**Are auditor reputation effects driven by competitive poaching or negative media publicity? [[1]](#footnote-1)\***

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**Abstract:** This study tests whether auditor reputations are affected primarily by competitive poaching or by adverse media reports about auditors. Competitive poaching refers to the situation in which a rival audit office privately informs potential clients about the alleged audit failure of a competitor in an attempt to win clients from the tainted office. We find very strong evidence supporting the poaching effect whereas the effects of media publicity are relatively small. We conclude that auditor reputation effects are driven more by private communications from rival auditors rather than adverse stories in the media.

**I. INTRODUCTION**

There is evidence that auditors lose clients and struggle to win new clients when they are accused of audit failures (Weber, Willenborg, and Zhang 2008; Skinner and Srinivasan 2012; Swanquist and Whited 2015). However, prior research has not examined how audit clients become informed about the audit failures. It is important to identify how clients become informed in order to better understand the information channels that drive auditor reputation effects. For example, do clients learn about alleged audit failures by reading negative stories in the media? Or do clients learn about alleged audit failures by hearing about them from rival auditors? In this study, we test whether the responses of audit clients to alleged audit failures are driven by their private communications with rival auditors or by adverse stories in the media.

We hypothesize that auditors have economic incentives to inform potential clients about the alleged audit failures of their rivals because doing so can help them win clients from the tainted auditors. To test this argument, we exploit the fact that some auditors are better informed than others about the alleged audit failures of their rivals. To see how our identification strategy works, consider the following illustrative example. Suppose a city-market has four auditors: A, B, C, and D. Auditor A is sued for an alleged audit failure at client X and X subsequently switches from auditor A to auditor B. Suppose the other three auditors in the city-market (B, C, and D) are not sued. The clients in this city-market could learn about A’s lawsuit from any of A’s rivals (i.e., B, C, or D). Importantly, however, B is in a better position than C or D to inform A’s clients about A’s alleged audit failure because B is X’s new auditor and A’s alleged failure occurred at X. As X’s new auditor, B would have detailed and credible inside information about the alleged audit failure that resulted in A being sued. We expect B to leverage this inside information to portray A in a negative light, which would help B poach clients from auditor A. Auditors C and D do not audit client X, which means they would not have the same inside information pertaining to A’s alleged audit failure at X. Thus, we expect that A would lose more clients to auditor B than to other rival auditors.[[2]](#footnote-2)

Our poaching argument has tension for a couple of reasons. First, clients could learn about alleged audit failures through adverse stories in the media rather than through private communications with rival auditors. Consistent with this argument, prior studies examine audit failures that are very well publicized in the media, such as Enron-Andersen in the United States, Comroad-KPMG in Germany, and Kanebo-ChuoAoyama in Japan (Chaney and Philipich 2002; Barton 2005; Krishnamurthy, Zhou, and Zhou 2006; Chen and Zhou 2007; Weber et al. 2008; Skinner and Srinivasan 2012). Second, some auditors may consider poaching to be unfair or unethical. We received mixed reactions when we discussed the poaching hypothesis with former audit partners. Some told us that poaching is tantamount to muck-raking and such behavior would be inappropriate for audit professionals. The partners we spoke to said they had not poached clients from other audit firms by telling clients about their poor audit quality. However, some former partners told us they suspected that other rival auditors were engaged in poaching behavior against them.

We test whether clients can potentially learn about alleged audit failures through adverse stories in the media by determining whether each audit lawsuit in our sample receives media coverage. The media outlets we examine include traditional media (i.e., local newspapers, national newspapers and newswires) and online media (i.e., Google and Twitter).[[3]](#footnote-3) To the extent that media coverage drives auditor reputation effects, we expect a sued office to lose more market share when its lawsuit is covered in the media. Importantly, we document that most audit lawsuits are never mentioned in the press. Consequently, there is a potential role for auditors to provide clients with new information regarding the lawsuits of their audit competitors.

We assemble a sample of 494 audit lawsuits during the period 2000-2019, which comprise 325 lawsuits against the Big 4 firms and 169 lawsuits against the non-Big 4 firms.[[4]](#footnote-4) Our research design utilizes a treatment group of sued offices and a matched control group of non-sued offices. We select the control group by matching each sued office to an office in the same city-market that is not sued and that is closest in size to the sued office. Next, we calculate the net number of clients that move from the treatment (control) offices to other non-sued offices in the same city-market. Returning to our earlier example, auditor A is the treatment office because A is sued for an alleged audit failure at client X (X subsequently switches from auditor A to auditor B). The matched non-sued office is either C or D, depending on whether C or D is closest in size to A. For example, we choose C as the non-sued match if C is closer in size to A. For the sued office (A), we calculate the net number of clients that move between A and B and the net number of clients that move between A and D. Similarly, for the matched non-sued office (C), we calculate the net number of clients that move between C and B and between C and D.

We begin our analysis by replicating the evidence from prior research that offices with alleged audit failures tend to lose clients (e.g., Swanquist and Whited 2015). Specifically, we show that a sued office (A) loses more clients to non-sued offices (B and D) than the matched non-sued office (C) loses to other non-sued offices (B and D). After establishing this baseline result, we examine whether this reputation effect is attributable to private communications from rival offices or media coverage.

To the extent that A’s losses of market share are attributable to private communications from rival auditors, we would expect A to lose more clients to auditor B than to other auditors, given that B has inside knowledge of A’s alleged audit failure because B is X’s new auditor and A’s alleged audit failure occurred at client X. In other words, B is in a better position than C or D to poach A’s clients because B has detailed and credible inside information about the alleged audit deficiencies that resulted in A being sued. Consistent with this poaching argument, we find very strong evidence that the sued office (A) loses significantly more clients to auditor B than A loses to other non-sued offices. To the extent that A’s losses of market share are attributable to media publicity, we would expect A to lose more clients when A’s lawsuit is mentioned in the press. We find some evidence that reputational effects are attributable to media coverage. Specifically, the reductions in market share at the sued offices are significantly greater when their lawsuits are publicized in the media. However, the marginal effect of media publicity is found to be much smaller than the marginal effect of competitive poaching.

Our research design includes fixed effects for each office which should control for all time-invariant differences that explain the extent of auditor switching at different offices.[[5]](#footnote-5) Nevertheless, there is a potential concern that there could be other time-varying and unobservable factors that affect the natural patterns in auditor-client realignments. For example, auditor A’s clients might switch to auditor B instead of a different auditor if B has relevant expertise in auditing A’s clients. Alternatively, A’s clients might switch to B because they have no reasonable alternative choice of auditor, for example, because other auditors provide non-audit services to A’s clients. Furthermore, there could be a contagion effect whereby the non-sued clients of auditor A simply mimic the behavior of the sued client (X) by switching from auditor A to auditor B.

We address this concern by employing multiple falsification tests to determine the normal patterns in client-auditor realignments in the absence of an alleged audit failure. The logic for our falsification tests is that other motivations for switching from A to B could be present even in the absence of a lawsuit against auditor A. For example, if A’s clients choose B because B has relevant expertise, then we should find a similar tendency for A’s clients to choose B even when A is not sued. Likewise, if A’s clients choose B because they have no reasonable alternative choice of auditor, we should find that A’s clients choose B rather than a different auditor even when A is not sued. We perform the falsification tests by utilizing matched samples of “pseudo-X” clients that are not sued but which change auditor at the same rate as the actual X clients. We then test whether the non-sued clients of auditor A follow the pseudo-X client by moving from A to B. If our poaching results simply capture normal patterns in auditor-client realignments, we should find significant results in the falsification tests just as in our main tests. We find insignificant results in all our falsification tests. These findings suggest that our inferences are not contaminated by other factors that affect the normal incidence of client switching between auditor-pairs.

We provide four cross-sectional tests that further strengthen our inference that clients learn about alleged audit failures through private communications with rival auditors. First, the evidence for client poaching is expected to be stronger when offices A and B are more similar to each other in terms of their relative size because the two offices are then closer competitors. Consistent with this prediction, we find significantly more poaching from A to B when the two offices are similar to each other in terms of their size. Second, we expect the poaching effect to be stronger in city-markets that are more competitive. We capture the strength of competition using the Herfindahl-Hirschman index of audit market concentration. As expected, we find stronger evidence of poaching in less concentrated city-markets.[[6]](#footnote-6) Third, we expect that poaching is more likely to occur when audit litigation is relatively rare in the city-market (i.e., when the lawsuit is more surprising). Consistent with this prediction, we find stronger evidence of poaching in city-markets with relatively low rates of auditor litigation. Finally, the evidence for poaching is expected to be stronger when the lawsuit against auditor A has merit because in this situation auditor B would have more compelling inside information that A is a low-quality auditor. To assess whether a lawsuit has merit, we determine whether the lawsuit results in the auditor defendant making a payout to the plaintiff. We find the poaching results are significantly stronger for lawsuits with auditor payouts compared to lawsuits without auditor payouts. Overall, these cross-sectional tests indicate that poaching is more prevalent when competition is fiercer and when the rival auditor has more compelling inside information about A’s alleged audit failure.

Our study makes three contributions to the existing literature. First, our study is the first to examine how audit clients learn about alleged audit failures. Specifically, we test whether clients become informed through private communications with rival auditors or by reading media articles. Our results suggest that clients learn about alleged audit failures through private communications with rival auditors and, to a lesser extent, from news articles in the media. The poaching effect has not been examined in prior literature. Thus, we identify a new mechanism that explains how and why auditors lose market share after they are sued.

Second, we contribute to the auditor reputation literature by being the first to examine the direction of auditor switching between offices. Prior studies show that scandal-hit auditors lose clients (Weber et al. 2008; Skinner and Srinivasan 2012; Swanquist and Whited 2015), but prior research has not made predictions about which audit firms are chosen by such clients. Our study contributes to the literature by making explicit predictions about which auditors are hired by the clients that switch away from the sued offices. Specifically, we predict (and find) that a sued auditor (A) loses more clients to the auditor (B) that is privately informed about A’s lawsuit because it is the new auditor at the sued client (X).

Finally, we contribute to the literature by assessing the extent to which auditor reputation effects are driven by media coverage. This examination is important because prior studies focus on audit failures that are very well publicized in the media (Chaney and Philipich 2002; Barton 2005; Krishnamurthy et al. 2006; Chen and Zhou 2007; Weber et al. 2008; Skinner and Srinivasan 2012; Ege, Wang, and Xu 2021). These studies do not examine any audit failures that are *not* covered in the media. Consequently, it is unclear from prior literature whether the scandal-hit auditors lose clients because of the audit failure *per se* or because the audit failure is covered in the media. We clarify this issue by including in our sample the alleged audit failures without media coverage as well as those with media coverage. Thus, we are able to test whether auditors lose more clients when their alleged audit failures are covered in the media. Moreover, by including in our sample the alleged audit failures without media coverage, we are better able to test the impact of private communications.

The rest of the paper proceeds as follows. Section 2 discusses the prior literature, develops the hypothesis, and explains the research design. Section 3 describes the data, the sample, descriptive statistics, and main regression results. Section 4 presents the cross-sectional tests and additional analyses. Section 5 concludes.

**II. LITERATURE REVIEW AND POACHING HYPOTHESIS**

**Prior Literature on Auditor Reputation Effects**

Prior research has found significant reputational consequences from the audit scandal at Enron (Chaney and Philipich 2002; Barton 2005; Krishnamurthy et al. 2006; Chen and Zhou 2007). International studies also find reputation damage from well-publicized audit scandals in Germany and Japan (Weber et al. 2008; Skinner and Srinivasan 2012). However, the evidence on reputation effects is far from unanimous. Lennox (1999) finds that the market shares of UK auditors are unaffected when the quality of their audits is questioned by regulators or the media. In the U.S. setting, Nelson, Price, and Rountree (2008) find that the market response to earnings news did not decline for Andersen clients after it was revealed that Andersen had shredded working papers from the Enron audit. Berglund (2020) documents that auditors do not suffer reputational damage when auditors issue clean opinions to clients that subsequently file for bankruptcy. Based on interviews with attorneys, Maksymov, Pickerd, Lowe, Peecher, and Reffett (2020) report that attorneys believe large audit firms are not especially worried about reputational damage because large audit firms are sued so often.

