solomon, and H. Thomas. 1997. *Auditing Organizations Through a Strategic-Systems Lens: the KPMG Business Measurement Process*. Montvale, NJ: KPMG.

Berger, P., and R. Hann. 2007. Segment profitability and the proprietary and agency costs of disclosure. *The Accounting Review* 82 (4): 869–906.

Bertrand, M., and A. Schoar. 2003. Managing with style: The effect of managers on firm policies. *The Quarterly Journal of Economics* 118 (4): 1169–1208.

Blacconiere, W., J. Frederickson, M. Johnson, and M. Lewis. 2011. Are voluntary disclosures that disavow the reliability of mandated fair value information informative or opportunistic? *Journal of Accounting and Economics* 52 (2–3): 235–251.

Boone, J., I. K. Khurana, and K. Raman. 2010. Do the Big 4 and the second-tier firms provide audits of similar quality? *Journal of Accounting and Public Policy* 29 (4): 330–52.

Botosan, J., and M. Stanford. 2005. Managers' motives to withhold segment disclosures and the effect of SFAS No. 131 on analysts' information environment. *The Accounting Review* 80: 751–771.

Bradshaw, M. T., and G. S. Miller. 2007. Will harmonizing accounting standards really harmonize accounting? Evidence from non-US firms adopting US GAAP. *Journal of Accounting, Auditing and Finance* 23 (2): 233–263.

Bradshaw, M. T., G. S. Miller, and S. J. Serafeim. 2011. *Accounting Method Heterogeneity and Analysts' Forecasts*. Working paper, Boston College, University of Michigan, and Harvard University.

Brown, L., and A. Pinello. 2007. To what extent does the financial reporting process curb earnings surprise games? *Journal of Accounting Research* 45 (5): 947–981.

Burgstahler, D., and I. Dichev. 1997. Earnings management to avoid earnings decreases and losses. *Journal of Accounting and Economics* 24 (1): 99–126.

Cassell, C., G. Giroux, L. Meyers, and T. Omer. 2013. The emergence of second-tier auditors in the US: Evidence from investor perceptions of financial reporting credibility. *Journal of Business Finance and Accounting* 40 (3-4): 350–372.

Chow, C., and S. Rice. 1982. Qualified audit opinions and auditor switching. *The Accounting Review* 57 (2): 326–335.

Comprix, J., and K. Muller. 2006. Asymmetric treatment of reported pension expense and income amounts in CEO cash compensation calculations. *Journal of Accounting and Economics* 42 (3): 385–416.

Cushing, B., and J. Loebbecke. 1986. *Comparison of Audit Methodologies of Large Accounting Firms*. Sarasota, FL: American Accounting Association.

Dechow, P., R. Sloan, and A. Sweeney. 1996. Causes and consequences of earnings manipulation: An analysis of firms subject to enforcement actions by the SEC. *Contemporary Accounting Research* 13 (1): 1–36.

Dechow, P., W. Ge, C. Larson, and R. Sloan. 2011. Predicting material accounting misstatements. *Contemporary Accounting Research* 28 (1): 17–82.

Dechow, P. M., and I. Dichev. 2002. The quality of accounting and earnings: The role of accrual estimation errors. *The Accounting Review* 77 (Supplement): 35–59.

DeFond, M. L. 1992. The association between changes in client agency costs and auditor switching. *Auditing: A Journal of Practice & Theory* 11:16–31.

DeFond, M., and K. R. Subramanyam. 1998. Auditor changes and discretionary accruals. *Journal of Accounting and Economics* 25 (1): 35–67.

DeFond, M., X. Hu, M. Hung, and S. Li. 2011. The impact of mandatory IFRS adoption on foreign mutual fund ownership: The role of comparability. *Journal of Accounting and Economics* 51 (3): 240–258.

De Franco, G., S. P. Kothari, and R. Verdi. 2011. The benefits of financial statement comparability. *Journal of Accounting Research* 49 (4): 895–931.

