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Audit seasonality and pricing of audit services: Theory and evidence from a meta-analysis

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ABSTRACT

Auditing is seasonal, with the majority of U.S. public companies having a December fiscal year-end. This results in an audit "busy season" and "off-season" with a non-trivial seasonal impact on the pricing of audit services. We apply an economic framework that explains how audit seasonality affects both the magnitude and the price elasticity of audit demand and audit supply. We find that the audit busy season is associated with an audit fee premium of approximately 10% based on a meta-analysis of 97 analyses from 18 audit fee studies of U.S public companies. A meta-regression of the contextual differences in research design between studies reveals that examining only Big N attenuates the busy season effect size but does not eliminate it, and that the busy season effect size may be larger post-SOX.

1. Introduction

Auditing is seasonal. This is because audit clients' choice of fiscal year-end is not uniformly distributed throughout the calendar year. In the U.S., the majority of public companies have a December fiscal year-end. This clustering of fiscal year-ends introduces audit seasonality to the auditing profession, resulting in an audit "busy season" and "off-season". This may affect the cost structure of audit firms, which in turn affects audit pricing, which is the subject of our study.

We use an economic framework that suggests that the differential pricing of audit services between the busy season and the off-season may be the result of two effects. First, the difference in demand for busy season and off-season audits gives rise to opportunities for audit service providers to engage in third-degree price discrimination, i.e. the ability to charge different prices to different groups of clients for similar services. Second, the capacity constraint experienced by audit firms during the busy season results in a relatively inelastic supply, raising the marginal cost of production and thus justifying higher audit fees. We conduct a meta-analysis of 97 analyses from 18 audit fee studies of U.S. public companies from 2005 to 2015, and find evidence in support of the framework that the audit busy season is associated with an audit fee premium of approximately 10%.

There are few archival studies in audit research that incorporate audit seasonality as a potential explanatory factor for audit fees. For example, Hay (2013, 174) notes that audit seasonality "... is frequently not included in audit fees studies, but [when it is] the evidence shows that it is significantly related to audit fees." However, even when archival studies recognize that audit seasonality may be an explanatory factor, this is often performed on an ad hoc basis with little or no theoretical justification (López & Peters, 2012). So while some studies recognize that audit seasonality has an impact, little space is devoted to why this is so.

The *price* of audit services transmits information between clients and auditors and allows for both the users and producers of audit services to be incentivized and guided by the information contained in those prices. Understanding how audit prices are determined is important because the audit price itself influences the organization, distribution and the production of audit services as well as the

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consumption of such services. In this respect, the distribution of fiscal year-ends as a recurring pattern within every calendar year that affects the consumption and production of audit services represents an important seasonality worthy of understanding, description, and potentially even quantification.

There are two prior meta-analysis studies that examine the commonly used independent variables in audit pricing research (Hay, 2013; Hay, Knechel, & Wong, 2006). While these two papers present wide-ranging meta-analysis of all audit fee studies, Hay (2013) acknowledges that they lack the depth of research that is required when the analysis is directed to individual issues. As such, our first overall contribution to the audit literature is the development of a theoretical framework for the causality between audit seasonality and audit pricing, thus providing the necessary research depth to this issue.

Our second contribution is to extend the time period coverage of the existing review literature concerning the busy season effect on audit pricing. In particular, we extend the meta-analysis research by including more recent studies (from 2005), which utilize underlying sample periods from 2000 to 2015. Furthermore, the two previous meta-analysis studies, as well as other reviews of audit archival research generally (DeFond & Zhang, 2014), have primarily been concerned with *compiling* and *summarizing* the findings of audit fee studies. Our third contribution is to *synthesize* these studies to *quantify* the average effect size of the busy season on audit fees across studies, and to examine if the contextual differences in research design between studies affect the results.

Our review of studies on U.S. public firms published after 2005 (covering a time period from 2000) shows that 75% of these studies show a statistically significant effect of busy season on audit fees. By combining the results from these studies we find that the audit fees charged by audit service providers during the busy season is approximately, and on average, 9.85% higher relative to the off-season. In addition, the busy season effect size is greater post-SOX. This result is in contrast to the findings of Hay et al. (2006, 177–178) and Hay (2013), in which only 25% of the individual studies reviewed identified a significant effect from the busy season. The authors conclude that the busy season effect disappeared after 1990. However, our result is consistent with surveys conducted by Sweeney and Summers (2002) and Persellin, Schmidt, and Wilkins (2015) that document a significantly increased workload for auditors during the busy season.

The remainder of this paper is organized as follows. In Section 2, we review why audit clients may choose a particular year-end date and how this leads to seasonality in the provision of audit services. In Section 3, we examine from a theoretical point of view the implications for demand and supply of audit services and how this affects the pricing of audit services. A description of the meta-analysis of recent empirical literature is provided in Section 4. Section 5 elaborates on our sample construction and presents the results of the meta-analysis. Section 6 provides some evidence on how heterogeneity in research design in these studies impacts the size of the busy season effect size, while Section 7 concludes.

2. Audit clients' choice of fiscal year-end

The fiscal year refers to the annual accounting period adopted by a business (Warren & Carl, 1993).[1] All businesses are required to have a fiscal year for tax purposes, and a fiscal year-end must be first specified when a firm is established. In the U.S., the majority of public companies have a December fiscal year-end.[2] A firm's choice of fiscal year-end may be influenced by factors such as business seasonality, regulation, and industry convention.

Some firms choose a fiscal year-end based on the natural cycle of their business (Huberman & Kandel, 1989; Smith & Pourciau, 1988). Specifically, firms that experience large seasonal variation in their sales activities may find it beneficial to choose the end of their busiest time as the fiscal year-end as this is the time when inventories are lowest (Huberman & Kandel, 1989). By avoiding the overlap of fiscal year-end and the peak of business activities, firms will be able to "coordinate conflicting demands for administrative resources" (Du & Zhang, 2013, 948).[3]

The choice of fiscal year-end is also governed by industry norms and convention. For instance, a December year-end appears to be the most popular choice in the software publishing industry (Sinha & Fried, 2008). Education services firms, on the other hand, commonly have a June year-end as it corresponds with the end of the school year (Sinha & Fried, 2008). Regulation also plays an important role in determining the fiscal year-end of firms in certain industries (Du & Zhang, 2013; Kamp, 2002).[4] Specifically, industries in which more than 90% of firms have December fiscal year-end are primarily from regulated or recently deregulated industries such as transportation, natural gas, banking and insurance industries (Du & Zhang, 2013; Smith & Pourciau, 1988).

[1] Firms usually have a 12-month reporting period. Nevertheless, this fiscal year convention introduces a comparability issue to some industries such as the retail industry. This is because the number of week and weekend days in any fiscal quarter vary from year to year and therefore it is problematic for retailers (which have most of their business activities on weekends) to complete within-year comparisons across quarters (Johnston, Leone, Ramnath, & Yang, 2012). Retailers are therefore recommended by the National Retail Federation to adopt a 52/53-week fiscal year because the number of days in a quarter is constant under this convention. The 52/53-week fiscal year convention is also common among manufacturing firms. This is because the fiscal year under this convention will always end on a given day of the week. For instance, manufacturing firms could choose to end on Friday under this convention in order to conduct inventory count over the weekend without disrupting their production (Sinha & Fried, 2008).