There are alternative explanations for the mixed findings in prior research. One explanation is that clients do not care about audit quality or, worse still, some clients may actively seek out low-quality auditors. Consistent with this view, Cowle and Rowe (2022) find that lenient auditors tend to gain clients, whereas tough auditors tend to lose clients. Likewise, Ege and Stuber (2022) report that lenient auditors enjoy subsequent increases in market shares. Another potential explanation for the mixed results is that studies use different approaches to identify alleged audit failures. Most studies identify alleged audit failures from cases that are well-publicized in the media (Barton 2005; Krishnamurthy et al. 2006; Chen and Zhou 2007; Weber et al. 2008; Skinner and Srinivasan 2012; Ege et al. 2021). However, such cases are unlikely to be representative of alleged audit failures in general. Financial media journalists tend to pick sensational stories and so they may choose not to cover an audit lawsuit if the story is not sensational (Call, Emett, Maksymov, and Sharp 2022).

Other than stories publicized by the media, some studies identify alleged audit failures from the reports issued by accounting regulators (Wilson and Grimlund 1990; Firth 1990; Aobdia and Shroff 2017). Other studies examine the reputational consequences of client restatements (Swanquist and Whited 2015; Irani, Tate, and Xu 2015) or instances in which clients file for bankruptcy after receiving clean audit opinions (Berglund 2020). However, it is debatable whether these situations are always indicative of audit failures. For instance, an auditor may not be to blame when a client files for bankruptcy following a clean audit opinion because the client’s bankruptcy may be triggered by events that occurred after the auditor issued the opinion.

An audit lawsuit is (by definition) an alleged audit failure. Therefore, in this study, we examine the reputational fallout of lawsuits against auditors. Specifically, we examine whether the lawsuits are publicized in the media. More importantly, we test whether clients respond to stories circulated in the media or they respond to communications from rival auditors.

**Prior Literature on Media Coverage of Audit Failures**

Prior studies document significant reputational damage from well-publicized audit failures (Barton 2005; Krishnamurthy et al. 2006; Chen and Zhou 2007; Weber et al. 2008; Skinner and Srinivasan 2012; Ege et al. 2021). However, it is unknown whether such failures would have resulted in significant reputation damage if they had not received significant media coverage. In other words, prior studies have not tested whether media coverage has a significant incremental effect on auditor reputations, relative to alleged audit failures not covered in the media. We address this gap in the literature by examining alleged audit failures that do not attract media coverage as well as those covered in the media.

**Client Poaching**

We hypothesize that rival auditors poach clients from their competitors by telling them about the lawsuits of their competitors. For instance, suppose that auditor A is sued for an alleged audit failure at client X and X subsequently switches from auditor A to auditor B. As X’s new auditor, B would have a unique insight into the quality of A’s past audits at client X. Auditing standards require communications between the predecessor auditor (A) and the incoming auditor (B). In addition, the incoming auditor (B) would likely have conversations with X’s management and audit committee about the events that led to its former auditor (A) being sued. Such conversations could take place during the audit engagement bidding process or after B has won the engagement. Given the existence of such conversations, we expect that B would have detailed knowledge about the circumstances that led to A being sued. In contrast, other clients and other auditors in the same city-market may not know about A’s lawsuit.[[7]](#footnote-7) Even if they know that A is being sued, they are unlikely to be as well informed as B about the circumstances that resulted in A being sued. We expect auditor B to be well informed about A’s alleged audit failure because B is X’s new auditor. Other auditors are not serving the litigated client X and would therefore not be as informed.

Armed with inside information about A’s alleged audit failure, we expect auditor B would communicate with A’s other (non-sued) clients to convince them that B is a superior auditor to A. Consistent with this expectation, former partners have told us that, during the audit engagement bidding process, an audit firm will try to convince a potential client that their firm is better than the client’s incumbent audit firm. The motivation for these private communications is that the bidding auditor desires to win the client from the incumbent auditor. Auditors have plenty of opportunities to communicate with the clients of other auditors. For example, auditors interact with potential clients when they bid for new audit engagements. They also talk to potential clients at professional conferences and local business events. Such communications can help a rival non-sued auditor poach clients from the sued auditor. For example, when B informs A’s clients about the alleged audit failure at client X, A’s clients are likely to downgrade their assessment of A’s quality. Those clients are likely to migrate from auditor A to auditor B because B’s communications signal to them that B understands the audit issues that arose during A’s failed audit. Accordingly, we hypothesize that rival auditors inform clients about the lawsuits of their competitors in order to poach clients from the sued offices.

*Hypothesis: Auditors inform clients about the lawsuits of their competitors in order to poach clients from the sued offices.*

While the poaching hypothesis is intuitive, there are several reasons why it may not hold true in the data. First, rival audit firms naturally have incentives to exaggerate the significance of the alleged audit failures of their competitors. Clients may therefore consider rival audit firms to be biased and unreliable sources of information about the alleged audit failures of their competitors. Clients may discount, or even ignore, the communications from rival audit firms unless such stories are independently corroborated by the press.

Second, when a rival auditor informs clients about a competitor’s lawsuit, those clients may decide to leave the sued auditor, but they may not necessarily migrate to the auditor that informed them. The clients of the sued auditor may instead switch to a different auditor. For example, when B informs A’s clients about A’s lawsuit, those clients may regard A as a low-quality auditor (because of the lawsuit) but they may also regard B as a low-quality auditor because B agreed to become the new auditor of the litigated client (X). Therefore, A’s clients may not move to B even though B told them about A’s lawsuit.

Finally, our hypothesis assumes that clients generally switch away from auditors who are accused of audit failures. This assumption may not be true to the extent that clients prefer lenient rather than high-quality auditors (Cowle and Rowe 2022; Moser 2021; Ege and Stuber 2022).

**Research Design**

The extant literature tests whether auditors lose clients following alleged audit failures (Weber et al. 2008; Skinner and Srinivasan 2012; Swanquist and Whited 2015). Our study is different because we also make predictions about which auditors gain the lost clients. When auditor A is sued for an alleged audit failure, we predict that B gains more clients from A if B is the new auditor at the sued client (X). Therefore, in contrast to the extant literature, our research design must test not only whether A loses clients after A is sued but whether those clients choose B as their new auditor. In other words, our research design must examine the direction of client moves from the sued auditor.

There are three steps to our research design, as illustrated in Figures 1a, 1b, and 1c, respectively. In the first step, we identify which auditors are sued (not sued) in each city-market-year. For example, Figure 1a illustrates a city-market with four auditors (A, B, C, and D), where A is sued for an alleged audit failure at client X and the other three auditors (B, C, and D) are not sued. In the second step (Figure 1b), we identify whether each sued client (X) switches auditor or instead stays with the sued auditor (A). If client X switches auditor, we label X’s new auditor as auditor B. Otherwise, client X remains with auditor A.

[INSERT FIGURES 1a, 1b, AND 1c HERE]

In the third step, we examine the auditor switch decisions of non-sued clients. Our objective is to determine whether A’s non-sued clients systematically move to auditor B (the incoming auditor at the sued client) rather than to other auditors. As shown in Figure 1c, we label the sued auditor (A) as office 1 and we label X’s new auditor (B) as office 2. We match each sued office (A) with another non-sued office in the same city-market. As shown in Figure 1c, the matched non-sued office is either C or D, depending on which office is closest in size to A. For example, we choose C as A’s non-sued match if C is closer in size to A than D is.

To examine the direction of client moves, we calculate the net number of clients that move between each pairwise combination of office 1 and office 2. For example, in a city-market with four auditors (as per Figure 1c), we count the net number of clients that move between A and B, between A and D, between C and B, and between C and D. To the extent that A loses clients as a result of B’s poaching behavior, we would expect the net number of clients moving from A to B to exceed the net number of clients moving from A to D (relative to the net number of clients who move from C to B and from C to D).

For each office-pair observation, we count the net number of clients that switch between offices 1 and 2. The *LOSSES1,2* variable counts the number of clients lost by office 1 to office 2 in a two-year window after the lawsuit is filed against auditor A, where the two-year window includes the year of the lawsuit and the following year. We choose a two-year window because it can take time for A’s clients to change auditor. The *GAINS1,2* variable equals the number of clients gained by office 1 from office 2 over the same two-year window. We subtract the *LOSSES1,2* variable from *GAINS1,2* to calculate the net number of clients that move from office 1 to office 2. Before doing the subtraction, we first take log transformations of the *LOSSES1,2* and *GAINS1,2* variables to mitigate skewness and outliers in the numbers of clients lost and gained. Therefore, the *NETMOVES1,2* variable is defined as *Ln(GAINS1,2)* minus *Ln(LOSSES1,2)*. The *NETMOVES1,2* measure is equivalent to the natural log of the ratio of client gains to client losses (i.e., *Ln(GAINS1,2) – Ln(LOSSES1,2)* = *Ln(GAINS1,2/LOSSES1,2*). *NETMOVES1,2* variable takes positive values when office 1 gains more clients from office 2 than it loses to office 2. Conversely, the *NETMOVES1,2* variable takes negative values when office 1 loses more clients to office 2 than it gains from office 2.

We test the poaching hypothesis by estimating eq. (1).

*NETMOVES1,2* = α0 + α1 *SUED1* + α2 *SUED\_POACH1,2* + α3 *SUED\_MEDIA1* +

α4 *SUED\_CONNECT1,2* + α5 *LARGE1* + α6 *LARGE2* + α7 *SIMILARITY1,2* + α8 *COMP* +

Year FE + Office 1 FE + Office 2 FE + u (1)

The *SUED1* variable equals one if office 1 is sued in year *t*, and zero otherwise. Recall from Figure 1c that office 1 is either a sued office (A) or its non-sued match (C). Therefore, the *SUED1* variable equals one for the sued office (A), while *SUED1* equals zero for the non-sued matched office (C).[[8]](#footnote-8) To the extent that auditors lose clients after they are sued, we would expect the sued office (A) to lose more clients than its non-sued match (C) loses, implying a significant negative coefficient on *SUED1*; i.e., α1 < 0.

The *SUED\_POACH1,2* variable is our main variable of interest. *SUED\_POACH1,2* equals one if office 1 is sued in year *t* *and* the litigated client (X) switches from office 1 to office 2 (from A to B in Figure 1b), and zero otherwise. For example, the *SUED\_POACH1,2* variable equals one for office-pair (A, B) if office A is sued in year *t* and the litigated client (X) switches from A to B. The *SUED\_POACH1,2* variable equals zero if office 1 is sued but the litigated client (X) does not switch auditor. Likewise, the *SUED\_POACH1,2* variable equals zero for the matched office (C) because C is not sued. (Definitions for each variable are shown in Table 1.) To the extent that client moves are driven by poaching, we would expect the sued office (A) to lose more clients to B than to other auditors because B is better informed about A’s alleged audit failure given that B is the incoming auditor for client X. Thus, under the poaching hypothesis, we predict a significant negative coefficient on *SUED\_POACH1,2* in eq. (1); i.e., α2 < 0.

[INSERT TABLE 1 HERE]

We control for other factors that could explain the changes in market share experienced by the sued auditor (A). We expect a sued auditor to lose more clients when its lawsuit is covered in the media. We test the effect of media coverage using the *SUED\_MEDIA1* variable, which equals one if office 1 is sued in year *t* *and* the lawsuit is covered in the media, and zero otherwise.[[9]](#footnote-9) The media outlets we examine include traditional media (i.e., local newspapers, national newspapers and newswires), as well as social/online media (i.e., Google and Twitter). To the extent that media coverage explains why auditors lose clients after they are sued, we would expect a significant negative coefficient on the *SUED\_MEDIA1* variable.