Dichev, I. J., J. R. Graham, C. R. Harvey, and S. Rajgopal. 2013. Earnings quality: Evidence from the field. *Journal of Accounting and Economics* 56 (2-3, Supplement 1): 1–33.

Dowling, C. 2009. Appropriate audit support system use: The influence of auditor, audit team, and firm factors. *The Accounting Review* 84 (3): 771–810.

Doyle, J., W. Ge, and S. McVay. 2007. Accruals quality and internal control over financial reporting. *The Accounting Review* 82 (5): 1141–1170.

Ernst & Young (E&Y). 1995. *Financial Reporting Developments: Accounting and Disclosure of Stock-Based Compensation Under SFAS 123*. New York, NY: E&Y.

Financial Accounting Standards Board (FASB). 1976. *Accounting for Leases*. Statement of Financial Accounting Standards No. 13. Norwalk, CT: FASB.

Financial Accounting Standards Board (FASB). 1980. *Qualitative Characteristics of Accounting Information*. Statement of Financial Accounting Concepts No. 2. Norwalk, CT: FASB.

Financial Accounting Standards Board (FASB). 1988. *Accounting for Leases*. Statement of Financial accounting Standards No. 98. Norwalk, CT: FASB.

Financial Accounting Standards Board (FASB). 2010. *Conceptual Framework for Financial Reporting*. Statement of Financial Accounting Concepts No. 8. Norwalk, CT: FASB.

Francis, J., L. E. Maydew, and H. C. Sparks. 1999. The role of Big 6 auditors in the credible reporting of accruals. *Auditing: A Journal of Practice & Theory* 18 (2): 17–34.

Francis, J. 2004. What do we know about audit quality? *The British Accounting Review* 34 (4): 345–368.

Francis, J., S. Huang, and I. Khurana. 2009. Does corporate transparency contribute to efficient resource allocation? *Journal of Accounting Research* 47 (4): 943–989.

Francis, J. 2011. A framework for understanding and researching audit quality. *Auditing: A Journal of Practice & Theory* 30 (2): 125–152.

Francis, J. R., and E. R. Wilson. 1988. Auditor changes: A joint test of theories relating to agency costs and auditor differentiation. *The Accounting Review* 63 (4): 663–682.

Frankel, R., M. Johnson, and K. Nelson. 2002. The relation between auditors' fees for nonaudit services and earnings management. *The Accounting Review* 77 (Supplement): 71–105.

Ge, W., D. Matsumoto, and J. Zhang. 2011. Do CFOs have style? An empirical investigation of the effect of individual CFOs on accounting practices. *Contemporary Accounting Research* 28 (4): 1141–1170.

Greene, W. 2011. *Econometric Analysis*. 7th edition. Englewood Cliffs, NJ: Prentice Hall.

Harris, M. 1998. The association between competition and managers' business segment reporting decisions. *Journal of Accounting Research* 36 (1): 111–128.

Hay, D., W. Knechel, and N. Wong. 2006. Audit fees: A meta-analysis of the effect of supply and demand attributes. *Contemporary Accounting Research* 23 (1): 141–192.

Johnson, V., I. Khurana, and J. K. Reynolds. 2002. Audit-firm tenure and the quality of financial reports. *Contemporary Accounting Research* 19 (4): 637–660.

Johnson, W. B., and T. Lys. 1990. The market for audit services. *Journal of Accounting and Economics* 12 (1-3): 281–308.

Jones, J. J. 1991. Earnings management during import relief investigations. *Journal of Accounting Research* 29 (2): 193–228.

Joos, P., and M. Lang. 1994. The effects of accounting diversity: Evidence from the European Union. *Journal of Accounting Research* 32 (Supplement): 141–168.

Kaplan. S., D. Williams, and K. Menon. 1990. The effects of audit structure on the audit market. *Journal of Accounting and Public Policy* 9 (3): 197–216.

