Office-Level versus Firm-Wide Industry Specialization and Auditor Oversight

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ABSTRACT I analyze third-party claim reserve practices in not-for-profit hospitals. The purpose of the paper is to assess the relation between industry expertise and the monitoring role of audits based on the firm-wide and office-level framework in Ferguson et al. (2003). The firm-wide view assumes that industry expertise is captured by the firm as a whole and therefore resides at the firm level. My findings provide support for the view that auditor expertise is tied to individual professionals and is not readily captured and distributed by accounting firms to other practice offices and clients. This paper provides guidance to studies testing if the audit market prices firm-wide versus local-office industry expertise.

Keywords Auditor industry expertise, hospital, local office, firm wide

JEL Classification

Data Availability Data can be obtained from sources described in the paper

1. Introduction

The purpose of this study is to examine the effect of auditor industry expertise on the monitoring role of audits based on the firm-wide and office-level framework in Ferguson et al. (2003). Industry knowledge and expertise in auditing derives from training and practice experience by professionals in a particular industry (Solomon et al. (1999)). The central issue in the firm-wide versus office-level view on industry expertise is the degree to which such expertise is transferred within the accounting firm. Craswell et al. (1995) take a firm-wide view on the accounting firm. They consider a firm's audit practice in aggregate and presume no differentiation exists across local offices of the firm. With the firm as the focal point, their evidence is supportive of audit fee premia for industry specialists based on firm-wide reputations. More recent audit pricing studies find that auditor reputation for industry expertise is neither strictly national (i.e., firm-wide) nor strictly local in character. The evidence in Francis et al. (2005) is mixed with respect to local-office industry leaders alone that are not also national industry leaders. On the other hand, Ferguson et al. (2003) conclude that "national leadership rankings are, in fact, driven by the specific offices where accounting firms are city leaders" (p. 429). My study contributes to resolving this issue by investigating whether industry expertise is transferred within accounting firms. After all, a knowledge-sharing practice across local offices within accounting firms is a pre-condition for industry-specialization fee premia based on firm-wide reputations to exist in a competitive audit market.

The goal of this study is to capitalize on insights from prior studies to set up a powerful test of the industry-specialization framework in Ferguson et al. (2003). For that reason I adopt a single-industry approach. Previous studies typically pool observations from a large range of industries (e.g., Craswell et al. (1995); Francis et al. (2005); Romanus et al. (2008)). This multiple-industry

approach has one important drawback: it implicitly assumes that expertise-outcome multiples (either audit fees or a proxy for audit quality) are identical across industries. However, there is reason to believe that this assumption is not valid.[1] That is to say, for certain types of industry little or no branch-specific expertise is required to perform high-quality audits. In these industries (e.g., mere retail and production firms), client-specific knowledge suffices to perform an above-the-legal-minimum audit. This point of view is supported by the lack of evidence of specialization for some industries in empirical studies (e.g., Craswell et al. (1995); Hogan and Jeter (1999)) and the limited number of industry lines structured in the Big Four accounting firms in practice. To illustrate, the Standard Industrial Classification (SIC) manual recognizes 85 *major groups* and 10 *divisions*. In the Netherlands (the empirical setting in this study), the Big Four accounting firms — Deloitte & Touche, Ernst & Young, KPMG, and PricewaterhouseCoopers — maintain *only* 31, 13, 24, and 30 different branch groups, respectively (using data from audit firms' recent websites).[2]

To improve the power of my tests, I select an industry where expertise and knowledge are expected to matter a priori. Among the industries available, the hospital sector provides a powerful research setting for the following two reasons. First, disclosure regulation typical to the industry allows comparison of third-party claim reserve estimates to ex-post outcomes. This provides an objective measure of bias (Ball (2008)). These reserves can be substantial and require considerable judgment by management because it is difficult to accurately forecast subsequent adjustments. The size of this client-specific accrual coupled with the substantial managerial

[1] Knechel et al. (2007) observe that auditors employ industry-specific measures of materiality. They report that the materiality threshold employed for nonprofit organizations is three percent of net assets (p. 397). For comparison purposes, the reported threshold used in the mutual fund industry is ½–1 percent of net asset value.

[2] To illustrate, on its website Ernst & Young recommends itself to potential clients for having "branch-expertise and specialized knowledge" in the healthcare sector. Each of the Big Four accounting firms has a healthcare branch group.

judgment required to estimate the settlement amount makes this account highly susceptible to manipulation (McNichols (2000)). Second, the hospital sector in the Netherlands is highly regulated (Büller (2007)) with divergent branch-specific accounting standards (Schaepkens (2001)). These regulatory complexities together with hospital-specific reporting requirements may lead to economies of scale for auditors by specializing (Danos and Eichenseher (1982)).