An auditor’s non-sued clients might learn about the lawsuit through board connections with the sued client (X). We therefore control for the existence of board connections between X and the non-sued clients of offices 1 and 2 (*SUED\_CONNECT1,2*). If directors share information about audit lawsuits, we would expect to see an increased incidence of non-sued clients switching from office 1 towards office 2. This would imply a negative coefficient on *SUED\_CONNECT1,2*.

Large offices have more clients that can be poached by rival offices. We therefore control for the size of office 1 (*LARGE1*) and office 2 *(LARGE2)*. We expect more switching between offices 1 and 2 if the offices are similar to each other in terms of their relative size. We therefore control for *SIMILARITY1,2*, which equals one if office 1 and office 2 are both large offices or both small offices, and zero if office 1 and office 2 are of different size. We expect more switching in city-markets that are more competitive. We therefore control for market competitiveness (*COMP*), which is measured using the Herfindahl-Hirschman index (HHI) of concentration. Eq. (1) includes year fixed effects to control for time-varying factors that could influence client moves between offices 1 and 2. Eq. (1) also includes fixed effects for each office 1 and office 2. These fixed effects control for time-invariant characteristics that can affect an office’s general tendency to gain or lose clients during the sample period.

Before proceeding further, we make several observations about our research design. These observations are important given that our paired design is different from prior research. First, the reason our research design is different is that we must examine the direction of auditor switches between offices 1 and 2 in order to test the poaching hypothesis. In contrast, prior research focuses on the client moves of sued and non-sued auditors (i.e., office 1). In Appendix A, we show that a traditional research design focused on office 1 yields similar results for the *SUED1*, *SUED\_MEDIA1*, and *LARGE1* variables.

Second, we exclude the auditor switches of each litigated client (X) when we construct the dependent variable (*NETMOVES1,2*). Thus, the dependent variable captures the net moves of *non-sued* clients (not sued clients). We exclude the switches of sued clients because otherwise there would be a mechanical association between *NETMOVES1,2* and *SUED\_POACH1,2*.

Third, we illustrated our research design for a city with just four audit firms (Figure 1a). However, in reality, most cities have more than four auditors. For example, New York City has more than 200 audit firms. In a city-market with four auditors (A, B, C, D), there are just four pairwise combinations: (A, B), (A, D), (C, B), and (C, D). A larger city-market has more auditors and therefore more pairwise observations. For example, a city-market with five auditors (and one sued auditor) has six pairwise combinations: (A, B), (A, D), (A, E), (C, B), (C, D), and (C, E). In a large city, such as New York, there are many pairwise combinations because there are many auditors. No matter how many auditors there are within a given city, each pairwise combination has an office 1 (which is either a sued office or its matched non-sued office) and an office 2 (which is another non-sued office).

Finally, as shown in Figure 1c, we use a matched design to count the net number of clients lost by the sued auditor (A) relative to the net number of clients lost by A’s non-sued match (C).[[10]](#footnote-10) An alternative approach would be to calculate the net number of client switches for every possible office-pair using an unmatched design. For example, in Figure 1c, we could compute the net number of clients that switch between A and C and the net number of clients that switch between B and D as well as the other four pairwise combinations [(A, B), (A, D), (B, C), (C, D)]. In this case, there would be six office pairs [(A, B), (A, C), (A, D), (B, C), (B, D), (C, D)] rather than four for a city with four auditors. In untabulated tests, we find that our results are robust when we use this alternative unmatched approach instead of the matched approach shown in Figure 1c.

**III. SAMPLE, DESCRIPTIVE STATISTICS, AND MAIN RESULTS**

**Data and Sample**

Using the *Audit Analytics* database, we identify 1,110 accounting lawsuits between 2000 and 2019, where the litigated client is an SEC registrant and the client’s auditor is a defendant. We drop 56 lawsuits where we cannot identify the location of the sued office using information from *Audit Analytics*, SEC filings, and a search of Google. We exclude another 120 lawsuits in which the sued auditor is from a foreign country. We delete lawsuits involving Arthur Andersen LLP because prior research has already examined the reputational consequences of the Andersen scandal (Chaney and Philipich 2002; Barton 2005; Krishnamurthy et al. 2006; Chen and Zhou 2007). Sometimes a single audit failure can result in multiple lawsuits against the auditor by different plaintiffs. In such cases, we keep the first lawsuit against the auditor to ensure that each lawsuit observation corresponds to a single audit failure. As shown in Table 2, these data screens yield a total of 494 audit lawsuits from 2000 to 2019. There are 325 lawsuits against the Big 4 and 169 lawsuits against the non-Big 4 firms.

[INSERT TABLE 2 HERE]

Next, we construct pairwise combinations of offices (as illustrated in Figure 1c) for each city-market-year. Using the *Audit Opinion* module in the *Audit Analytics* database, we obtain 21,957 office-years (after dropping 143 office-years pertaining to Arthur Andersen LLP). We merge this sample with the data on audit lawsuits to ensure that each city-market-year has at least one sued office. This last data screen and the matched research design leave a final sample of 14,120 office-pair-years.

To construct the *SUED\_POACH1,2* variable, we examine whether the litigated client (X) switched auditor in the period from the end of the misreporting to the end of the lawsuit.[[11]](#footnote-11) Of the 494 audit lawsuits in our sample, we find 173 cases in which the litigated client (X) switches from the sued auditor (A) to another auditor (B) as shown in Figure 1b.

Next, we calculate the net number of non-sued clients that move between each pairwise combination of office 1 and office 2 (*NETMOVES1,2*), as shown in Figure 1c.[[12]](#footnote-12) Using the *Auditor Change* module in the *Audit Analytics* database, we identify 17,839 companies that change auditor between 2000 and 2020. We drop 1,428 auditor changes that are caused by audit firm mergers, splits, or exits. We delete another 1,606 auditor changes that involve Arthur Andersen LLP. We drop 173 litigated clients (i.e., client X) to avoid a mechanical association between *NETMOVES1,2* and *SUED\_POACH1,2*.[[13]](#footnote-13) We use the remaining sample of 14,632 auditor changes to calculate the net number of non-sued clients that move between office 1 and office 2 (*NETMOVES1,2*).[[14]](#footnote-14)

**Descriptive Statistics**

Table 3 shows the numbers of lawsuits and offices in each sample year (2000-2019). The number of lawsuits is presented in Col. (1), while Col. (2) shows how many lawsuits are publicized in the media. Consistent with Honisberg, Rajgopal, and Srinivasan (2020), we find a marked drop in the number of lawsuits against auditors following the 2011 ruling in *Janus v. First Derivative*. Of the 494 lawsuits in our sample, 161 (32.6%) are mentioned in the media.[[15]](#footnote-15) Col. (3) reports the number of audit offices, while Col. (4) shows how many offices are sued. In an average year, the frequency of an office being sued is only 1.93%, which reflects that litigation against auditors is quite rare.

[INSERT TABLE 3 HERE]

Panel A of Table 4 shows the number of audit engagements and the number of audit lawsuits for each city-market. New York has the most engagements (45,340) and the most lawsuits (85). Chicago has the second most engagements (35,177) and the second most lawsuits (20). The third largest city is Boston (21,344 engagements) with 16 lawsuits, followed by Philadelphia (12,021 engagements and 11 lawsuits) and Houston (11,550 engagements and 17 lawsuits). Of the 868 cities in our sample, there are 24 cities with at least 2,000 audit engagements each. There are 844 cities with no more than 2,000 audit engagements each (88,772 engagements in total).

[INSERT TABLE 4 HERE]

Panel B of Table 4 reports the number of audit lawsuits for each office location. The offices with the most lawsuits are the New York offices of PricewaterhouseCoopers LLP and Deloitte & Touche LLP, which were sued 13 times each during the sample period (2000 to 2019). The office with the next most lawsuits is the New York office of Ernst & Young (12 lawsuits), followed by the San Jose office of PricewaterhouseCoopers LLP (10 lawsuits). There are 22 offices that are sued at least four times, another 16 offices are sued three times, 58 offices are sued twice, and 189 offices are sued once. The remaining 3,462 offices are not sued at all during the sample period (2000 to 2019). These statistics indicate reasonable time variation in the incidence of litigation at a given office, which is important because our research design includes fixed effects for each office.[[16]](#footnote-16)

Panel A of Table 5 reports descriptive statistics for the variables. The mean number of clients lost by office 1 to office 2 (*LOSSES1,2*) is 0.059 and the mean number of clients gained by office 1 from office 2 (*GAINS1,2*) is 0.043. Therefore, office 1 loses more clients to office 2 than it gains from office 2 (which is expected because office 1 is either a sued office or its non-sued match, whereas office 2 is always a non-sued office). Accordingly, the mean value of *NETMOVES1,2* is negative (−0.012), indicating that office 1 tends to lose clients on a net basis. On average, the sued office (A) loses (gains) 0.353 (0.050) clients to the poaching office (B), based on the untabulated mean values of *LOSSES1,2* and *GAINS1,2* in the sub-sample where *SUED\_POACH1,2* = 1. Thus, the net number of clients lost by office 1 to office 2 is primarily attributable to clients moving from the sued auditor (A) to the poaching auditor (B). The mean values of *SUED\_MEDIA1* and *SUED\_POACH1,2* are 0.169 and 0.010, respectively.[[17]](#footnote-17)

[INSERT TABLE 5 HERE]

Panel B of Table 5 presents the Pearson (Spearman) correlations in the lower (upper) diagonal. We find a significant negative correlation between *NETMOVES1,2* and *SUED1*, indicating that sued offices lose more clients than non-sued offices. Similarly, there is a significant negative correlation between *NETMOVES1,2* and *SUED\_POACH1,2*, suggesting that sued offices lose clients as a result of poaching.

**Univariate Tests**

Panel C of Table 5 provides univariate tests. The mean value of *NETMOVES1,2* is significantly more negative for the sued offices (*SUED1* = 1) than the matched non-sued offices (*SUED1* = 0). Therefore, sued offices tend to lose more market share than non-sued offices. More importantly, the losses in market share are largely driven by a poaching effect. The sued offices with exposure to the poaching effect (*SUED\_POACH1,2* = 1) lose significantly more clients than sued offices in general (*SUED1* = 1) or sued offices whose lawsuits are publicized in the media (*SUED\_MEDIA1* = 1). Overall, these univariate results provide initial evidence that client poaching drives the losses of market share suffered by sued auditors.

**Regression Results**

Panel D of Table 5 reports the regression results for eq. (1). Consistent with prior research (and the analyses of Appendix A), we find that sued offices lose more clients (on a net basis) than matched non-sued offices. Col. (1) shows a significant negative coefficient on *SUED1* (*t-stat.* = −3.057), suggesting that sued offices lose more clients (on a net basis) than matched non-sued offices.

Having established this baseline result, we next examine whether the client losses of sued offices are driven by competitive poaching or media publicity. Specifically, we add the *SUED\_POACH1,2* and *SUED\_MEDIA1* variables to Col. (2). Consistent with our poaching hypothesis, we find a significant and negative coefficient on *SUED\_POACH1,2* (*t-stat.* = −4.299). This finding indicates that poaching is a significant driver of the losses in market share experienced by the sued auditors.