[2] This varies across countries. In South Pacific countries such as Australia and New Zealand, most public companies have a strong preference for a June fiscal year-end (López & Pitman, 2014). In Japan, most public companies have a March year-end (Kamp, 2002). Most continental European public companies, similar to U.S. companies, have a strong preference for December fiscal year-end (Kamp, 2002).

[3] Lehman Brothers changed its fiscal year-end from December to November in 1994 to shift "year-end administrative activities to a time period that conflicts less with the business needs of institutional customers" as documented in the *Transition Report Pursuant to Section 13 or 15(d) of the Securities Exchange Act of 1934, filed on February 28, 1995.*

[4] In most continental European countries in the European Union, the law assumes a December fiscal year-end for firms which do not have an explicit preference for their balance sheet date (Kamp, 2002).

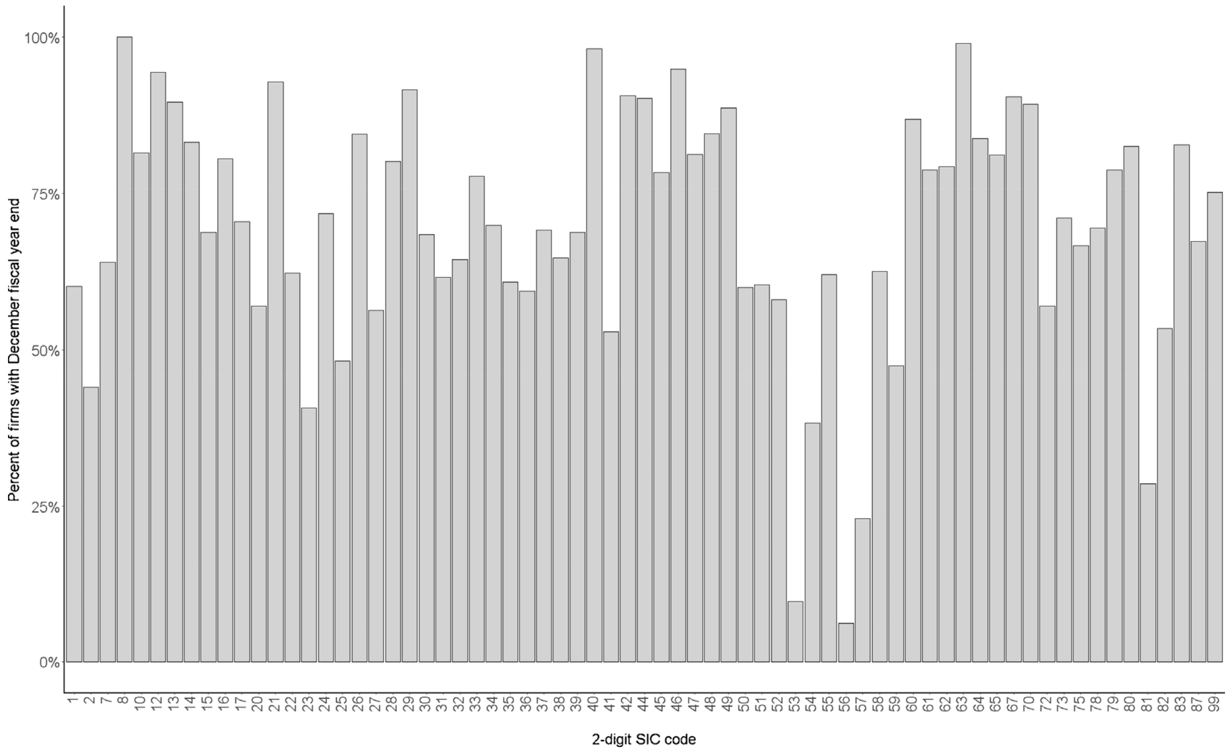

Fig. 1. Audit Seasonality by 2-Digit SIC code. The figure presents the percentage of firms with fiscal-year end in December for each 2-digit SIC code, with a minimum of 10 firm-years per SIC code. The sample consists of 73,201 firm-years from 2000 to 2015, with non-missing auditor values (COMPUSTAT: AU). In total, 73.7% of the sample has a December fiscal-year end.

Accessing the CRSP/Compustat Merged fundamental annual dataset of firm-years from 2000 to 2015, we find 73,201 firm-year observations with non-missing auditor values (COMPUSTAT: AU), of which 73.7% have a December fiscal-year end. This is consistent with earlier studies where approximately two-thirds to three-quarters of the U.S. public companies (non-financial) used in audit fee research samples close their fiscal year in December.[5]

Fig. 1 shows a bar plot of the percentage of firm-years between 2000 and 2015 that have a December fiscal-year end by industry (as proxied by 2-digit SIC). Some industries Exhibit 100% (or almost 100%) of firms with December fiscal-year ends, including SIC 8 (forestry), 40 (railroad transportation) and 63 (insurance carriers). Some have few firms with December fiscal-year ends, including 53 (general merchandise stores) and 56 (apparel and accessory stores). This may imply that there are some industry specific factors that determine the outcome of December year-ends or non-December year-ends. However, the remaining industries under study exhibit a reasonable proportion of firms that have December and non-December fiscal year-ends, which may imply that fiscal year-end is not entirely determined by industry specific factors, and that there is some element of within-industry choice for a substantial number of firms.

Once a firm has decided upon its fiscal year-end date, it may be difficult and costly to change. First, firms are required to apply to the Internal Revenue Service (IRS) to obtain an approval for the switch of fiscal year-end (Smith & Pourciau, 1988). Second, firms are also mandated by Rules 13a-10 and 15d-10 of the Securities Exchange Act 1934 to file a Form 8-K within 15 days of the fiscal year-end change decision. Third, firms are also required to file a transition report covering the transition period (Du & Zhang, 2013). The financial statement for the transition period must also be audited if it covers a period of more than six months. As such, firms may incur high costs because of these additional reporting and auditing requirements. Finally, a change in fiscal year-end is also likely to increase administrative costs as firms must adjust their accounting system to align with the new reporting period. These considerable one-off switching costs may potentially explain why firms rarely change their fiscal year-end date after it has been chosen upon establishment.

[5] For instance, López and Peters (2012) report that 64% of the firm-year observations within their sample had a December fiscal year end; Hribar, Kravet, and Wilson (2014) report 71.3% of the companies from 2000 to 2010; Fung et al. (2012) report 72.3% of the companies from 2000 to 2007; Gul and Goodwin (2010) report 73.6% of the companies from 2003 to 2006; Ball, Jayaraman, and Shivakumar (2012) report 72% from 2000 to 2007; Minutti-Meza (2013) report 71.4% from 2000 to 2008; Cahan, Godfrey, Hamilton, and Jeter (2008) report 62.7% from 2001 to 2004; Doogar et al. (2010) report 78% for 2005–2008; Francis, Reichelt, and Wang (2005) report 68.1% from 2000 and 2001; Francis and Wang (2005) report 83.9% for 2000–2001; Donohoe and Knechel (2014) report 68.2% from 2002 to 2010; Cassell, Drake, and Rasmussen (2011) report 73.5% over 2000–2008; Bentley, Omer, and Sharp (2013) report 64% from 2001 to 2009; Vermeer et al. (2008) report 71% for the year 2002; Doogar, Sivadasan, and Solomonreport (2015) 67% for 2001–2011; and Blankley et al. (2012) report 77.5% for 2004–2007. However, Cao, Myers, and Omer (2012), Chan, Chen, Chen, and Yu (2012) and Lobo and Zhao (2013) do not report descriptive statistics for the December year-end variable used.