As discussed in the next section, there are important institutional differences between the Netherlands and the empirical settings in Francis et al. (2005) (U.S.) and Ferguson et al. (2003) (Australia). Characteristics of the Dutch hospital sector make my data set ideally suited to investigate knowledge transfers within accounting firms. First, geographical distance between local offices within audit firms is typically small making it possible for industry experts to travel between local offices to provide a firm-wide uniform level of expertise (Ferguson et al. (2003)). By contrast, the Big Four accounting firms in the U.S. are widely dispersed around the country having from 91 (Deloitte) to 115 (KPMG) separate practice offices (Francis et al. (2005)). In the Netherlands the Big Four accounting firms have local practice offices (servicing clients in the hospital sector) in 8 (Deloitte), 12 (Ernst & Young), 5 (KPMG), and 6 (PricewaterhouseCoopers) cities, respectively. Since the Big Four have relatively fewer offices in the Netherlands compared to the U.S., it may be more straightforward for Big Four firms in the Netherlands to achieve firm-wide transferability of office-specific industry expertise. Second, as hospital regulations in the U.S. vary from state to state (Eldenburg et al. (2008)), there may be relatively lower economies of scale for auditors in the U.S. to achieve a uniform firm-wide level of industry expertise. The sample hospitals in my study, however, are all subject to the same regulation. This attribute may, again, support industry knowledge-sharing practices within accounting firms. Third, Ferguson et al. (2003) examine only publicly-listed companies headquartered in the five largest Australian

cities. They report that the audit market in the Healthcare and Biotechnology sector in each of these remote cities is dominated by one accounting firm.[3] The Big Four accounting firms in my sample, however, display considerable variation in the number of hospitals they service from their local practice offices. The average number of hospital-clients in my sample ranges from 1.5 (Deloitte) to 3.7 (PricewaterhouseCoopers) per local office. This greater cross-sectional variation adds to the power of my test.

Notwithstanding these aforementioned power arguments, the reverse side of selecting one industry is a reduced sample size—the maximum number of observations amounts to 78 nonprofit hospitals in this study. To mitigate concerns about moderate sample-size effects on inferences, I perform two tests. First, a non-parametric bootstrap analysis shows that the finite-sample properties of my OLS statistics are robust. Second, as opposed to 'artificially' increasing sample size by taking random draws with replacement (in case of the bootstrap procedure), in another test I further *downsize* my sample and estimate reduced-form regressions for the two market leaders separately. Provided that inferences remain unaffected from this reduction in sample size, this test provides support for the principal findings based on the full sample.

Another important yet unresolved issue in audit research is the appropriate definition and measurement of auditor industry expertise (see Gramling and Stone (2001) for an extensive review of this literature). Albeit different metrics for industry specialization are used in the literature, the common denominator is that servicing a larger number of clients in a specific industry relative to other accounting firms makes an auditor a specialist in that industry. In this

[3] Ferguson et al. (2003) report the following city-level market shares in the Healthcare and Biotechnology sector (based on industry categories used by the Australian Stock Exchange): KPMG 70% in Perth and 73% in Melbourne, PricewaterhouseCoopers 46% in Sydney, and Deloitte 100% in Adelaide. In Brisbane Johnston Rorke (one of Queensland's largest accounting firms) is market leader with a market share of 74%.

study I focus on clients of the Big Four accounting firms only.[4] Each of them claims to be a healthcare specialist on its website. Krishnan (2001) finds that these so-called 'self-reported specializations' typically do not concur with an auditor's market share in that industry. This does not apply to the healthcare industry in the Netherlands, however. The Big Four's market shares range from eighteen (Deloitte & Touche and KPMG) to thirty-four percent (Ernst & Young). Also periodical reviews of best practices in the healthcare sector (e.g., in the field of corporate governance by Ernst & Young and PricewaterhouseCoopers) and publishing guidebooks and newsletters on several healthcare-related financial reporting issues (e.g., Deloitte) give evidence of branch-related expertise. Rational economics suggests accounting firms will spread this knowledge throughout the firm to offer higher-quality services or to reduce audit production costs (Hogan and Jeter (1999)).