We also find a significant and negative coefficient on *SUED\_MEDIA1* (*t-stat.* = −2.232), suggesting that media coverage is a significant driver of reputation effects. An F-test reveals that the coefficient on *SUED\_POACH1,2* is significantly more negative than the coefficient on *SUED\_MEDIA1* (p<0.001). Thus, the marginal effect of poaching is significantly stronger than the marginal effect of media coverage.[[18]](#footnote-18) In contrast, the board connections variable (*SUED\_CONNECT1,2*) is statistically insignificant. The other control variables (*LARGE1*, *LARGE2*, *SIMILARITY1,2*, and *COMP*) are also insignificant.

**Falsification Tests**

A potential concern with our research design is that A’s non-sued clients might switch from A to B (Figure 1c) for the same reason that the sued client (X) switched from A to B (Figure 1b). In other words, our results might be explained by factors that are common to the switching decisions of sued clients (Figure 1b) and non-sued clients (Figure 1c). For example, B might attract clients from A because B’s partners have greater expertise in auditing A’s clients. Alternatively, A’s clients might switch to B because they have no reasonable alternative choice of auditor. To address the concern that such factors explain why A’s clients choose auditor B rather than another auditor, we employ multiple falsification tests.

The logic for our falsification tests is that the other factors that drive switching from A to B should be present even in the absence of a lawsuit against auditor A. For example, if A’s clients move to B because B has greater expertise, there should be a similar tendency for A’s clients to move to B rather than an alternative auditor even when A is not sued. Similarly, if A’s clients choose B because they have no reasonable alternative choice of auditor, we should find that A’s clients choose B rather than a different auditor even when A is not sued.

We therefore employ falsification tests that use matched samples of pseudo-X clients (the pseudo-X clients are companies that change auditor but which are not involved in an audit lawsuit). Specifically, we test whether the non-sued clients of auditor A follow the pseudo-X clients by moving from auditor A to auditor B. If our results for client poaching are attributable to other confounding factors that affect the moves from A to B, we should find the same systematic tendency for A’s clients to move to B rather than to a different auditor when we use the pseudo-X clients instead of the actual litigated X clients.

Our lawsuit sample has 321 clients that do not switch auditor and 173 clients that switch auditor. To construct the sample of pseudo-X clients, we first match the non-sued clients to the sued clients based on their size and industry. We thus identify 321 non-sued pseudo-X clients that do not switch auditor and 173 non-sued pseudo-X clients that switch auditor. We use the 321 observations with no auditor change to construct a new set of office-pairs in which *PSEUDO\_SUED1* = 1 and *PSEUDO\_SUED\_POACH1,2* = 0. Similarly, we use the 173 observations with auditor changes to construct a new set of office-pairs in which *PSEUDO\_SUED1* = 1 and *PSEUDO\_SUED\_POACH1,2* = 1. We then re-compute the *NETMOVES1,2* variable using this new sample.[[19]](#footnote-19) If our poaching results are attributable to other factors that explain client moves from A to B, we would expect the inferences for *PSEUDO\_SUED\_POACH1,2* in this falsification test to be the same as the inferences for *SUED\_POACH1,2*.

To conduct the falsification test, we re-estimate the models of Panel D, Table 5, after replacing *SUED1* with *PSEUDO\_SUED1* and replacing *SUED\_POACH1,2* with *PSEUDO\_SUED\_POACH1,2*. In untabulated tests, we find insignificant coefficients on *PSEUDO\_SUED1* and *PSEUDO\_SUED\_POACH1,2* (*t-stats.* = −1.579, −0.551, respectively). This finding suggests that our poaching results are not attributable to other factors that explain the propensity for A’s clients to switch to B.

In another falsification test, we match the pseudo-X clients to the sued clients based on the identity of the client’s audit office as well as the client’s size and industry. In this falsification test, we continue to find insignificant coefficients on *PSEUDO\_SUED1* and *PSEUDO\_SUED\_POACH1,2* (*t-stats.* = 0.268, 0.791, respectively).

As a final falsification test, we create the sample of pseudo-X clients by testing for client poaching in the two years *prior* to office A being sued. In this test, we continue to find insignificant coefficients on *PSEUDO\_SUED1* and *PSEUDO\_SUED\_POACH1,2* (*t-stats.* = − 0.570, 0.170, respectively). Therefore, there is no evidence of client poaching during the two years before the lawsuit is initiated against auditor A.

Overall, these falsification tests suggest that our main results are not attributable to other factors that prompt A’s clients to switch to B.

**IV. CROSS-SECTIONAL TESTS AND ADDITIONAL ANALYSES**

**Cross-sectional Tests**

In this section, we strengthen our causal inferences by exploiting cross-sectional variation in the incentives and ability of auditors to poach clients from their rivals. We argue that an auditor’s ability to poach clients from the sued auditor varies with: 1) the similarity in office size, 2) the intensity of competition, 3) the overall frequency of audit litigation in the city-level market, and 4) the severity of the lawsuit against the sued auditor. We therefore test whether the strength of the poaching effect varies with: 1) the similarity in size between pairs of offices, 2) the level of competition in the city-level market, 3) the frequency of audit litigation in the city-level market, and 4) the presence of a payout by the sued auditor to the plaintiffs.

***Similarity in Size Between Offices 1 and 2***

We expect more client poaching when offices 1 and 2 are closer competitors to each other. We assume the two offices are closer competitors when they are more similar to each other in terms of their relative size. Therefore, in this section, we test whether poaching occurs more often when offices 1 and 2 are similar in size. To test this idea, we interact the *SIMILARITY1,2* variable with the *SUED1* and *SUED\_POACH1,2* variables, as shown in eq. (2):

*NETMOVES1,2* = α0 + α1 *SUED1* + α2 *SUED\_POACH1,2* + α3 *SUED\_MEDIA1* +

α4 *SUED1* × *SIMILARITY1,2* + α5 *SUED\_POACH1,2* × *SIMILARITY1,2* +

α6 *SUED\_CONNECT1,2* + α7 *LARGE1* + α8 *LARGE2* + α9 *SIMILARITY1,2* + α10 *COMP* +

Year FE + Office 1 FE + Office 2 FE + u (2)

We expect the poaching effect to be stronger when offices 1 and 2 are similar in terms of their relative size. Accordingly, we predict a negative coefficient on *SUED\_POACH1,2 × SIMILARITY1,2* (α5 < 0).

The results for eq. (2) are shown in Col. (1) of Table 6. We find a significant and negative coefficient for *SUED1 × SIMILARITY1,2* (*t-stat.* = −3.711). Therefore, a sued office 1 loses more clients to the non-sued office 2 when offices 1 and 2 are more similar in terms of their relative size. More importantly, we find a significant and negative coefficient for *SUED\_POACH1,2 × SIMILARITY1,2* (*t-stat.* = −2.492). Therefore, the poaching effect is significantly stronger when offices 1 and 2 are more similar to each other.

[INSERT TABLE 6 HERE]

***City-markets with Low versus High Competition***

We next investigate whether the strength of the poaching effect varies with the intensity of competition within the city-level audit market. We expect poaching to be more prevalent in city-markets that are more competitive. To test this idea, we interact the *COMP* variable with the *SUED1* and *SUED\_POACH1,2* variables, as shown in eq. (3):[[20]](#footnote-20)

*NETMOVES1,2* = α0 + α1 *SUED1* + α2 *SUED\_POACH1,2* + α3 *SUED\_MEDIA1* +

α4 *SUED1* × *COMP* + α5 *SUED\_POACH1,2* × *COMP* + α6 *SUED\_CONNECT1,2* +

α7 *LARGE1* + α8 *LARGE2* + α9 *SIMILARITY1,2* + α10 *COMP* + Year FE + Office 1 FE +

Office 2 FE + u (3)

We expect more poaching in city-markets that are more competitive. We therefore predict a negative coefficient on the *SUED\_POACH1,2* × *COMP* interaction variable; i.e., α5 < 0.

The results for eq. (3) are shown in Col. (2) of Table 6. We find a significant and negative coefficient for *SUED1 × COMP* (*t-stat.* = −4.048). Therefore, a sued office loses more clients when the market is more competitive. More importantly, we find a significant negative coefficient on *SUED\_POACH1,2* × *COMP* (*t-stat.* = −4.966). This result suggests that the poaching effect is significantly stronger when offices operate in competitive city-markets.

***City-markets with Low versus High Rates of Audit Litigation***

We expect that a lawsuit would be more surprising and therefore more noteworthy in cities with low rates of litigation. Moreover, it is easier for rival auditors to poach clients from a sued office when the lawsuit is more noteworthy. Accordingly, we expect the poaching effect to be stronger in city-markets with relatively low rates of litigation. To test this idea, we construct a *LOW* dummy variable which takes the value one if the city’s rate of litigation (measured as the number of lawsuits divided by the number of audit engagements) is below or equal to the median rate of litigation among all cities in the sample, and zero otherwise. We then interact the *LOW* variable with the *SUED1* and *SUED\_POACH1,2* variables, as shown in eq. (4):[[21]](#footnote-21)

*NETMOVES1,2* = α0 + α1 *SUED1* + α2 *SUED\_POACH1,2* + α3 *SUED\_MEDIA1* +

α4 *SUED1* × *LOW* + α5 *SUED\_POACH1,2* × *LOW* + α6 *SUED\_CONNECT1,2* + α7 *LARGE1* + α8 *LARGE2* + α9 *SIMILARITY1,2* + α10 *COMP* + Year FE + Office 1 FE + Office 2 FE + u (4)

The results for eq. (4) are shown in Col. (3) of Table 6. We find a significant and negative coefficient for *SUED1 × LOW* (*t-stat.* = −3.073). Therefore, a sued office loses more clients when the market has a relatively low rate of litigation. More importantly, we find a significant negative coefficient on *SUED\_POACH1,2* × *LOW* (*t-stat.* = −2.945). This result suggests that the poaching effect is significantly stronger when offices are located in city-markets with relatively low rates of audit litigation.

***Lawsuit Payouts by Auditor Defendants to the Plaintiffs***

Finally, we examine whether the poaching effect is stronger when the allegations are more meritorious. We identify whether the lawsuit is meritorious by examining whether it results in a subsequent payout from the sued auditor to the plaintiffs. We define *PAY1* asequal to one if office 1 is sued in year *t* *and* the lawsuit results in the sued auditor making a payout to the plaintiffs, and zero otherwise*.* We interact the *PAY1* variable with the *SUED1* and *SUED\_POACH1,2* variables, as shown in eq. (5):[[22]](#footnote-22)

*NETMOVES1,2* = α0 + α1 *SUED1* + α2 *SUED\_POACH1,2* + α3 *SUED\_MEDIA1* +

α4 *SUED1* × *PAY1* + α5 *SUED\_POACH1,2* × *PAY1* + α6 *SUED\_CONNECT1,2* + α7 *LARGE1* +

α8 *LARGE2* + α9 *SIMILARITY1,2* + α10 *COMP* + Year FE + Office 1 FE + Office 2 FE + u (5)

Of the 494 lawsuits in our sample, 156 (31.6%) result in sued auditors making payouts; 37 lawsuits (7.5%) result in the plaintiff receiving a payout from at least one defendant but we do not have information on whether the auditor contributed to the payout; finally, 301 lawsuits (60.9%) result in the sued auditors not making a payout to the plaintiffs (e.g., the judge granted the auditor’s motion to dismiss the case against it).[[23]](#footnote-23) When we restrict our sample to the 457 lawsuits with auditor payout information, our estimation sample drops from 14,120 office-pair observations to 13,304.

The results for eq. (5) are shown in Col. (4) of Table 6. We find a significant negative coefficient on *SUED1* × *SUED\_PAY1* (*t-stat.* = −1.905). Therefore, a sued office loses more clients when the lawsuit is meritorious. More importantly, we find a significant negative coefficient on *SUED\_POACH1,2* × *SUED\_PAY1* (*t-stat.* = −5.298). Therefore, the poaching effect is significantly stronger when the lawsuit is meritorious.