The clustering of audit clients' fiscal year-end around December introduces seasonality into the auditing profession, dividing a calendar year into an audit "busy season" and an audit "off-season". The audit busy season (also known as "peak period") is the period when audit services are in high demand because an auditor's client portfolio is concentrated with clients of the same fiscal year-end (López & Peters, 2012). This affects both market demand and supply of audit services.

3. Implications for the demand and supply of audit services

For firms operating in industries where the majority of firms have a December year-end but where choice of year-end exists, we might consider that the observed fiscal year-end might reflect the elasticity of demand. Price elasticity of demand refers to how much the quantity demanded responds to changes in price (Gans et al., 2011). In a multi-period decision-making model, audit firms can respond to client firms' fiscal year-end clustering by raising audit fees. Client firms could in principle respond to this audit fee increase by choosing to switch to the non-busy season. However, audit cost is only one of the many and complex factors that firms consider in the original decision of fiscal year-end, and there are considerable costs associated with changing the fiscal year-end. This makes it rare for firms to change fiscal year-ends. While Fig. 1 indicates that there is some element of within-industry choice of fiscal year-ends, a large proportion of firms (73.7%) retain a December-year end despite the potential higher costs of these audits. This may suggest that December year end firms did not respond to higher audit costs, exhibiting a lower elasticity of demand for audit services compared to those with non-December year-ends.

In terms of price elasticity of supply of audit services, audit firms are presented with client ficrms' choice of fisal year-ends at equilibrium. Price elasticity of supply refers to how the quantity supplied responds to changes in price (Gans, King, & Mankiw, 2011). Audit firms have their own supply curves of audit services based on the costs of their factors of production. Audit services are inherently labor intensive (Palmrose, 1989) and auditor staff are perhaps the most important factor of production in audit firms. In the short run, audit staff capacity during the busy season cannot be increased beyond every audit staff member working at peak overtime.[6] As such, the busy season is a "bottleneck" for audit service providers as their capacities, resources, and labor are utilized to the fullest extent during this period, thus introducing an constraint (López & Peters, 2011). For an audit firm to have sufficient audit staff available to deploy during this busy season, they would need to have full-time staff employed and trained in anticipation of this busy season.[7]

One of the determinants of the price elasticity of supply of a service is how difficult or costly it is to acquire additional units of the inputs in delivering that service (Frank, Jennings, & Bernanke, 2012). Consequently, a more inelastic market supply curve of audit services implies a higher difficulty in acquiring additional inputs to deliver the audit services. As audit staff capacity has substantial slack capacity to accept new clients or additional non-audit work from existing clients during the off-season, this implies that supply is more elastic during the off-season. On the other hand, as audit staff capacity is constrained during the busy season, an audit firm may find it more difficult to accept new clients (Rubin, 1988) or additional work from existing clients during the busy season, implying that audit service supply is very inelastic during the busy season when capacity is reached.

The increased cost during the busy season could be due to out-of-pocket expenses (e.g. related to staff working overtime), implied costs such as lower audit quality,[8] or the opportunity costs of accepting a new client when there is no spare capacity to service the client.[9] The cost of labor in an auditing setting may act more like a fixed cost rather than a variable cost at the audit office level in the short run, even if audit staff labor is a variable cost at the audit engagement level.

An audit firm would maximize profit by matching marginal audit revenue from new clients or additional work from existing clients with their short run marginal costs regarding audit staff services during the off-season (where there is capacity slack) and the busy season (where there are constraints). While prices are set at the margin, the profit-maximizing price could be applied to all clients − existing and new − in terms of the audit charge out costs for additional audit work.[10] To some extent, audit service providers may be able to manage this supply constraint via several strategies. For instance, audit service providers could adopt "continuous auditing" strategies, adopt a "hard close" of their accounts before year end, or conduct more interim audit procedures instead of clustering all procedures at year-end (López & Peters, 2012). Nonetheless, seasonality will still prevail because some

[6] A survey study conducted by Sweeney and Summers (2002) on a public accounting firm finds that the average hours worked during off-season is 48.9 h per week, whereas during the busy season the hours worked increase to an average of 62.7 h per week. This prevalence of a busy season is also confirmed in a more recent survey conducted by Persellin et al. (2015), 3–4) where auditors note that the "…workweek during busy season is approximately 65 h, with an average maximum of 80 h. These numbers reveal that in an average busy season workweek, auditors work 10 h above the 55-h mandate in place at most firms, with a further increase to 25 h above the mandate during the busiest periods."

[7] Unlike unskilled laborers, audit staff need to be hired with relevant graduate and/or professional qualifications (Palmrose, 1989) and trained in the specific firm's audit procedures (Elliott, 1983).

[8] This can be observed from a significant body of literature on the effect of seasonality and thus workload compression on audit quality (e.g. Goodwin & Wu, 2016; Knechel & Payne, 2001; López & Peters, 2012; López & Pitman, 2014; Persellin et al., 2015; Sweeney & Summers, 2002).

[9] If an audit firm has "idle" capacity during the non-busy season, the audit firm might opt to take on an additional client whose majority of work falls into the non-busy season because there is a small opportunity cost, with no profit being foregone due to the additional work of servicing this client. On the other hand, if the audit firm is operating at full capacity during the busy season, and the audit firm opts to take on an additional client whose majority of work falls into the busy season, the audit firm would have to forego profit from other clients presently being serviced by doing less work for these other clients, thus earning less profit and perhaps sacrificing audit quality (which are both opportunity costs) in order to provide audit services to the additional client. Because the opportunity costs of servicing an additional client are different between clients with a December year-end and non-December year-end, the marginal costs are also different and this causes the supply curve to exhibit different elasticities between the busy season and non-busy season.

[10] It is impractical and unnecessary for our analysis to disentangle the impact of the effect, whether on new or existing clients. Moreover, all new clients would already have been taken on and are thus existing clients when observed in the data. As with the analysis for the demand of audit services by client firms, the existing clientele of audit firms represents their 'revealed preference' at equilibrium from their matching of marginal revenue with marginal costs for the busy and off-season.

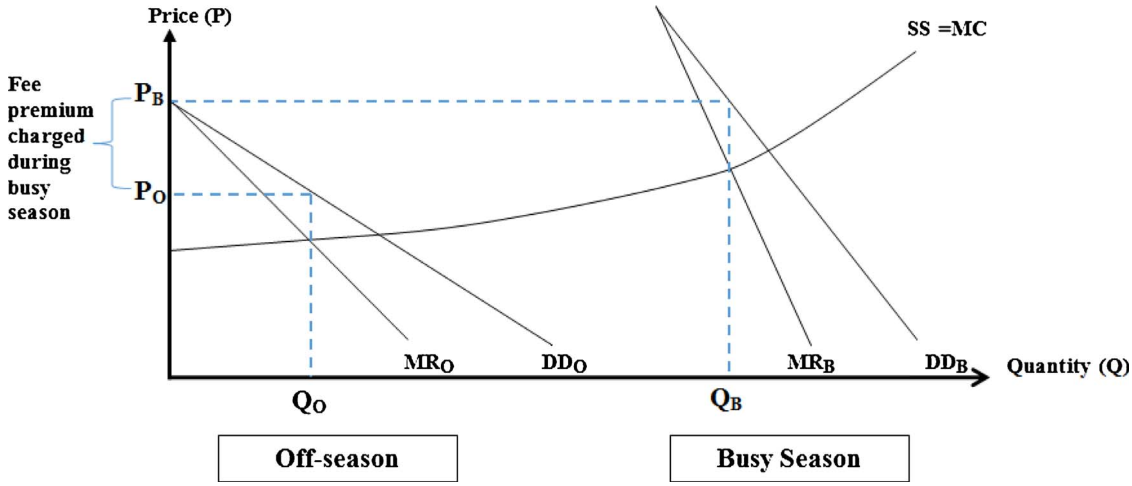

Fig. 2. Audit Seasonality effects on Audit Pricing. Notes to Fig. 2: SS represents the short run supply curve (SS), and is essentially a marginal cost curve (MC) (Frank et al. 2012). DDB (DDO) represents the market demand curve during the busy season (off-season). MRB (MRO) represents the marginal revenue curve during the busy season (off-season). Profit maximization output, QB/Q occurs when MR intersects MC.