My results are as follows. I analyze the third-party claim reserve practices of not-for-profit hospitals in the Netherlands. The collective evidence does not support the existence of positive network externalities across offices, as assumed under the firm-wide view of industry expertise. Rather, my study consistently shows across all tests that industry-specific knowledge resides decentralized (i.e., at the local practice office) in accounting firms. Specifically, I find that hospitals' tendency to under-reserve third-party claim reserves is increasing in claim complexity (using ex-post length of the claim cycle as a proxy). However, this behavior is attenuated when the hospital is audited by a local-office based industry specialist. These findings provide support for the view that auditor expertise is tied to individual professionals and is not readily captured and distributed by the firm to other offices and clients. Similar arguments have been made about

[4] DeAngelo (1981) argues that auditor size is a proxy for quality because no single audit client is important to a large auditor and larger auditors have established brand name reputations to lose (their entire clientele) if they misreport. Accordingly, prior research considers audits of Big Four firms *as a group* to be of higher quality than other (smaller) accounting firms. These arguments do not necessitate that the Big Four provide a uniform level of audit quality, however. This is why I control for fixed accounting-firm effects in my empirical analysis.

human capital and client-specific expertise in law firms (Gilson and Mnookin (1985)) and knowledge-intensive firms in general (Weiss (1999)).

The paper proceeds as follows. Section 2 explains industry practices typical of the hospital sector that are important to the study. Section 3 states the hypothesis. Section 4 describes the sample selection process, variable measurement, and provides descriptive statistics. Section 5 reports the results. Finally, Section 6 concludes.

2. Industry practices

2.1 Third-party claim reserves

Hospitals follow what are known as Healthcare Accounting Standards (HAS) for reporting to health insurers, banks, supervisory authorities, and other stakeholders. Under HAS, the third-party allowance account is the estimated liability (asset) on hospitals' financial statements for settlement of over-financing (under-financing) with the health insurers. Over- (under-) financing can occur if the annual budget is exceeded (underspent) by funds received from health insurers for performed healthcare services. Audits by supervisory authorities and/or health insurers after a hospitals' fiscal year end can lead to denied or adjudicated claims. Further, the ultimate rates paid are subject to retrospective adjustments. HAS requires hospitals to reserve for any anticipated adjustments to payments resulting from audits and retrospective rate adjustments by third-party insurers that occur after the year-end. This requires considerable judgment by management because it is difficult to accurately forecast subsequent adjustments, and this liability (or, asset in case of a financing deficit) can be substantial. For the sample hospitals claim reserves average about 6 percent (€6,715,892) of total assets. More importantly, claim-reserve bias averages 33 percent (€1,279,599) of the initial reserve (statistics are untabulated). The size of this account

coupled with the substantial management judgment required to estimate the settlement amount makes this account susceptible to earnings management.

The amount of error in the reported third-party claim reserve is known with certainty only after a claim arising from a given period is settled. If managers, however, know the unbiased accounting numbers when making the initial estimates, then comparison of the developed reserve to the initial reserve provides an objective gauge of earnings management (Gaver and Paterson (2007)). This settlement verification is possible because of the extensive disclosure requirements under HAS. Table 1, excerpted from the 2002-2007 *Universitair Medisch Centrum Utrecht* (UMC)'s annual reports, provides an example of these disclosures. The third-party claim reserve nets aggregate estimated annual budget deficits and surpluses against cumulative cash payments and receipts for current and previous years. UMC's claim reserve for fiscal year 2002 amounts to 3.1 (4.4) percent of the total assets (external budget). In my study, I use a four-year development period to determine reserve bias.[5] To compute the 2002 claim-reserve bias for UMC, I subtract the original budget deficit of €16,956 from the final settlement by health insurers and/or the supervisory authority NZa (€16,956 + €4,569 + €1,483) (all Euro amounts are in thousands).[6] For UMC, the four-year developed BIAS equals €6,052 (approximately 35 percent of the original 2002 reserve). UMC reports in her 2005 annual report that "[t]he costings upto and including 2002 are definitively settled with CTG/ZAio [NZa]" (p. 60; between brackets added).

[5] Petroni (1992) and Petroni and Beasley (1996) study the property-casualty insurance industry and use a five-year development period.

[6] The Healthcare Authority (NZa) is the supervisory body for all the healthcare markets in the Netherlands. The NZa was established on 1 October 2006. The NZa evolved out of the supervisory Board for Health Care Insurance (CTZ) and the National Health Tariffs Authority (CTG). To avoid indistinctness, the abbreviation NZa in this study also refers to the CTG and ZAio (i.e., the NZa during its founding stage).

2.2 Monitoring role of audits

Auditors play a vital role in monitoring reliable financial reporting in the healthcare sector, with increasing attention for corporate governance. The *Regeling Jaarverslaggeving Zorginstellingen* dictates that all hospitals have their annual report attested by an independent auditor (Article 32, §8). Auditor oversight is also imposed by other regulations, like the auditor reports under *Regulation Internal Control* in the Cure and Care sector (CU/NR-100.060, CA/NR-100.063, and GG/NR-100.040). A consultation body of auditors in the healthcare sector (*Coziek*) reports on its webportal that "[o]n account of a multitude of regulation and supervisory authorities in the healthcare sector in the Netherlands, auditors face a great variety in audit regulations and protocols". To indicate the extent of regulation in the healthcare industry, the Committee Büller (2007) observes that "there are nearly forty supervisors in the sector, who all examine, inspect, accredit, and audit" (p. 2).