**Additional Analyses**

***Alternative Measures of the Dependent Variable***

In our tabulated tests, the dependent variable (*NETMOVES1,2*) equals the log of the number of clients gained by office 1 from office 2 minus the log of the number of clients lost by office 1 to office 2. That is, *NETMOVES1,2* equals *Ln(GAINS1,2)* minus *Ln(LOSSES1,2)*. In this section, we test the robustness of our results using alternative measures of the dependent variable.

We begin by replacing the logged dependent variable (*NETMOVES1,2*) with an unlogged variable (*#NETMOVES1,2* = *GAINS1,2* − *LOSSES1,2*) in order to measure the economic significance of our findings. The results are shown in Col. (1) of Table 7. We continue to find a significant negative coefficient on *SUED\_POACH1,2* (*t-stat.* = −3.647). The *SUED\_POACH1,2* coefficient implies that the average net number of lost clients increases from 0.034 to 0.117 when *SUED\_POACH1,2* switches from zero to one. This corresponds to a 553.3% increase in the auditor switch rate relative to the mean auditor switch rate of 1.5% (i.e., 553.3% = (0.117 − 0.034)/0.015). Thus, the marginal effect of client poaching is economically large.

[INSERT TABLE 7 HERE]

Next, we measure the percentage change in market share (%*NETMOVES1,2*), which is defined as the unlogged net number of client moves (*#NETMOVES1,2*) divided by the total number of clients audited by office 1. As shown in Col. (2) of Table 7, we continue to find a significant negative coefficient on *SUED\_POACH1,2* (*t-stat.* = −3.648). Therefore, our primary findings are robust to using a percentage measure for the change in market share.

Thus far, we have measured client moves using a two-year window, which comprises the year of the lawsuit and the following year. As a robustness check, we re-compute the *NETMOVES1,2* variable using a three-year window. As shown in Col. (3) of Table 7, we continue to find significant negative coefficients on *SUED1* and *SUED\_POACH1,2* (*t-stats.* = −2.179, −2.784, respectively).

We further explore whether the poaching effect is driven by client losses or client gains. In other words, does office 1 lose more clients to office 2 or does office 1 gain fewer clients from office 2? We expect that poaching mainly involves increased client losses rather than fewer client gains because office 1 would be losing more clients to office 2 rather than gaining fewer clients from office 2. Cols. (4) and (5) of Table 7 report the results using *Ln(GAINS1,2)* and *Ln(LOSSES1,2)* as separate dependent variables. Col. (4) shows that, in the client gains model, the coefficient on *SUED\_POACH1,2* is statistically insignificant (*t-stat.* = −1.010). Col. (5) shows that the coefficient on *SUED\_POACH1,2* is positive and highly significant in the client losses model (*t-stat.* = 4.374). Therefore, the results for poaching are driven mainly by client losses rather than client gains.

***Spillover Effects on Other Non-sued Offices?***

When auditor B tells A’s clients about A’s lawsuit, A’s clients may move to a different auditor rather than B. Thus, B’s attempts to poach clients from A could have positive spillover effects on the market shares of other offices. Such spillover effects can only occur when the litigated client (X) switches auditor. Therefore, we test for spillover effects by creating a new independent variable that captures whether X changes its auditor (*SUED\_CH1,2*). The *SUED\_CH1,2* variable equals one if office 1 is sued in year *t* *and* the litigated client (X) switches to office 2, where office 2 is either the non-poaching auditor or the poaching auditor, and zero otherwise. In the presence of spillover effects, we would expect a significant negative coefficient on *SUED\_CH1,2*. In untabulated tests, the coefficient on *SUED\_CH1,2*is insignificant. In contrast, the coefficient on *SUED\_POACH1,2* remains negative and highly significant (*t-stat.* = −4.363). Therefore, we find no evidence that poaching (by B) has positive spillover effects on the market shares of other offices.

***Listed and Non-listed Clients***

Losing a listed client is likely to be more costly than losing a non-listed client. Therefore, we test whether our results hold after partitioning companies into listed and non-listed. Specifically, we create two dependent variables (*NETMOVES\_LIST1,2* and *NETMOVES\_NONLIST1,2*), where *NETMOVES\_LIST1,2* is the net number of listed clients that move from office 1 to office 2, and *NETMOVES\_NONLIST1,2* is the net number of non-listed clients that move from office 1 to office 2.[[24]](#footnote-24) We then re-estimate our models using *NETMOVES\_LIST1,2* and *NETMOVES\_NONLIST1,2* as the dependent variables. We find significant negative coefficients on *SUED\_POACH1,2* for both dependent variables. These findings suggest that there is poaching of both listed and non-listed clients.

***Accounting Restatements***

In our main tests, we use audit lawsuits to capture alleged audit failures. Other studies test whether auditors lose market share after their clients announce accounting restatements (Swanquist and Whited 2015; Irani et al. 2015). We therefore test whether our inferences about client poaching hold when we use restatements rather than audit lawsuits.

Appendix B reports the descriptive statistics and regression results for our analysis of accounting restatements. Consistent with our tabulated analysis, we find strong evidence in support of the poaching hypothesis. The coefficient on the *RESTATED\_POACH1,2* variable is negative and highly significant (*t-stat.* = −11.646). This finding suggests that auditors inform clients about the restatements of their competitors in order to poach clients from the offices with the restatements.Thus, the evidence supports the poaching hypothesis irrespective of whether we identify low-quality audits using audit lawsuits or accounting restatements.

**V. Conclusion**

This study examines alternative ways in which clients become informed about alleged audit failures. Specifically, we examine whether clients learn about audit failures as a result of communications from rival audit offices or the media. Our results suggest that clients learn about alleged audit failures through communications from rival auditors. In particular, sued offices lose more clients to rival offices that have inside information about the alleged audit failures because they are the incoming auditors at the litigated clients. These results are more pronounced when the two offices are similar in size, suggesting that poaching is greater when the two offices are closer competitors. In addition, the results are significantly stronger in city-markets with more intense competition and in city-markets with low rates of audit litigation. Overall, our findings suggest that clients learn about alleged audit failures as a result of conversations with rival auditors. We conclude that inter-office competition is an important reason why auditors have economic incentives to protect their reputations.

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**Figure 1a**

**Identifying the sued and non-sued audit offices**

In this example, the city-market has four audit offices: A, B, C, and D. Office A is sued for an alleged audit failure by one of its clients (client X) whereas offices B, C, and D are not sued.

City-market with four audit offices (A, B, C, and D)

Office C

(not sued)

Office B

(not sued)

Office A

(sued by client X)

Office D

(not sued)

**Figure 1b**

**Auditor switching by the sued client (X)**

The sued client (X) moves from office A to office B.

Office B

Office A

The sued client (X) does not change auditor.

Office A

Office A

**Figure 1c**

**Auditor switching by the non-sued clients**

We count the net numbers of non-sued clients that move from A to B and from A to D, and we count the net numbers of non-sued clients that move from C to B and from C to D. Office 1 is either the treated office (A) or its non-sued match (C). Office 2 is another non-sued office to which clients may move (i.e., office B or D).

The dependent variable (*NETMOVES1,2*) captures the number of net client moves between each office pair (i.e., office 1 and office 2).

Client moves between

office 1 and office 2

(*NETMOVES1,2*)

Office 2

Office A

(treated)

Office C

(control)

Office D

Office B

Office 1

*NETMOVES1,2* = α0 + α1 *SUED1* + α2 *SUED\_POACH1,2* + α3 *SUED\_MEDIA1* +

*CONTROLS* + u (1)

The *SUED1*variable equals one if office 1 is sued, and zero otherwise. In the above illustrative example, *SUED1* equals one for office A because A is sued by one client (client X) whereas *SUED1*equals zero for office C because C is not sued by any client.

The *SUED\_POACH1,2* variable captures whether the sued clients switch auditor. For example, Figure 1b shows the sued client X switching from auditor A to auditor B. In this example, the *SUED\_POACH1,2* variable equals one for switches between A and B, whereas *SUED\_POACH1,2* equals zero for other types of switches (i.e., between A and D, C and B, or C and D). Under the poaching hypothesis, we expect A to lose more non-sued clients to B than to other auditors. Therefore, we predict a negative coefficient on *SUED\_POACH1,2* in eq. (1); i.e., α2 < 0.

The *SUED\_MEDIA1* variable equals one if office 1 is sued *and* the lawsuit is covered in the media, and zero otherwise. For example, *SUED\_MEDIA1* equals one if A’s lawsuit is covered in the media. In contrast, *SUED\_MEDIA1* equals zero if A’s lawsuit is not covered in the media. *SUED\_MEDIA1* equals zero for office C given that C is not sued.

**Table 1**

**Variable definitions**

|  |  |
| --- | --- |
| *LOSSES1,2* | = Number of clients lost by office 1 to office 2 in a two-year window surrounding a lawsuit being filed against office 1. See Fig. 1. |
| *GAINS1,2* | = Number of clients gained by office 1 from office 2 in a two-year window surrounding a lawsuit being filed against office 1. See Fig. 1. |
| *Ln(LOSSES1,2)* | = Natural log of (one plus) *LOSSES1,2*. |
| *Ln(GAINS1,2)* | = Natural log of (one plus) *GAINS1,2*. |
| *NETMOVES1,2* | = *Ln(GAINS1,2) − Ln(LOSSES1,2)*. |
| *#NETMOVES1,2* | = *GAINS1,2 − LOSSES1,2*. |
| *%NETMOVES1,2* | = *#NETMOVES1,2* divided by the total number of clients audited by office 1. |
| *SUED1* | = one if office 1 is sued in year *t*, and zero otherwise. |
| *SUED\_POACH1,2* | = one if office 1 is sued in year *t* *and* the client involved in the lawsuit switches from office 1 to office 2, and zero otherwise. |
| *SUED\_MEDIA1* | = one if office 1 is sued in year *t* *and* the lawsuit(s) is (are) covered by traditional (i.e., newspapers and newswires) or social/online media, and zero otherwise. |
| *SUED\_CONNECT1,2* | = one if office 1 is sued in year *t* *and* clients at offices 1 and 2 has at least one board connection with the litigated client, and zero otherwise. |
| *LARGE1* | = one if office 1 is above or equal to the median office size (where office size is measured as the number of clients audited by the office), and zero otherwise. |
| *LARGE2* | = one if office 2 is above or equal to the median office size (where office size is measured as the number of clients audited by the office), and zero otherwise. |
| *SIMILARITY1,2* | = one if office 1 and office 2 are similar in terms of their size (i.e., *LARGE1* = *LARGE2* = 1 or *LARGE1* = *LARGE2* = 0), and zero otherwise. |
| *COMP* | = one if the Herfindahl-Hirschman index of audit market concentration (HHI), measured as the sum of the squared market shares of all audit offices in a city-market, is below or equal to the median HHI value (where market share is measured as the percentage of the clients audited by the office), and zero otherwise. |
| *LOW* | = one if the city’s rate of litigation (measured as the number of audit lawsuits divided by the number of audit engagements) is below or equal to the median rate of litigation among all cities in the sample, and zero otherwise. |
| *PAY1* | = one if office 1 is sued in year *t* *and* the lawsuit results in the sued auditor making a payout to the plaintiffs, and zero otherwise. |

**Table 2**

**The sample of audit lawsuits (2000-2019)**

|  |  |
| --- | --- |
|  | Number of  audit lawsuits |
| Lawsuits that allege misreporting by an SEC registrant and the registrant’s audit firm is named as a defendant. | 1,110 |
| Less: lawsuits in which we cannot identify the location of the audit office. | (56) |
| Less: lawsuits in which the audit office is located in a foreign country. | (120) |
| Less: lawsuits against Arthur Andersen LLP. | (44) |
| Less: multiple lawsuits that pertain to the same misreporting event. | (396) |
|  |  |
| Final audit lawsuits sample | 494 |