procedures can only be conducted at the fiscal year-end (AICPA, 2006).[11] Indeed, the observation that audit staff have a substantially increased workload of more than 60 h during the busy season compared to 50 h pre-busy season and the 55 h mandate, which far exceeds the standard 40 h work week (Persellin et al., 2015; Sweeney & Summers, 2002), is evidence that the busy season has not been eliminated and that auditors face a non-uniform workload throughout the year.

A graphical illustration of the previous discussion regarding how audit pricing is determined by market demand and supply of audit services during the busy season and off-season is presented in Fig. 2. It illustrates the intersection of market demand (DD) and supply (SS = MC) curves of audit services with the corresponding audit fees (P) charged by audit service providers. The demand curve by client firms during the busy season (off-season) is represented by the DD_B (DD_O) demand curve. The busy season is potentially more inelastic and the demand curve is drawn with a steeper slope (DD_B) relative to the off-season (DD_O). The audit supply curve (SS) is derived from the marginal cost curves at each point of supplying an additional unit of audit service quantity (x-axis). The marginal cost of production is high during the busy season as audit service providers face higher opportunity costs during this period as audit firm resources are fully utilized. The supply curve is therefore relatively more inelastic during the busy season in comparison to the off-season, as illustrated by the relatively steeper supply curve in Fig. 2.

While audit service providers provide audit services at the level of output, Q (where MR = MC), they charge audit fees corresponding to the market demand (DD) curve, P.[12] During the busy season, the market demand for audit services is relatively higher (DD_B) and potentially more inelastic in comparison to the off-season. Consequently, audit service providers will supply at Q_B and charge a relatively higher fee at $P_{B,}$ as clients are less sensitive to fee changes. In comparison, during the off-season, the demand schedule of client firms (DD_O) reflects the lesser and more inelastic demand for audit services during the off-season. The lower marginal cost of providing audit services and the marginal revenue to the audit firm (MR_O) results in the supply of audit services of quantity $Q_{O,}$ and the audit fee price P_O charged, which is lower than that for the busy season P_B. The difference between busy season and off-season fees (i.e. $P_B - P_O$) is therefore the fee premium charged by audit service providers during the busy season.

Summarizing our analysis above, the seasonal variation in market demand and supply of audit services may affect the pricing of audit services (Frank et al., 2007). Therefore, the differential pricing of audit services between the busy season and off-season may be the result of two combined effects. First, the difference in demand for busy season and off-season audits gives rise to opportunities for audit service providers to engage in third-degree price discrimination. That is, the ability to charge different prices to different groups of clients for similar services.[13] Second, the capacity constraints faced by audit firms during the busy season results in a relatively inelastic supply, raising the marginal cost of production, which in turn justifies higher audit fees.

[11] Year-end evidence is also necessitated by certain auditing standards to minimise audit risk (AICPA, 1983).

[12] Economic theory suggests that the profit maximisation output occurs when marginal revenue (MR) equals marginal cost (MC). MR refers to the change in total revenue when one additional unit of output is sold whereas MC refers to the change in total cost from selling an additional unit of output.

[13] Third-degree price discrimination refers to the practice of charging different groups of customers different prices for similar goods/services (Phlips, 1988). There are three conditions that are necessary for such price discrimination which occurs in the auditing market: (1) the price elasticity of demand is different during the busy season and off-season; (2) audit service providers are able to separate their clients into busy season and off-season clients (based on clients' fiscal year-end date); and (3) clients are unlikely to switch from the busy season to off-season due to high switching costs such as additional filing and administrative costs (Allen, Weigelt, Doherty, & Mansfield, 2013; Du & Zhang, 2013; Varian, 1989). While audit service providers need to enjoy some market power in order to price discriminate, this does not suggest that audit fees are determined free of competitive pressures. Indeed, the audit market has been described as contestable with price competition (i.e. Simunic, 1980) and it appears that the existence of competitive pressure on audit fees has been prevalent in recent times (Ettredge, Fuerherm, & Li 2014). Nevertheless, and bar the special case where a market can be best described as perfectly competitive, competitive pressure does not necessarily preclude price discrimination; in some cases competitive pressures may instead increase the offering of different prices to readily identifiable groups of consumers even though the goods/services offered are similar (Borzekowski, Thomadsen, & Taragin, 2009).

4. Meta-analysis

Few auditing studies incorporate the busy season as a control variable in their audit fee models for public companies despite its potential explanatory power. In addition, the few archival studies that include the busy season as an explanatory/control factor for audit fees often do so from an ad hoc perspective with little or no theoretical justification (López & Peters, 2012). There are two prior meta-analysis studies of the various commonly used independent variables in audit pricing research. Hay et al. (2006) examine publications during the period 1977–2003, and Hay (2013) extends the earlier study to include publications in the 2004–2007 period and also additional countries that were previously not included. Out of 147 analyses included in Hay et al. (2006), 32 control for audit seasonality, but only five of these individual studies show a positive and statistically significant ($p < 0.05$) effect of the busy season on audit fees. Out of 313 studies, Hay (2013) finds that 83 studies control for audit seasonality, and 19 studies show a significant and positive association between audit fees and the busy season. The varying results with regard to significance across the individual analysis may be explained by the settings of the studies as well as sampling variation.[14] Nevertheless, based on their meta-analysis results testing the direction and statistical significance of the effects, both Hay et al. (2006) and Hay (2013) conclude that audit seasonality should be included in more studies. Our meta-analysis differs, in that: (1) we define our population of studies to be those focusing on U.S. public firms; and (2) we are primarily interested in assessing the busy season's effect size on audit fees, i.e. we aggregate the results from U.S. focused studies to quantify the economic significance of the busy season on U.S. public firms.

To assess the magnitude of the 'busy season' audit fee premium, we conduct a straightforward meta-analysis based on the method devised by Hunter and Schmidt (Hunter & Schmidt, 2000; Hunter et al., 1982). The Hunter-Schmidt approach to meta-analysis is to estimate the true effect sizes in the population by averaging all the effect sizes estimated in the samples across the identified studies, weighted by their sample size as an indication of accuracy (Hunter & Schmidt, 2000, 2004). As noted by Khlif and Chalmers (2015), this method has also been used in auditing research (see Trotman & Wood, 1991), but is somewhat different to the more common Stouffer's approach (Habib, 2012, 2013; Kinney & Martin, 1994; Lin & Hwang, 2010).