Khan, M., and R. Watts. 2009. Estimation and empirical properties of a firm-year measure of accounting conservatism. *Journal of Accounting and Economics* 48 (2-3): 132–150.

Kinney, W. 1986. Audit technology and preferences for auditing standards. *Journal of Accounting and Economics* 8 (1): 73–89.

Kothari, S., A. Leone, and C. Wasley. 2005. Performance matched discretionary accruals. *Journal of Accounting and Economics* 39 (1): 163–197.

Kothari, S. P., K. Ramanna, and D. Skinner. 2010. Implications for GAAP from an analysis of positive research in accounting. *Journal of Accounting and Economics* 50 (2-3): 246–286.

Krishnan, G. 2005. Did Houston clients of Arthur Andersen recognize publicly available bad news in a timely fashion? *Contemporary Accounting Research* 22 (1): 165–193.

Land, J., and M. Lang. 2002. Empirical evidence on the evolution of international earnings. *The Accounting Review* 77 (Supplement): 115–133.

Lang, M., M. Maffett, and E. Owens. 2010. *Earnings Comovement and Accounting Comparability: the Effects of Mandatory IFRS Adoption*. Working paper, The University of North Carolina, The University of Chicago, and University of Rochester.

Lennox, C. 2000. Do companies engage in successful opinion shopping? *Journal of Accounting and Economics* (29): 321–337.

Leuz, C., D. Nanda, and P. Wysocki. 2003. Earnings management and investor protection: An international comparison. *Journal of Financial Economics* 69 (3): 505–527.

Menon, K., and D. Williams. 2004. Former audit partners and abnormal accruals. *The Accounting Review* 79 (4): 1095–1118.

Petersen, M. A. 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies* 22 (1): 435–480.

Phillips, F., R. Libby, and P. Libby. 2013. *Fundamentals of Financial Accounting*. 4th edition. New York, NY: McGraw-Hill Irwin.

Picconi, M. 2006. The perils of pensions: Does pension accounting lead investors and analysts astray? *The Accounting Review* 81 (4): 925–955.

Reichelt, K., and D. Wang. 2010. National and office-specific measures of auditor industry expertise and effects on audit quality. *Journal of Accounting Research* 48 (3): 647–686.

Revsine, L., D. Collins, W. B. Johnson, and H. F. Mittelstaedt. 2011. *Financial Reporting and Analysis*. 5th edition. New York, NY: McGraw-Hill Irwin.

Reynolds, J. K., and J. Francis. 2000. Does size matter? The influence of large clients on office-level auditor reporting decisions. *Journal of Accounting and Economics* 30 (3): 375–400.

Shu, S. 2000. Auditor resignations: Clientele effects and legal liability. *Journal of Accounting and Economics* 29 (2): 173–205.

Smith, D. B. 1986. Auditor subject to opinions, disclaimers, and auditor changes. *Auditing: A Journal of Practice & Theory* 6 (Fall): 95–108.

Teoh, S., and T. Wong. 1993. Perceived auditor quality and the earnings response coefficient. *The Accounting Review* 68 (2): 346–366.

Vera-Muñoz, S., J. Ho, and C. Chow. 2006. Enhancing knowledge sharing in public accounting firms. *Accounting Horizons* 20 (2): 133–155.

Zerni, M. 2012. Audit partner specialization and audit fees: Some evidence from Sweden. *Contemporary Accounting Research* 29 (1): 312–340.

APPENDIX A

Variable Definitions

Dependent Variables (all measures are in period *t* unless noted otherwise)

Difference in total accruals (*Diff_Total_Accruals*)	Equals the absolute value of the difference between total accruals of firm *i* and total accruals of firm *j* in firm-pair in year *t*. Total accruals are calculated as the difference between income before extraordinary items and cash flows from operations adjusted for cash flows from extraordinary items (IB − (OANCF − XIDOC)), scaled by beginning of year total assets. This variable measures how close total accruals are for two firms.
Difference in signed abnormal accruals (*Abn_Accr_Diff*)	Equals the absolute value of the difference between abnormal accruals of firm *i* and abnormal accruals of firm *j* in firm-pair in year *t*. Abnormal accruals are calculated using Jones (1991) model of discretionary accruals as modified by Kothari et al. (2003). This variable measures how close abnormal accruals are for two firms.
ECOMP_COV	Within-industry earnings co-movement in firm-pair firm *i* and firm *j* across 16 consecutive quarters, calculated as defined in Section III.