**Table 3**

**Audit lawsuits and offices by year**

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | | (3) | (4) | |
| Year | Number of audit lawsuits | Number (%) of audit lawsuits covered in the media | | Number of offices | Number (%) of sued offices | |
| 2000 | 38 | 10 | 26.32% | 1,564 | 35 | 2.24% |
| 2001 | 21 | 11 | 52.38% | 1,490 | 21 | 1.41% |
| 2002 | 54 | 22 | 40.74% | 1,385 | 46 | 3.32% |
| 2003 | 43 | 16 | 37.21% | 1,262 | 42 | 3.33% |
| 2004 | 40 | 16 | 40.00% | 1,160 | 32 | 2.76% |
| 2005 | 40 | 11 | 27.50% | 1,173 | 37 | 3.15% |
| 2006 | 32 | 6 | 18.75% | 1,177 | 30 | 2.55% |
| 2007 | 34 | 7 | 20.59% | 1,133 | 32 | 2.82% |
| 2008 | 32 | 9 | 28.13% | 1,107 | 27 | 2.44% |
| 2009 | 33 | 8 | 24.24% | 1,094 | 29 | 2.65% |
| 2010 | 22 | 6 | 27.27% | 1,064 | 21 | 1.97% |
| 2011 | 38 | 14 | 36.84% | 1,033 | 30 | 2.90% |
| 2012 | 13 | 6 | 46.15% | 1,007 | 12 | 1.19% |
| 2013 | 14 | 5 | 35.71% | 954 | 14 | 1.47% |
| 2014 | 6 | 3 | 50.00% | 948 | 6 | 0.63% |
| 2015 | 11 | 2 | 18.18% | 929 | 11 | 1.18% |
| 2016 | 9 | 3 | 33.33% | 907 | 9 | 0.99% |
| 2017 | 7 | 2 | 28.57% | 872 | 7 | 0.80% |
| 2018 | 3 | 2 | 66.67% | 856 | 3 | 0.35% |
| 2019 | 4 | 2 | 50.00% | 842 | 4 | 0.48% |
|  |  |  |  |  |  |  |
| Total | 494 | 161 | 32.59% |  |  |  |

**Table 4**

**Audit lawsuits by city**

*Panel A: Audit engagements and audit lawsuits by city*

|  |  |  |  |
| --- | --- | --- | --- |
| City | Number of  audit engagements | Number (%) of audit  lawsuits (2000-2019) | |
| New York | 45,340 | 85 | 0.19% |
| Chicago | 35,177 | 20 | 0.06% |
| Boston | 21,344 | 16 | 0.07% |
| Philadelphia | 12,021 | 11 | 0.09% |
| Houston | 11,550 | 17 | 0.15% |
| Denver | 8,329 | 8 | 0.10% |
| Los Angeles | 7,390 | 18 | 0.24% |
| Dallas | 6,697 | 14 | 0.21% |
| Salt Lake City | 6,045 | 12 | 0.20% |
| Minneapolis | 6,022 | 2 | 0.03% |
| San Francisco | 5,129 | 12 | 0.23% |
| Atlanta | 4,660 | 12 | 0.26% |
| San Jose | 4,300 | 15 | 0.35% |
| Baltimore | 3,552 | 0 | 0.00% |
| Kansas City | 3,039 | 3 | 0.10% |
| San Diego | 3,000 | 9 | 0.30% |
| Seattle | 2,925 | 7 | 0.24% |
| Cleveland | 2,921 | 6 | 0.21% |
| Las Vegas | 2,877 | 3 | 0.10% |
| Mclean | 2,800 | 4 | 0.14% |
| Milwaukee | 2,570 | 4 | 0.16% |
| Columbus | 2,450 | 3 | 0.12% |
| Tampa | 2,386 | 6 | 0.25% |
| Irvine | 2,026 | 2 | 0.10% |
| 844 cities with ≤ 2,000 audit engagements | 88,772 | 205 | 0.23% |
|  |  |  |  |
| Total | 293,322 | 494 | 0.17% |

**Table 4 (cont.)**

**Audit lawsuits by city**

*Panel B: Audit lawsuits by office location (2000-2019)*

|  |  |  |
| --- | --- | --- |
| Audit firm | Office location | Number of audit lawsuits |
| PricewaterhouseCoopers LLP | New York | 13 |
| Deloitte & Touche LLP | New York | 13 |
| Ernst & Young LLP | New York | 12 |
| PricewaterhouseCoopers LLP | San Jose | 10 |
| PricewaterhouseCoopers LLP | Boston | 9 |
| KPMG LLP | Chicago | 8 |
| KPMG LLP | New York | 8 |
| KPMG LLP | Los Angeles | 6 |
| Ernst & Young LLP | Atlanta | 5 |
| Ernst & Young LLP | Chicago | 5 |
| KPMG LLP | Mountain View | 5 |
| KPMG LLP | San Diego | 5 |
| Sherb & Co LLP | New York | 5 |
| MaloneBailey LLP | Houston | 5 |
| PricewaterhouseCoopers LLP | Chicago | 4 |
| PricewaterhouseCoopers LLP | Houston | 4 |
| PricewaterhouseCoopers LLP | Los Angeles | 4 |
| PricewaterhouseCoopers LLP | Memphis | 4 |
| PricewaterhouseCoopers LLP | San Juan | 4 |
| KPMG LLP | San Francisco | 4 |
| BDO Seidman LLP | New York | 4 |
| Feldman Sherb & Co PC | New York | 4 |
| 16 offices with three audit lawsuits |  | 3 |
| 58 offices with two audit lawsuits |  | 2 |
| 189 offices with one audit lawsuit |  | 1 |
| 3,462 offices with zero audit lawsuits |  | 0 |
| 3,747 offices in total |  |  |
| Total number of audit lawsuits |  | 494 |

**Table 5**

**Descriptive statistics, correlation matrix, univariate tests, and regression results**

*Panel A: Descriptive statistics*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | Mean | Median | S.D. | Min | Max |
| *LOSSES1,2* | 0.059 | 0.000 | 0.474 | 0.000 | 27.000 |
| *GAINS1,2* | 0.043 | 0.000 | 0.603 | 0.000 | 48.000 |
| *Ln(LOSSES1,2)* | 0.032 | 0.000 | 0.175 | 0.000 | 3.332 |
| *Ln(GAINS1,2)* | 0.020 | 0.000 | 0.148 | 0.000 | 3.892 |
| *NETMOVES1,2* | −0.012 | 0.000 | 0.202 | −2.639 | 3.258 |
| *SUED1* | 0.500 | 0.500 | 0.500 | 0.000 | 1.000 |
| *SUED\_POACH1,2* | 0.010 | 0.000 | 0.099 | 0.000 | 1.000 |
| *SUED\_MEDIA1* | 0.169 | 0.000 | 0.375 | 0.000 | 1.000 |
| *SUED\_CONNECT1,2* | 0.077 | 0.000 | 0.266 | 0.000 | 1.000 |
| *LARGE1* | 0.772 | 1.000 | 0.420 | 0.000 | 1.000 |
| *LARGE2* | 0.453 | 0.000 | 0.498 | 0.000 | 1.000 |
| *SIMILARITY1,2* | 0.453 | 0.000 | 0.498 | 0.000 | 1.000 |
| *COMP* | 0.423 | 0.000 | 0.494 | 0.000 | 1.000 |
| *LOW* | 0.628 | 1.000 | 0.483 | 0.000 | 1.000 |

**Table 5 (cont.)**

**Descriptive statistics, correlation matrix, univariate tests, and regression results**

*Panel B: Correlation matrix*

|  |  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | *1.* | *2.* | *3.* | *4.* | *5.* | *6.* | *7.* | *8.* | *9.* | *10.* |
| *1.* | *NETMOVES1,2* | **1.000** | **−0.034** | **−0.034** | **−0.030** | **−0.031** | **−0.029** | **−0.028** | **−0.030** | −0.013 | 0.007 |
| *2.* | *SUED1* | **−0.038** | **1.000** | **0.100** | **0.452** | **0.288** | −0.003 | 0.000 | −0.002 | 0.013 | −0.007 |
| *3.* | *SUED\_POACH1,2* | **−0.038** | **0.100** | **1.000** | **0.053** | **0.025** | −0.004 | **0.029** | **0.020** | **−0.051** | **−0.052** |
| *4.* | *SUED\_MEDIA1* | **−0.029** | **0.452** | **0.053** | **1.000** | **0.236** | **0.059** | −0.001 | −0.003 | **0.079** | **0.052** |
| *5.* | *SUED\_CONNECT1,2* | **−0.028** | **0.288** | **0.025** | **0.236** | **1.000** | **0.082** | 0.006 | −0.001 | **0.048** | 0.007 |
| *6.* | *LARGE1* | **−0.023** | −0.003 | −0.004 | **0.059** | **0.082** | **1.000** | **−0.051** | **−0.051** | **0.260** | 0.010 |
| *7.* | *LARGE2* | **−0.026** | 0.000 | **0.029** | −0.001 | 0.006 | **−0.051** | **1.000** | **0.540** | −0.009 | −0.009 |
| *8.* | *SIMILARITY1,2* | **−0.025** | −0.002 | **0.020** | −0.003 | −0.001 | **−0.051** | **0.540** | **1.000** | 0.002 | 0.016 |
| *9.* | *COMP* | −0.011 | 0.013 | **−0.051** | **0.079** | **0.048** | **0.260** | −0.009 | 0.002 | **1.000** | **0.659** |
| *10.* | *LOW* | 0.011 | −0.007 | **−0.052** | **0.052** | 0.007 | 0.010 | −0.009 | 0.016 | **0.659** | **1.000** |

*Notes:* The Spearman (Pearson) correlations are reported in the upper (lower) diagonal. Statistically significant correlations are shown in **bold** (p-value < 0.05, two-tailed).

**Table 5 (cont.)**

**Descriptive statistics, correlation matrix, univariate tests, and regression results**

*Panel C: Univariate tests*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Non-sued offices | Sued offices without media exposure and poaching | Sued offices with media exposure | Sued offices with poaching |
|  | *SUED1* = 0 | *SUED1* = 1, *SUED\_MEDIA1* = 0, and  *SUED\_POACH1,2* = 0 | *SUED\_MEDIA1* = 1 | *SUED\_POACH1,2* = 1 |
|  | (1) | (2) | (3) | (4) |
| Mean value of  *NETMOVES1,2* | −0.004 | −0.015 | −0.024 | −0.089 |
|  |  |  |  |  |
| Pairwise tests: | *t-stats.* | *t-stats.* | *t-stats.* |  |
|  | (1) vs. (2) | (1) vs. (3) | (1) vs. (4) |  |
|  | −3.077\*\*\* | −4.482\*\*\* | −5.333\*\*\* |  |
|  |  |  |  |  |
|  | (2) vs. (3) | (2) vs. (4) | (3) vs. (4) |  |
|  | −1.648\* | −3.857\*\*\* | −2.056\*\* |  |

*Notes:* \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

**Table 5 (cont.)**

**Descriptive statistics, correlation matrix, univariate tests, and regression results**

*Panel D: Regression results (dependent variable = NETMOVES1,2)*

|  |  |  |
| --- | --- | --- |
|  | (1) | (2) |
| *SUED1* | −0.021\*\*\* | −0.014\* |
|  | (−3.057) | (−1.886) |
| *SUED\_POACH1,2* |  | −0.086\*\*\* |
|  |  | (−4.299) |
| *SUED\_MEDIA1* |  | −0.016\*\* |
|  |  | (−2.232) |
| *SUED\_CONNECT1,2* | −0.008 | −0.006 |
|  | (−0.857) | (−0.649) |
| *LARGE1* | 0.006 | 0.007 |
|  | (0.347) | (0.404) |
| *LARGE2* | −0.008 | −0.007 |
|  | (−1.155) | (−1.098) |
| *SIMILARITY1,2* | −0.006 | −0.006 |
|  | (−1.280) | (−1.320) |
| *COMP* | −0.002 | 0.002 |
|  | (−0.125) | (0.130) |
| Constant | 0.002 | −0.000 |
|  | (0.164) | (−0.026) |
|  |  |  |
| F-test (α2 = α3) |  | 10.57  (p = 0.00) |
|  |  |  |
| Office-pair observations | 14,120 | 14,120 |
| Year fixed effects | YES | YES |
| Office 1 fixed effects | YES | YES |
| Office 2 fixed effects | YES | YES |
| Adjusted *R*2 | 0.053 | 0.055 |

*Notes:* \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

**Table 6**

**Cross-sectional tests**

Client poaching by similarly-sized offices (Col. (1)), client poaching in city-markets with low versus high competition (Col. (2)), client poaching in city-markets with low versus high rates of audit litigation (Col. (3)), and client poaching when auditors make (do not make) payouts to the plaintiffs (Col. (4)).