More formally, the Hunter-Schmidt method first combines the effect sizes (r) from prior studies and computes a weighted mean of the effect sizes ($\bar{r}$), where the weight used is simply the sample size (n):

$$\bar{r} = \frac{\sum_{i=1}^{k} n_i r_i}{\sum_{i=1}^{k} n_i} \tag{1}$$

Hunter and Schmidt (2004) further argue that the variance across sample effect sizes consists of: (1) the variance of effect sizes in the population; and (2) the sampling error. The variance in population effect sizes can therefore be estimated by correcting the variance in sample effect sizes by the sampling error. Formally, the first step is to calculate the variance of sample effect sizes by taking the frequency weighted average squared error:

$$\sigma_r^2 = \frac{\sum_{i=1}^{k} n_i (r_i - \bar{r})^2}{\sum_{i=1}^{k} n_i} \tag{2}$$

The second step is to calculate the sampling error variance:

$$\sigma_e^2 = \frac{(1-\bar{r}^2)^2 K}{\bar{N}-1} \tag{3}$$

where $\bar{r}$ is the weighted mean of the effect sizes and $\bar{N}$ is the mean sample size. The variance in the population effect sizes is then simply estimated by subtracting the sampling error variance from the variance in sample effect sizes:

$$\hat{\sigma}_p^2 = \sigma_r^2 - \sigma_e^2 \tag{4}$$

This allows us to construct *credibility intervals*[15] for the weighted average 'busy season' audit fee premium. A credibility interval can be constructed by taking the mean effect size ($\bar{r}$) and adding or subtracting from it the square root of the estimated population variance multiplied by an appropriate (asymptotic) critical value from the *t* distribution.

$$Credibility\ \ Interval_{Upper} = \bar{r} + t\sqrt{\hat{\sigma}_p^2} \tag{5}$$

$$Credibility\ \ Interval_{Lower} = \bar{r} - t\sqrt{\hat{\sigma}_p^2} \tag{6}$$

[14] The 147 audit fee analyses included in Hay et al. (2006) and the 313 studies in Hay (2013) includes analysis conducted on both U.S. and non U.S. jurisdictions, public and non-public entities, for-profit and not-for-profit firms, as well as government and non-government organizations. This raises some concern about whether these studies are comparable in terms of the effect of seasonality on audit fees or whether they do in fact estimate different population parameters. In addition, the average and median sample size in Hay et al. (2006) and Hay (2013) are 423 and 216 observations, respectively. Hay (2013) reports that the mean number of observations increased to 1539 in studies published post 2004. Larger sample analysis is more precise and has more statistical power compared to smaller samples. Consequently, for a given sample effect size, the larger the sample size, the lower the standard errors. Therefore, larger samples are likely to report statistically significant effects in the population.

[15] A credibility interval should not be confused with a confidence interval as their interpretation is somewhat different.

Thus, for the 99%, 95%, 90% and the 80% credibility intervals the appropriate critical values for *t* would be 2.576, 1.96, 1.645 and 1.282, respectively.

In essence, the Hunter-Schmidt method is a random effects meta-analysis that assumes that the effect sizes based on large samples are more likely to reflect the population accurately than those based on small samples. Beyond the convenience of the summary statistic that this method provides, its great advantage lies in the increase in power that follows from the aggregation of studies. In other words, the precision and accuracy of estimates can be improved with more underlying studies.

5. Sample and evidence

To systematically identify archival studies to include in the meta-analysis, we focus on articles that are published in the leading journals identified for the database of Audit Research prepared by the AAA Auditing Section Research Committee.[16] There are eight journals that are identified as leading journals for audit research: *AOS*, *AJPT*, *BRIA*, *CAR*, *JAE*, *JAPP*, *JAR*, and *TAR*. However, not all of these have an archival focus.[17] Furthermore, we focus only on conceptually similar studies and scan the journals for archival auditing articles published between 2000 and 2015[18] that includes the effect of seasonality on audit fees and fit the following additional requirements: (1) the observations regarding engagement audit fees must be drawn from U.S. public firms[19]; (2) the operationalization of fees in the OLS regressions must be in the form of the natural log of audit fees[20]; and (3) one of the independent variables must denote the busy/non-busy season using a dummy variable, and the coefficient for this variable must be disclosed.18

Where a given article tests the relation between seasonality and audit fees using different samples/sub-samples, or with different model specifications − but which still meet the requirements above − the coefficients on the busy season from those tests are included in our meta-analysis as separate estimates of effect sizes.[21] All of the selected studies model audit fees for U.S. public companies using a number of explanatory factors and include audit seasonality as a control variable, but not necessarily as the variable of interest. Because audit seasonality is not the variable of interest in any of these studies, but rather included in the audit fee models on an ad hoc basis as a control variable, this will to some extent mitigate the impact of publication bias from any results with respect to audit seasonality. That is, because the specific results with respect to the busy season are not relevant to the conclusions of the identified papers, we do not expect there to be a publication bias with regard to the busy season effect result either. We therefore do not explicitly test for any publication bias because we consider it unlikely to affect our analysis. Similarly, because we focus only on U.S. public companies, we do not expect there to be a large effect due to the exclusion of non-English language studies.

Table 1 illustrates the results of the meta-analysis on prior audit fee studies where the dependent variable is the natural logarithm of audit fees. We identify 18 studies containing 97 disclosed estimates of the effect size of seasonality on audit fees that satisfy our inclusion requirements. The average sample size is 9873 observations but there is wide variability: the effect size reported from the smallest sample is based upon only 238 observations whereas the effect size from the largest sample is based upon 54,545 observations. In aggregate, the identified 97 effect size estimates are based on a total of 957,712 ob-

[16] By focusing on leading journals with a reputation for quality publications, we hope to increase the chance of including studies that are methodologically sound in our meta-analysis. This is a practice generally known as "best evidence synthesis" (Slavin, 1986). This is important because badly designed studies will result in bad statistics irrespective of the quality of the meta-analysis. This is not to say that all studies published in other journals are necessarily of poor quality; instead it acknowledges that the review and publication process at the identified journals are considered to be of high quality by consensus, and as such the likelihood for substandard studies appearing in these journals are lower.

[17] The following abbreviations are used: AOS − Accounting, Organization and Society; AJPT − Auditing: A journal of Practice and Theory; BRIA − Behavioural Research in Accounting; CAR − Contemporary Accounting Research; JAE − Journal of Accounting and Economics; JAPP − Journal of Accounting and Public Policy; JAR- Journal of Accounting Research; TAR- The Accounting Review.

[18] Because we focus on U.S. archival studies, for efficiency reasons we limit our search for articles published after 2000 when audit fees on audit engagements became publically available through disclosures in annual reports.

[19] For effect size estimates to be meaningfully compared across studies, it is necessary that all effect sizes estimate the same population parameter. We focus only on studies that have been conducted on U.S. public firms as this limits the institutional differences between the studies we aggregate to ensure a meaningful estimate of the effect size. Studies that focus on non-public or non-U.S. firms may lead to different definitions of the relevant populations and as such estimate different population parameters.

[20] For effect size estimates to be meaningfully compared across studies, it is also necessary that all effect sizes have the same scale, otherwise studies with different operationalizations of the dependent variable or the independent variable may produce different treatment effects (Cortina & DeShon, 1998; Hunter & Schmidt, 2004). Consequently, we limit ourselves to studies that use natural log of audit fees as a dependent variable and a dummy variable for seasonality as an independent variable in an OLS regression as this ensures that the coefficients across the studies are comparable. That is, the exponential of the coefficient on the dummy variable for seasonality minus one measures the percentage change in mean audit fees between the busy and off-season. The reason for focusing on audit fees and not fees from non-audit services is because seasonality is likely to specifically result in constraints on audit firms in respect of their auditing work, not necessarily regarding their non-audit work, which may have the flexibility to be performed at other times.