Explanatory Variables

Same_Big4	Coded 1 if both auditors in firm-pair firm *i* and firm *j* are the same Big 4 firm, 0 if auditors in firm-pair firm *i* and firm *j* are two different Big 4 auditors.

Control Variables

Accruals_Min	Minimum value of total (or abnormal) accruals in firm-pair firm *i* and firm *j*.
Size_Diff	Absolute value of difference in size in firm-pair firm *i* and firm *j*. Size equals natural logarithm of total assets.
Size_Min	Minimum value of size in firm-pair firm *i* and firm *j*.
LEV_Diff	Absolute value of the difference in leverage in firm-pair firm *i* and firm *j*, where leverage is a debt-to-assets ratio of a company.
LEV_Min	Minimum value of leverage in firm-pair firm *i* and firm *j*.
MB_Diff	Absolute value of difference in market-to-book ratio in firm-pair firm *i* and firm *j*. Market-to-book ratio is calculated as market value of equity divided by book value of equity.
MB_Min	Minimum value of market-to-book ratio in firm-pair firm *i* and firm *j*.
CFO_Diff	Absolute value of difference in cash flows from operations (scaled by total assets in year *t*−1) in firm-pair firm *i* and firm *j*.
CFO_Min	Minimum value of scaled cash flows from operations in firm-pair firm *i* and firm *j*.
LossProb_Diff	Absolute value of the difference in loss probability in firm-pair firm *i* and firm *j*. Loss probability is the proportion of quarters for which the firm reports a negative quarterly income before extraordinary items in the past 16 quarters.

(continued on next page)

APPENDIX A (continued)

LossProb_Min	Minimum value of loss probability in firm-pair firm *i* and firm *j*.
STD_Sales_Diff	Absolute value of the difference in standard deviation of quarterly sales in firm-pair firm *i* and firm *j*. Standard deviation of sales is calculated over the preceding 16 quarters.
STD_Sales_Min	Minimum value of standard deviation of quarterly sales in firm-pair firm *i* and firm *j*.
STD_CFO_Diff	Absolute value of the difference in standard deviation of quarterly operating cash flows in firm-pair firm *i* and firm *j*, where standard deviation of cash flows from operations is calculated over the preceding 16 quarters.
STD_CFO_Min	Minimum value of the standard deviation of quarterly cash flows from operations in firm-pair firm *i* and firm *j*.
STD_Sales_Grth_Diff	Absolute value of the difference in standard deviation of quarterly sales growth in firm-pair firm *i* and firm *j*, where standard deviation of sales growth is calculated over the preceding 16 quarters. Sales growth equals sales in current year *t* minus sales in year $t-1$ divided by sales in year $t-1$.
STD_Sales_Grth_Min	Minimum value of the standard deviation of quarterly sales growth in firm-pair firm *i* and firm *j*.
STD_CFO_Diff	Absolute value of the difference in standard deviation of quarterly operating cash flows in firm-pair firm *i* and firm *j*, where standard deviation of cash flows from operations is calculated over the preceding 16 quarters.
STD_CFO_Min	Minimum value of the standard deviation of quarterly cash flows from operations in firm-pair firm *i* and firm *j*.
CFO_COMP_COV	Within-industry cash flow co-movement across 16 consecutive quarters in firm-pair firm *i* and firm *j*, calculated as defined in Section III.
RET_COV	Within-industry return co-movement across 16 consecutive quarters in firm-pair firm *i* and firm *j*, calculated as defined in Section III.