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) |
| *SUED1* *× SIMILARITY1,2* | −0.025\*\*\* |  |  |  |
|  | (−3.711) |  |  |  |
| *SUED\_POACH1,2 × SIMILARITY1,2* | −0.101\*\* |  |  |  |
|  | (−2.492) |  |  |  |
| *SUED1* *× COMP* |  | −0.054\*\*\* |  |  |
|  |  | (−4.048) |  |  |
| *SUED\_POACH1,2 × COMP* |  | −0.242\*\*\* |  |  |
|  |  | (−4.966) |  |  |
| *SUED1* *× LOW* |  |  | −0.042\*\*\* |  |
|  |  |  | (−3.073) |  |
| *SUED\_POACH1,2 × LOW* |  |  | −0.119\*\*\* |  |
|  |  |  | (−2.945) |  |
| *SUED1* *× PAY1* |  |  |  | −0.014\* |
|  |  |  |  | (−1.905) |
| *SUED\_POACH1,2 × PAY1* |  |  |  | −0.215\*\*\* |
|  |  |  |  | (−5.298) |
| *SUED1* | −0.003 | 0.009 | 0.009 | −0.005 |
|  | (−0.407) | (0.954) | (0.857) | (−0.643) |
| *SUED\_POACH1,2* | −0.027 | −0.036 | −0.038 | 0.008 |
|  | (−0.906) | (−1.623) | (−1.449) | (0.294) |
| *SUED\_MEDIA1* | −0.016\*\* | −0.015\*\* | −0.015\*\* | −0.017\*\* |
|  | (−2.217) | (−2.058) | (−2.073) | (−2.234) |
| *SUED\_CONNECT1,2* | −0.006 | −0.010 | −0.008 | −0.002 |
|  | (−0.627) | (−1.123) | (−0.813) | (−0.223) |
| *LARGE1* | 0.008 | 0.008 | 0.004 | 0.011 |
|  | (0.459) | (0.498) | (0.247) | (0.632) |
| *LARGE2* | −0.008 | −0.008 | −0.008 | −0.006 |
|  | (−1.142) | (−1.141) | (−1.140) | (−0.875) |
| *SIMILARITY1,2* | 0.007 | −0.006 | −0.006 | −0.006 |
|  | (1.335) | (−1.255) | (−1.314) | (−1.209) |
| *COMP* | 0.002 | 0.031\* | −0.001 | 0.003 |
|  | (0.117) | (1.931) | (−0.089) | (0.216) |
| Constant | −0.007 | −0.014 | 0.005 | −0.007 |
|  | (−0.473) | (−0.946) | (0.329) | (−0.451) |
|  |  |  |  |  |
| Office-pair observations | 14,120 | 14,120 | 14,120 | 13,304 |
| Year fixed effects | YES | YES | YES | YES |
| Office 1 fixed effects | YES | YES | YES | YES |
| Office 2 fixed effects | YES | YES | YES | YES |
| Adjusted *R*2 | 0.056 | 0.058 | 0.056 | 0.055 |

*Notes:* \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

**Table 7**

**Results using alternative dependent variables**

Table 7 presents the results using alternative definitions of the dependent variable. Col. (1) uses the unlogged measure of client moves (*#NETMOVES1,2*). Col. (2) uses a percentage measure of client moves (%*NETMOVES1,2*). Col. (3) constructs *NETMOVES1,2* using the number of clients lost (gained) by office 1 to (from) office 2 in a three-year window surrounding a lawsuit being filed against office 1. Cols. (4) and (5) use client gains (*Ln(GAINS1,2)*) and client losses (*Ln(LOSSES1,2)*) as separate dependent variables. \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) | (5) |
| *Dep. Variables =* | *#NETMOVES1,2* | %*NETMOVES1,2* | *NETMOVES1,2* | *Ln(GAINS1,2)* | *Ln(LOSSES1,2)* |
| *SUED1* | −0.008 | 0.000 | −0.021\*\* | 0.010\* | 0.024\*\*\* |
|  | (−0.894) | (1.209) | (−2.179) | (1.839) | (3.864) |
| *SUED\_POACH1,2* | −0.082\*\*\* | −0.002\*\*\* | −0.062\*\*\* | −0.014 | 0.072\*\*\* |
|  | (−3.647) | (−3.648) | (−2.784) | (−1.010) | (4.374) |
| *SUED\_MEDIA1* | −0.021\*\* | −0.000\*\* | −0.015\* | −0.010\* | 0.007 |
|  | (−2.541) | (−2.004) | (−1.687) | (−1.907) | (1.091) |
| *SUED\_CONNECT1,2* | −0.009 | −0.000 | −0.016 | 0.006 | 0.012 |
|  | (−0.812) | (−1.104) | (−1.493) | (0.918) | (1.573) |
| *LARGE1* | 0.001 | 0.000 | 0.003 | 0.010 | 0.003 |
|  | (0.063) | (0.357) | (0.192) | (0.861) | (0.241) |
| *LARGE2* | −0.006 | −0.000 | 0.002 | −0.003 | 0.004 |
|  | (−0.798) | (−0.932) | (0.306) | (−0.667) | (0.768) |
| *SIMILARITY1,2* | −0.007 | −0.000 | −0.014\*\*\* | 0.017\*\*\* | 0.023\*\*\* |
|  | (−1.381) | (−1.433) | (−2.789) | (5.613) | (6.392) |
| *COMP* | 0.010 | −0.000 | −0.015 | 0.010 | 0.008 |
|  | (0.637) | (−0.329) | (−1.074) | (1.012) | (0.705) |
| Constant | −0.006 | −0.000 | 0.006 | −0.002 | −0.001 |
|  | (−0.367) | (−0.796) | (0.362) | (−0.154) | (−0.100) |
|  |  |  |  |  |  |
| Office-pair observations | 14,120 | 14,120 | 13,278 | 14,120 | 14,120 |
| Year fixed effects | YES | YES | YES | YES | YES |
| Office 1 fixed effects | YES | YES | YES | YES | YES |
| Office 2 fixed effects | YES | YES | YES | YES | YES |
| Adjusted *R*2 | 0.087 | 0.113 | 0.107 | 0.135 | 0.145 |

**Appendix A**

Appendix A uses an office-level analysis to test whether sued offices lose market share. Specifically, we estimate the following models where each office-year is the unit of observation (variable definitions are shown in Panel B of Table A):

*NETMOVES* = α0 + α1 *SUED* + α2 *SUED\_MEDIA* + α3 *SUED\_CONNECT* + α4 *LARGE* +

α5 *COMP* + Year FE + Office FE + u (A)

Consistent with our main tabulated analyses, *LOSSES* is the number of clients lost by the office in a two-year window (from year *t* to year *t+1*), and *GAINS* is the number of clients gained by the office in a two-year window. We measure the change in market share using the *NETMOVES* variable, which equals *Ln(GAINS)* minus *Ln(LOSSES)*.

We construct the sample by identifying 22,100 office-year observations with at least one audit engagement from 2000 to 2019. We drop 143 office-years pertaining to Arthur Andersen LLP. Following Swanquist and Whited (2015), we require each office to have at least one competitor in the city-market. This leaves a final sample of 17,520 office-years. Panel A of Table A reports the descriptive statistics for the office-year sample.

Panel B reports the regression results for eq. (A). Col. (1) shows a significant negative coefficient on *SUED* (*t-stat.* = −4.686), suggesting that sued offices lose more clients than non-sued offices. We add *SUED\_MEDIA* to Col. (2) and find a significant and negative coefficient on *SUED\_MEDIA* (*t-stat.* = −2.193), indicating that sued offices lose more clients when the lawsuit is covered in the media.

**Table A**

**Descriptive statistics and regression results when the unit of observation is the office-year**

*Panel A: Descriptive statistics*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | *Mean* | *Median* | *S.D.* | *Min* | *Max* |
| *LOSSES* | 1.047 | 0.000 | 2.628 | 0.000 | 67.000 |
| *GAINS* | 1.390 | 0.000 | 3.391 | 0.000 | 85.000 |
| *Ln(LOSSES)* | 0.426 | 0.000 | 0.639 | 0.000 | 4.220 |
| *Ln(GAINS)* | 0.527 | 0.000 | 0.703 | 0.000 | 4.454 |
| *NETMOVES* | 0.101 | 0.000 | 0.693 | −3.497 | 3.526 |
| *SUED* | 0.020 | 0.000 | 0.140 | 0.000 | 1.000 |
| *SUED\_MEDIA* | 0.007 | 0.000 | 0.085 | 0.000 | 1.000 |
| *SUED\_CONNECT* | 0.003 | 0.000 | 0.056 | 0.000 | 1.000 |
| *LARGE* | 0.579 | 1.000 | 0.494 | 0.000 | 1.000 |
| *COMP* | 0.501 | 1.000 | 0.500 | 0.000 | 1.000 |

**Table A (cont.)**

**Descriptive statistics and regression results when the unit of observation is the office-year**

*Panel B: Regression results (dependent variable = NETMOVES)*

|  |  |  |
| --- | --- | --- |
|  | (1) | (2) |
| *SUED* | −0.191\*\*\* | −0.131\*\*\* |
|  | (−4.686) | (−2.690) |
| *SUED\_MEDIA* |  | −0.163\*\* |
|  |  | (−2.193) |
| *SUED\_CONNECT* | 0.018 | 0.018 |
|  | (0.185) | (0.178) |
| *LARGE* | 0.078\*\*\* | 0.078\*\*\* |
|  | (4.784) | (4.771) |
| *COMP* | 0.017 | 0.017 |
|  | (0.835) | (0.795) |
| Constant | 0.051\*\*\* | 0.052\*\*\* |
|  | (3.301) | (3.336) |
|  |  |  |
| Office-year observations | 17,520 | 17,520 |
| Year fixed effects | YES | YES |
| Office fixed effects | YES | YES |
| Adjusted *R*2 | 0.158 | 0.158 |
| Variable definitions:  *Ln(LOSSES)* = Natural log of (one plus) *LOSSES*, where *LOSSES* is the number of clients lost by the audit office in a two-year window (from year *t* to year *t+1*).  *Ln(GAINS) =* Natural log of (one plus) *GAINS*, where *GAINS* is the number of clients gained by the audit office in a two-year window (from year *t* to year *t+1*).  *NETMOVES* = *Ln(GAINS) − Ln(LOSSES)*.  *SUED* = one if the audit office is sued in year *t*, and zero otherwise.  *SUED\_MEDIA* = one if the audit office is sued in year *t* *and* the lawsuit(s) is covered in the media, and zero otherwise.  *SUED\_CONNECT* = one if the audit office is sued in year *t* *and* the non-sued clients at the office have at least one board connection with the litigated client, and zero otherwise.  *LARGE* = one if the audit office is above or equal to the median office size (where office size is measured as the number of clients audited by the office), and zero otherwise.  *COMP* = one if the Herfindahl-Hirschman index of audit market concentration (HHI), measured as the sum of the squared market shares of all audit offices in a city-market, is below or equal to the median HHI value (where market share is measured as the percentage of the clients audited by the office), and zero otherwise. | | |

*Notes:* \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

**Appendix B**

Appendix B considers whether our inferences are robust to using restatements rather than audit lawsuits as the proxy for audit quality. Using the *Audit Analytics* database, we obtain 18,194 restatements from 2000 to 2019. We drop 2,385 restatements in which the restated firm is in a foreign country. We exclude 571 restatements not related to application failures of accounting principles, financial fraud, irregularities, and misrepresentations. We delete 374 restatements involving Arthur Andersen LLP. We require the restatements to be related to audited annual financial statements, which yields a final sample of 9,466 restatements. Among the 9,466 restatements, we find 2,214 (23.39%) in which the client switches from the restated auditor to another auditor, and 707 (7.47%) in which the restatement is announced in a press release.