[21] We chose to implement this as an objective selection criteria, rather than subjectively select what we believe is the best estimate from each paper. Admittedly, in some cases this results in almost identical effect size estimation from the same study due to relatively small model variation between estimates (e.g. Gul & Goodwin, 2010) or due to relatively small sample variation (e.g. Doogar, Sivadasan, & Solomon, 2010) within the same study. As such, the effect size estimations may not be completely independent of each other, but on the other hand all estimates provide incremental information to some degree or another. However, we choose to exclude any *firm-fixed* effect analysis − whether this is estimated by first-differencing, demeaning or by dummy variables − because the information between firms, and not only within firms, is likely to be required to obtain a stable estimate of the busy season effect size. In other words, there are likely to be too few observations that *change* their year-end to obtain reliable and precise estimates of within firm fixed effects. Consequently, we exclude three effect size estimates from Cassell, Drake, and Rasmussen (2011); two effect size estimates from Francis and Wang (2005), and four effect size estimates from Ball et al. (2012).

Table 1
Meta-analysis of the Busy Season Audit Fee Premium Effect Size.

Authors	Year	Journal[c]	Sample Size (n)	Coef. Size (r)	Significant[e]	n x r
Publications						
Francis et al.[a,b]	2005	TAR	3994	0.032	no	127.81
Francis et al.[a,b]	2005	TAR	3045	0.003	no	9.14
Francis et al.[a,b]	2005	TAR	3045	0.005	no	15.23
Francis et al.[a,b]	2005	TAR	3838	0.040	yes	153.52
Francis et al.[a,b]	2005	TAR	2902	0.014	no	40.63
Francis et al.[a,b]	2005	TAR	2902	0.015	no	43.53
Francis and Wang[a]	2005	AJPT	2123	0.019	no	40.34
Francis and Wang[a]	2005	AJPT	2123	0.025	no	53.08
Vermeer et al. [a]	2008	AJPT	575	0.240	yes	138.00
Vermeer et al. [a]	2008	AJPT	288	0.320	yes	92.16
Vermeer et al. [a]	2008	AJPT	287	0.160	yes	45.92
Cahan et al. [a]	2008	TAR	560	0.288	yes	161.28
Cahan et al. [a]	2008	TAR	560	0.161	yes	90.16
Cahan et al. [a]	2008	TAR	560	0.149	yes	83.44
Doogar et al.[a]	2010	JAR	938	0.109	yes	102.24
Doogar et al.[a]	2010	JAR	1075	−0.004	no	−4.30
Doogar et al.[a]	2010	JAR	1010	0.021	no	21.21
Gul and Goodwin[a,b]	2010	TAR	2826	0.230	yes	649.98
Gul and Goodwin[a,b]	2010	TAR	728	0.190	yes	138.32
Gul and Goodwin[a,b]	2010	TAR	710	0.670	yes	475.70
Gul and Goodwin[a,b]	2010	TAR	719	0.010	no	7.19
Gul and Goodwin[a,b]	2010	TAR	669	0.050	no	33.45
Gul and Goodwin[a,b]	2010	TAR	2826	0.230	yes	649.98
Gul and Goodwin[a,b]	2010	TAR	728	0.180	yes	131.04
Gul and Goodwin[a,b]	2010	TAR	710	0.670	yes	475.70
Gul and Goodwin[a,b]	2010	TAR	719	0.010	no	7.19
Gul and Goodwin[a,b]	2010	TAR	669	0.050	no	33.45
Cassell et al.[a]	2011	CAR	33,991	0.013	no	441.88
Cassell et al.[a]	2011	CAR	28,212	0.099	yes	2792.99
Cassell et al.[a]	2011	CAR	28,212	0.099	yes	2792.99
Publications						
Authors	Year	Journal[c]	Sample Size (n)	Coef. Size (r)	Significant[e]	n x r
Bentley et al. [a]	2012	CAR	11,837	0.084	yes	994.31
Bentley et al. [a]	2012	CAR	11,837	0.086	yes	1017.98
Bentley et al. [a]	2012	CAR	11,147	0.086	yes	958.64
Cao et al.[a]	2012	CAR	4846	0.158	yes	765.67
Cao et al.[a]	2012	CAR	4244	0.174	yes	738.46
Blankley et al.	2012	AJPT	5978	0.020	no	119.56
Fung et al. [a,b]	2012	TAR	17,207	0.178	yes	3062.85
Fung et al. [a,b]	2012	TAR	4235	0.089	yes	376.92
Fung et al. [a,b]	2012	TAR	12,972	0.199	yes	2581.43
Fung et al. [a,b]	2012	TAR	17,207	0.177	yes	3045.64
Fung et al. [a,b]	2012	TAR	4235	0.090	yes	381.15
Fung et al. [a,b]	2012	TAR	12,972	0.198	yes	2568.46
Ball et al. [a]	2012	JAE	44,883	0.075	yes	3366.23
Ball et al.[a]	2012	JAE	8869	0.143	yes	1268.27
Ball et al.[a]	2012	JAE	8869	0.114	yes	1011.07
Ball et al.[a]	2012	JAE	7342	0.124	yes	910.41
Ball et al. [a]	2012	JAE	8869	0.182	yes	1614.16
Ball et al.[a]	2012	JAE	8869	0.140	yes	1241.66
Ball et al.[a]	2012	JAE	8869	0.109	yes	966.72
Ball et al.[a]	2012	JAE	7342	0.159	yes	1167.38
Ball et al.[a]	2012	JAE	18,093	0.096	yes	1736.93
Ball et al.[a]	2012	JAE	18,094	0.079	yes	1429.43
Ball et al. [a]	2012	JAE	5858	0.154	yes	902.13
Ball et al.[a]	2012	JAE	2978	0.101	yes	300.78
Ball et al.[a]	2012	JAE	5858	0.113	yes	661.95
Ball et al.[a]	2012	JAE	2978	0.090	yes	268.02
Ball et al.[a]	2012	JAE	4992	0.125	yes	624.00
Ball et al.[a]	2012	JAE	2332	0.112	yes	261.18
Ball et al.[a]	2012	JAE	9172	0.051	yes	467.77
Ball et al.[a]	2012	JAE	2280	0.053	yes	120.84
Ball et al.[a]	2012	JAE	2280	0.049	yes	111.72

(continued on next page)

Table 1 (*continued*)

Publications						
Authors	Year	Journal[c]	Sample Size (n)	Coef. Size (r)	Significant[e]	n x r
Ball et al.[a]	2012	JAE	1897	0.08	yes	151.76
Ball et al.[a]	2012	JAE	44,883	0.018	no	807.89
Ball et al.[a]	2012	JAE	8869	0.084	no	745.00
Ball et al.[a]	2012	JAE	8869	0.078	no	691.78
Ball et al.[a]	2012	JAE	7342	0.102	no	748.88
Ball et al.[a]	2012	JAE	44,883	0.136	yes	6104.09
Ball et al.[a]	2012	JAE	8869	0.185	yes	1640.77
Ball et al.[a]	2012	JAE	8869	0.087	yes	771.60
Ball et al.[a]	2012	JAE	7342	0.108	yes	792.94
Ball et al.[a]	2012	JAE	8869	0.079	no	700.65
Ball et al.[a]	2012	JAE	8869	0.074	no	656.31
Ball et al.[a]	2012	JAE	7342	0.171	no	1255.48
Chan et al.[b]	2012	JAE	15,157	0.167	yes	2531.22
Lobo and Zhao	2013	TAR	32,915	0.094	yes	3094.01
Minutti-Meza[a,b]	2013	JAR	24,279	0.044	yes	1068.28
Minutti-Meza[a,b]	2013	JAR	5960	0.094	yes	560.24
Minutti-Meza[a,b]	2013	JAR	5906	0.096	yes	566.98
Minutti-Meza[a,b]	2013	JAR	16,388	0.06	yes	983.28
Minutti-Meza[a,b]	2013	JAR	9626	0.087	yes	837.46
Minutti-Meza[a,b]	2013	JAR	9710	0.077	yes	747.67
Donohoe and Knechel[a]	2014	CAR	32,315	0.093	yes	3005.30
Donohoe and Knechel[a]	2014	CAR	19,208	0.151	yes	2900.41
Donohoe and Knechel[a]	2014	CAR	13,107	0.003	no	39.32
Hribar et al. [b]	2014	RAST	54,545	0.05	yes	2727.25
Doogar et al.[a]	2015	RAST	23,943	0.103	yes	2466.13
Doogar et al.[a]	2015	RAST	9802	0.062	yes	607.72
Doogar et al.[a]	2015	RAST	19,440	0.093	yes	1807.92
Doogar et al.[a]	2015	RAST	7576	0.071	yes	537.90
Doogar et al.[a]	2015	RAST	19,202	0.102	yes	1958.60
Doogar et al.[a]	2015	RAST	7122	0.067	yes	477.17
Doogar et al.[a]	2015	RAST	238	0.219	yes	52.12
Publications						
Authors	Year	Journal[c]	Sample Size (n)	Coef. Size (r)	Significant[e]	n x r
Doogar et al.[a]	2015	RAST	454	0.092	no	41.77
Doogar et al.[a]	2015	RAST	345	0.092	yes	31.74
Doogar et al.[a]	2015	RAST	19,440	0.102	yes	1982.88
Doogar et al.[a]	2015	RAST	7576	0.069	no	522.74
Doogar et al.[a]	2015	RAST	7467	0.066	yes	492.82
Sum			957,712			89,992.-99
Weighted mean effect size			(Sum of (n x r)/ Sum of n)			0.09397
Weighted mean effect size in percent[d]						9.85%

Notes to Table 1: [a] These are multiple samples which are taken from the same study with the identified authors. In all of these samples, a different coefficient estimate (β_{busy}) is expected because (i) the audit fee model differs in some respect or (ii) the number of sample observations are different due to some form of sample exclusion criteria. Because of the arbitrariness concerning which analysis to include, we include all of them, to the extent they meet the inclusion criteria as specified in this paper.

[b] These studies code "0" as busy season and "1" as off season. For the purpose of consistency with other studies that code the variable otherwise, the coefficients of the busy season variable of these studies are inverted and recorded as β_{busy} as shown in the table above.

[c] The studies are sampled from well-recognized journals. Journals abbreviated as: JAE-Journal of Accounting and Economics; TAR-The Accounting Review; JAR-Journal of Accounting Research, CAR-Contemporary Accounting Research; RAST-Review of Accounting Studies; and AJPT-Auditing: A Journal of Practice and Theory.

[d] Weighted effect size of busy period effects on audit fees is computed as $9.85\% = e^{0.09397}-1$.

[e] Significance is reported as in the respective analysis at the conventional 5% level, irrespective of whether the authors choose to use one-tailed or two-tailed tests.

Table 2
Credibility intervals for the effect size.

Panel A: Variance (standard deviations)		Variance	(St. dev.)
Variance (standard deviation) of sample effect sizes (σ_r^2):		0.003	(0.054)
Variance (standard deviations) in sampling error (σ_e^2):		0.000	(0.010)
Variance (standard deviations) in the population effect sizes (σ_p^2):		0.003	(0.053)
Panel B: Credibility intervals	**Lower Bound**	**Upper Bound**	**Range**
99% credibility interval	−4.22%	25.99%	30.21pp
95% credibility interval	−1.03%	21.93%	22.95pp
90% credibility interval	0.65%	19.90%	19.25pp
80% credibility interval	2.61%	17.61%	15.00pp

servations.[22] All effect sizes are positive (as expected) except for one (which is also insignificant) indicating that the busy season is associated with higher fees. However, 25 of the effect size estimates − about a quarter of the studies − are reported as not significantly different from zero at conventional levels (5% level), with the remaining 72 effect size estimates are reported to be statistically significant. Interestingly, this is almost opposite to the results reported in Hay et al. (2006) and Hay (2013), in which a significant busy season effect is only found in less than 25% of existing studies that include a busy season variable. As our sample of papers dates from 2005, with underlying data from 2000, this is in direct contrast to Hay et al. (2006, 177–178) who conclude that the busy season effect almost disappeared after 1990, at least for U.S. public firms.

Furthermore, by viewing these effect sizes in Table 1 as imperfect estimates of the true effect size of the busy period on audit fees, synthesizing the effect sizes from these studies will increase the statistical power and as such help us avoid Type II errors or false negatives (i.e. not finding an effect when one exists). Indeed, it is this important point that motivates most meta-analysis (including ours) and enables us to draw generalizable conclusions despite the existing heterogeneity across empirical audit fee research studies (Financial Reporting Council, 2012).

Despite the variability in the effect sizes, statistical significance, and sample sizes across these studies, aggregating the result using Eq. (1) above indicates a weighted mean coefficient of 0.0941 on the busy season indicator variable being regressed on natural log of audit fees, after controlling for other various confounding factors. In other words, the audit fees charged by audit service providers during the busy season is approximately, and on average, 9.85% higher relative to the off-season.[23] We consider this almost 10% audit fee premium to be economically significant.

Furthermore, in Table 2 we calculate various credibility intervals using Eqs. (2)–(6) above. As noted earlier, credibility intervals can be constructed from the estimated population variance, which in turn is the variance in sample effect sizes corrected by the sampling error. We calculate four credibility intervals at 99%, 95%, 90% and 80%.

All four of the credibility intervals show a large range, with even the 80% credibility interval having a range of 15% points.[24] The 99% and the 95% credibility intervals both straddle zero, but the 90[%] credibility interval does not. The 90% credibility interval shows an upper bound of 19.25% and a lower bound of 0.65% of the busy season effect size. This 90% *credibility interval* implies two things: first, it does not straddle zero, and we can therefore conclude that the busy season's positive effect on audit fees is marginally statistically significant at conventional levels (10% level); and second, there is a probability of 0.90 that the 'true' busy season audit fee premium is somewhere between a low 0.65% and a substantial 19.25% on top of the off-season audit fees. Hence, based on the above theoretical explanation as well as the results from the meta-analysis of extant empirical studies, audit seasonality is almost certainly an important determinant of audit fees, which is non-trivial in magnitude.