Consistent with our main tabulated analyses (Figure 1), we construct a matched office-pair design. We then re-compute the *NETMOVES1,2* variable using the new sample.[[25]](#footnote-25) We test the poaching effect by estimating eq. (B):

*NETMOVES1,2* = α0 + α1 *RESTATED1* + α2 *RESTATED\_POACH1,2* + α3 *RESTATED\_MEDIA1* +

α4 *LARGE1* + α5 *LARGE2* + α6 *SIMILARITY1,2* + α7 *COMP* + Year FE + Office 1 FE +

Office 2 FE + u (B)

The *RESTATED1* variable takes the value one if office 1’s client announces a restatement in year *t*, and zero otherwise; *RESTATED\_POACH1,2* takes the value one if office 1’s client announces a restatement in year *t* *and* the restated client switches from office 1 to office 2, and zero otherwise; *RESTATED\_MEDIA1* takes the value one if office 1’s client announces a restatement in year *t* *and* the restatement is announced in a press release, and zero otherwise. Table B reports the descriptive statistics and regression results for eq. (B). The coefficient on *RESTATED\_POACH1,2* is negative and highly significant (*t-stat.* = −11.646), while the coefficient on *RESTATED\_MEDIA1* is also negative and significant (*t-stat.* = −2.036). Therefore, our inferences are similar whether we use audit lawsuits or accounting restatements as the proxy for audit quality.

**Table B**

**Descriptive statistics and regression results for accounting restatements**

*Panel A: Descriptive statistics*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | Mean | Median | S.D. | Min | Max |
| *LOSSES1,2* | 0.023 | 0.000 | 0.238 | 0.000 | 23.000 |
| *GAINS1,2* | 0.012 | 0.000 | 0.161 | 0.000 | 18.000 |
| *Ln(LOSSES1,2)* | 0.014 | 0.000 | 0.110 | 0.000 | 3.178 |
| *Ln(GAINS1,2)* | 0.007 | 0.000 | 0.078 | 0.000 | 2.944 |
| *NETMOVES1,2* | −0.006 | 0.000 | 0.132 | −3.178 | 2.944 |
| *RESTATED1* | 0.500 | 0.500 | 0.500 | 0.000 | 1.000 |
| *RESTATED\_POACH1,2* | 0.003 | 0.000 | 0.057 | 0.000 | 1.000 |
| *RESTATED\_MEDIA1* | 0.069 | 0.000 | 0.253 | 0.000 | 1.000 |
| *LARGE1* | 0.834 | 1.000 | 0.372 | 0.000 | 1.000 |
| *LARGE2* | 0.283 | 0.000 | 0.451 | 0.000 | 1.000 |
| *SIMILARITY1,2* | 0.325 | 0.000 | 0.468 | 0.000 | 1.000 |
| *COMP* | 0.440 | 0.000 | 0.496 | 0.000 | 1.000 |
| *LOW* | 0.606 | 1.000 | 0.489 | 0.000 | 1.000 |

**Table B (cont.)**

**Descriptive statistics and regression results for accounting restatements**

*Panel B: Regression results (dependent variable = NETMOVES1,2)*

|  |  |  |
| --- | --- | --- |
|  | (1) | (2) |
| *RESTATED1* | −0.004\*\* | −0.003\* |
|  | (−2.529) | (−1.713) |
| *RESTATED\_POACH1,2* |  | −0.120\*\*\* |
|  |  | (−11.646) |
| *RESTATED\_MEDIA1* |  | −0.005\*\* |
|  |  | (−2.036) |
| *LARGE1* | 0.007\*\* | 0.008\*\* |
|  | (2.443) | (2.525) |
| *LARGE2* | −0.006\*\*\* | −0.006\*\* |
|  | (−2.633) | (−2.541) |
| *SIMILARITY1,2* | −0.002 | −0.001 |
|  | (−0.918) | (−0.658) |
| *COMP* | −0.004 | −0.005 |
|  | (−1.159) | (−1.204) |
| Constant | −0.006\* | −0.006\* |
|  | (−1.874) | (−1.955) |
|  |  |  |
| Office-pair observations | 52,582 | 52,582 |
| Year fixed effects | YES | YES |
| Office 1 fixed effects | YES | YES |
| Office 2 fixed effects | YES | YES |
| Adjusted *R*2 | 0.063 | 0.066 |

*Notes:* \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

1. \* This paper has benefited from the comments of Eric Allen, Nathan Berglund, Jere Francis, Kenny Lin, Jaime Schmidt (discussant), Guochang Zhang, Yue Zhang and workshop participants at the Chinese University of Hong Kong (Shenzhen), University of Buffalo, the University of California at Riverside, Lingnan University, Northeastern University, the University of Connecticut, and the University of Illinois at Urbana-Champaign. We thank Joanna Ge and Jiyuan Li for providing research assistance. [↑](#footnote-ref-1)
2. When calculating the changes in market share for each audit office, we exclude client X’s switch from A to B because otherwise X’s switch would confound our test for poaching. [↑](#footnote-ref-2)
3. The national newspapers and newswires include the Wall Street Journal, The New York Times, The Times, Financial Times, The Washington Post, USA Today, Reuters News, Dow Jones News Service, Business Wire, and so on. Local newspapers include the Chicago Daily Herald, Chicago Tribune, Boston Globe, and so on. [↑](#footnote-ref-3)
4. The audit lawsuits sample is quite small. Therefore, we also follow Swanquist and Whited (2015) by using a much larger sample of restatements as an alternative proxy for audit failures. Similar to the lawsuits sample, the results in the restatements sample strongly support the poaching hypothesis (see Appendix B). [↑](#footnote-ref-4)
5. We also control for the possibility that auditor reputation effects might be driven by private communications among directors. Returning to the above example, a client’s board may learn about A’s alleged audit failure if one or more of its directors sit on the board of client X which is where the alleged audit failure occurred. Therefore, we test whether changes in market share are affected by board connections between an audit client and the client where the audit failure allegedly occurred. [↑](#footnote-ref-5)
6. To the extent that A’s clients switch to B because B is the only reasonable choice, we would expect the poaching results to be stronger in audit markets that are more concentrated because, in a concentrated market, there are fewer available options for A’s clients. To the contrary, we find the poaching effect is significantly weaker in more concentrated city-markets. This finding helps mitigate the concern that A’s clients are choosing B because they have limited other choices. [↑](#footnote-ref-6)
7. Lawsuit filings are available from databases such as Audit Analytics and PACER (the Public Access to Court Electronic Records), but accessing these documents is costly and time-consuming as the databases require a subscription. Consequently, it may be difficult for A’s non-sued clients and other auditors to find out about the lawsuit at client X. [↑](#footnote-ref-7)
8. Our results are robust to using a count of the number of lawsuits instead of the indicator variable *SUED1*. [↑](#footnote-ref-8)
9. For example, *SUED\_MEDIA1* equals one if office A is sued in year *t* and the lawsuit is covered in the media. The *SUED\_MEDIA1* variable equals zero for the matched non-sued office (C). In addition, the *SUED\_MEDIA1* variable equals zero for the sued office (A) if its lawsuit is not covered in the media. [↑](#footnote-ref-9)
10. We obtain similar results if we require the sued office (A) to be closely matched to the non-sued office (C) (i.e., we require the difference in office size between A and C to be below or equal to the median value). [↑](#footnote-ref-10)
11. There are 51 lawsuits in which the litigated client switches auditor, but we do not know the location of the incoming audit office. In such cases, we assume the client switched to the closest office of its incoming audit firm. [↑](#footnote-ref-11)
12. The last year in our lawsuit sample is 2019. We use one additional year – 2020 – to identify auditor changes in the subsequent year. [↑](#footnote-ref-12)
13. Companies are not required to disclose their reason for changing auditor. Of the 173 litigated clients that change auditor, we find only two companies (1.2%) that disclose in their 8-K filing a lawsuit against their former auditor. [↑](#footnote-ref-13)
14. Our main analysis includes both resignations and dismissals as auditor changes. Our results are robust to dropping auditor resignations and using just the dismissals to calculate net client moves (*NETMOVES1,2*). [↑](#footnote-ref-14)
15. Of the 494 audit lawsuits in our sample, there are 34 (6.9%) that also have an enforcement action by the SEC or PCAOB. In untabulated tests, the changes in audit office market shares are found to be insignificantly different for these 34 enforcement cases. In these untabulated tests, the *SUED\_POACH1,2* coefficient remains highly significant (t-stat. = −4.295). [↑](#footnote-ref-15)
16. We do not find any office that has been sued every year in our sample period (2000-2019). The most litigation-prone audit office in our sample is sued in nine out of the twenty sample years. [↑](#footnote-ref-16)
17. The mean value of *SUED\_POACH1,2* is small because a litigated client can choose only one office from all the offices in the city-market. For example, New York has 202 offices in total. A litigated client in New York would choose only one office out of the 202 offices that are available in New York. [↑](#footnote-ref-17)
18. We obtain similar results when we control for the poaching auditor (B) being sued during the previous five years. In addition, we find insignificant coefficients when we interact media coverage with the poaching variable (*SUED\_MEDIA1 × SUED\_POACH1,2*). Thus, we find no evidence that media coverage amplifies the effects of poaching. [↑](#footnote-ref-18)
19. Consistent with our earlier analysis, we exclude moves by the pseudo-X clients when we construct the new dependent variable (*NETMOVES1,2*) because otherwise there would be a mechanical association between *NETMOVES1,2* and *PSEUDO\_SUED\_POACH1,2*. [↑](#footnote-ref-19)
20. Here, audit market share is measured as the percentage of clients audited by the office. We obtain similar results when using audit fees, instead of the number of clients, to construct the *COMP* variable. [↑](#footnote-ref-20)
21. The main effect of *LOW* is subsumed by the office fixed effects (Office 1 FE and Office 2 FE). Therefore, the *LOW* indicator is not included as a separate variable in eq. (4). [↑](#footnote-ref-21)
22. We do not separately include *PAY1* because *PAY1* is equivalent to *SUED1* × *PAY1* given that a payout can only occur when the auditor is sued (*SUED1* = 1). [↑](#footnote-ref-22)
23. Hadfield (2004) explains that it can be difficult to identify lawsuit outcomes. Moreover, lawsuit outcomes are often missing from the *Audit Analytics* database. Wherever possible, we manually collect data on missing outcomes from the lawsuit complaint files and case histories. We supplement this manual data collection with searches of Google, Public Accounting Reports, and PACER (the Public Access to Court Electronic Records). [↑](#footnote-ref-23)
24. In this analysis, we exploit the fact that not all SEC registrants are listed on a stock exchange. [↑](#footnote-ref-24)
25. We exclude the restated client’s switch when calculating the changes in market share for each audit office. [↑](#footnote-ref-25)