[22] It is worth nothing that these are not necessarily 957,712 independent observations. Most studies draw their samples from Compustat North America and Audit Analytics, which is a near census of the population of listed firms in the U.S. In addition, the vast majority of these studies appear to have pooled samples where observations belonging to a unique firm are likely to appear multiple times across years if it passes the inclusion criteria. It also appears that there is some commonality in sample inclusion and exclusion procedure between the papers as well as data cleaning procedures, such as winsorizing. Indeed, studies often try to match the sample inclusion criteria and data cleaning procedures of prior studies to control for sample differences being the reason for differences in results. Furthermore, most papers that complete additional analysis either use the same sample or they use various sub-samples of the original sample used for their main analysis. Thus, the aggregated observations from these samples do not represent independent observations.

[23] Out of the 97 analyses, we also identify 30 analyses (from 7 of the studies) that used only observations from the Post-SOX era (i.e. after 2002). The weighted mean effect size from these 30 analyses suggest that after the implementation of SOX, audit fees charged by audit service providers during the busy season is approximately, and on average, 11.55% higher relative to the off-season.

[24] The estimated population variance is large because of two factors: (1) there is a large variation in the effect sizes tabulated in Table 1; and (2) the relatively large sample sizes of these studies results in a relatively small sampling error. Interestingly, this suggests that the observed inconsistency in the effect size estimates does not arise from sampling error, but may be (partially) influenced by a substantial degree of between-study heterogeneity in modelling choices related to control variables and/or sample selection.

Table 3
Meta-regression on Busy Season Audit Fee Premium Effect Size.

Variables[a]	Coef.	p-values	95% Confidence Interval	
			Lower bound	Upper bound
Constant	21.389***	0.007	5.890	36.888
SampleSize(in '000)	−0.176	0.205	−0.450	0.098
#Variables	0.307	0.479	−0.551	1.166
YearFE	−1.156	0.773	−9.087	6.775
IndustryFE	−1.943	0.655	−10.554	6.667
Pre2002Obs	−8.131*	0.064	−16.749	0.486
BigNOnly	−6.956**	0.046	−13.779	−0.133
N	97			
Prob > F	0.043**			
R^2	13.19%			
Adj-R^2	7.41%			

Notes to Table 3: [a] Variables are defined as: *SampleSize* is the sample size used in each of the regression analyses in thousands of observations; *#Variables* is the number of variables in each of the regression analyses, excluding the intercept, the busy season variable and industry and year fixed effects; *YearFE* is an indicator variable that takes the value of one if the regression analysis included year fixed effects, zero otherwise; *IndustryFE* is an indicator variable that takes the value of one if the regression analysis included industry fixed effects, zero otherwise; *Pre2002Obs* is an indicator variable that takes the value of one if the regression analysis included observations prior to 2002, zero otherwise; *BigNOnly* is an indicator variable that takes the value of one if the regression analysis examined only observations from BigN auditors, zero otherwise.

6. Between study heterogeineity

While we have focused on fairly homogeneous studies where observations are drawn from U.S. public firms, and the busy season estimated effect size in these studies are all derived from similar OLS regressions (where the dependent variable is the natural log of audit fees), our results in Table 1 highlight that there is substantial heterogeneity in the estimated effect sizes. We investigate the impact (if any) on the estimated busy season effect size that may be due to contextual differences in research design between studies and analyses by using a meta-regression approach similar to Hay and Knechel (2017), except that we utilize Ordinary Least Squares (OLS) instead of Weighted Least Squares (WLS):

$$Busy_j = \beta_0 + \beta_1 SampleSize_J + \beta_2 \# Variables_J + \beta_3 YearFE_J + \beta_4 IndustryFE_J + \beta_5 Pre2002Obs_J + \beta_6 BigNOnly_J + \varepsilon \quad (7)$$

where, $Busy_j$ is the busy season effect size[25] in audit fee regression analysis *j,* and β_0 (intercept) is the estimated overall (unweighted) mean busy season effect size when all other variables equal zero. The independent variables specify different characteristics of the regression pertaining to the busy season effect size. In particular, we examine whether the sample size used to obtain the estimated busy season effect size matters by including the variable *SampleSize*. It is possible that the effect size coefficient has been biased by the variation of sample sizes among the studies. We also examine whether the size of the regression specification used to obtain the busy season effect size estimates matters by including variables that denote the number of variables in the audit fee regressions (*#Variables*) and whether the regression included year fixed effects (*YearFE*) as well as industry fixed effects (*IndustryFE*). Smaller regression specifications may suffer from omitted variable bias that could lead to either over- or under-estimation of the busy season effect size. We also include an indicator variable if the audit fee regression included pre-2002 observations (*Pre2002Obs*), as the passing of SOX might have had an impact on the estimated busy season effect size. Lastly, we include an indicator variable if the audit fee regression only included BigN auditor observations (*BigNOnly*) as there might be a difference in how the busy season effect size manifests itself between BigN auditors and non-BigN auditors. We acknowledge, however, that these variables may be a crude classification of the potential sources of between study heterogeneity.

The results from our meta-regression are presented in Table 3. The intercept (β_0) suggests that the overall (unweighted) mean busy season premium is equal to about 21.3% higher than the non-busy season, when all other variables in the meta-regression is equal to zero.[26] The only two variables that show an impact on the estimated busy season effect sizes are *Pre2002Obs* and *BigNOnly*, the coefficients of which are negatively signed and marginally significant ($p < 0.10$, two-tailed) and significant ($p < 0.05$, two-tailed) at conventional levels, respectively. In particular, if the estimation sample includes observations prior to 2002, the estimated busy season premium is lower by about 8.13% points. If the sample includes only observations from Big N auditors, the estimated busy season premium is lower by about 6.96% points. This result lends further support to the notion that a busy season still exists, and is in fact stronger in the post-SOX era. It also suggests that the busy season effect is smaller for BigN auditors, perhaps because larger auditors are better able to manage supply constraints. It is nevertheless worth noting that both coefficients are substantially smaller

[25] For ease of interpretation, effect size here is how many percent larger the audit fees are during the busy season compared to the non-busy season. That is, all the coefficient size estimates (r) in Table 1 has been transformed by 100(exp(r)-1).

[26] Some care must be taken in interpreting this number. Firstly, and in contrast to our method in Table 1 where estimates from larger samples were weighted higher, this is an unweighted average. Secondly, this is an extrapolation from the model beyond the data supported by the actual estimates used, as for example, none of the studes have a sample size of zero or have audit fee regression with no other control variables.

than the intercept. While the meta-regression model is significant ($p < 0.05$) it is only successful in explaining about 13.2% of the heterogeneity in the busy season effect size estimates, and it must therefore be noted that a substantial portion of the variation in the busy season effect size estimates observed in Table 1 is not accounted for by this model.

7. Conclusion

At the most fundamental level, the non-uniform client distribution of fiscal year-ends throughout the calendar year leads to seasonal variation in the manifestation of audit demand and supply. From an auditor's perspective, the calendar year may be divided into a busy season and an off-season. Consistent with our economic theoretical framework, the busy season leads to increased audit demand but constrained and inelastic supply, which in turn increases the price of audit services.

By conducting a meta-analysis of existing empirical studies that use an audit fee model calibrated on data from U.S. public companies, we find that *ceteris paribus* the audit busy season is associated with an approximately 10% increase in average audit fees. We consider this to be an economically significant audit fee premium. A meta-regression of the estimated busy season effect size and the contextual differences in research design between studies show that examining only Big N in the studies attenuates the busy season effect size but does not eliminate it, and that the busy season effect size might have increased post-SOX. Our results should be of interest to both audit clients and researchers who may be interested in the effect and fee premiums associated with seasonality.

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