Hospitals are required to file their annual reports with *Centraal Informatiepunt Beroepen Gezondheidszorg* (CIBG). Annual reports are used in setting reimbursement policies (e.g., a hospital's external budget) and in other forms of decision-making—both by the internal organization (for example, in compensating managers) and by external parties (for example, the NZa, Ministry of Healthcare, and financial institutions, like lending banks). To illustrate the use of a hospital's annual report in policy-making by the NZa and the Ministry of Healthcare, consider the following two excerpts taken from NZa's annual report of 1996 and 1997, respectively (between brackets added):

> "During several years, academic hospitals show a steady capital growth of € 31 million a year consequent on returns. I [the Minister of Healthcare], therefore, deduce that the annual external budgets are too high structurally. In light of these facts, I have decided to lower the acceptable costs with € 16 million structurally."

> "On December 6, 1996, the Minister of Healthcare has imposed academic hospitals with a rebate of € 16 million in accordance with the 'temporizing of capital growth'. The Minister has based this act on the analysis by the CTG of income statements of hospitals for financial year 1994."

Not-for-profit hospitals are allowed to earn profits, but these profits cannot be directly distributed to residual claimants. Despite their nonprofit status, management and other users of hospitals' financial statements are expected to focus on reported accounting numbers (Deneffe and Masson (2002)). This expectation is grounded on findings reported in prior research. Apart from political-cost motives, managers in nonprofits have incentives to reduce the cost of debt capital (Leone and Van Horn (2005)), to avert CEO turnover and accrue compensation (Lambert and Larcker (1995); Eldenburg et al. (2004)). Brickley and Van Horn (2002) conclude that managers in nonprofit hospitals "face incentives to concentrate on financial performance."

3. Hypothesis development

Prior studies document evidence of differential audit quality (above the legal minimum) along dimensions such as accounting firm size, industry specialization, and audit office characteristics (see Francis (2004) for an extensive review of this literature). Solomon et al. (1999) argue that industry experts have a deeper knowledge than non-experts due to greater experience in the industry which enables experts to make more accurate audit judgments. An important question is whether positive *firm-wide* industry-expertise externalities exist or whether industry expertise resides in individual *office-based* engagement partners (acting on behalf of the accounting firm). Put differently, is industry expertise transferred within accounting firms? Evidence of a firm-wide industry premium in prior audit-pricing studies suggests positive network externalities across individual offices exist (e.g., Craswell et al. (1995)). Ferguson et al. (2003) argue that such positive network externalities across local offices are likely for *brand name* reputations, due to

firm-wide control procedures (e.g., standardized audit programs, peer review) and uniform personnel practices with respect to hiring, training, and promotions. With *industry expertise* it is less clear if there are such positive firm-wide externalities. I now discuss in greater detail the assumptions underlying the firm-wide and office-level views of industry expertise within the Big Four accounting firms.

Firm-wide industry expertise. The firm-wide view assumes that industry expertise is captured by the firm as a whole and therefore resides at the firm level. In this view, all offices of a particular firm are assumed suppliers of homogenous expertise. For example, Francis et al. (2005) contend that accounting firms capture industry expertise through knowledge-sharing practices such as internal benchmarking of best practices, and the employment of firm-wide quality control procedures including the use of standardized industry-tailored audit programs. In addition, Ferguson et al. (2003) argue that travel by industry experts between offices and other forms of contact between them and local-office partners provides a firm-wide uniform industry expertise. The fact that Big Four firms hold themselves out in the media (including their websites) as having firm-wide operations organized along industry lines supports this view.[7] Transferring industry-specific expertise within the firm is rational economic behavior to the extent that it enables auditors to provide higher-quality services or to reduce audit production costs.

Office-based industry expertise. Under the office-level view, industry expertise resides in individuals and is acquired through (repeated) experience with clients in specific industries (Solomon et al. (1999)). Ferguson et al. (2003) argue that some aspects of industry expertise can be distributed within the firm through industry-tailored audit programs and databases with best industry practices. However, deep industry knowledge is limited to specific engagements and is

[7] On the other hand, Krishnan (2001) reports that such 'self-reported specializations' are not correlated with actual market shares for most auditors.

expected to reside in individual experts operating from local offices. Supportive of this view—in spite of claiming expertise in both industries through their firms' websites, the subjects in Solomon et al. (1999) (affiliated with three of the Big Four accounting firms) were designated by their employers as *either* specialists in the health *or* financial industry. Moreover, the overall evidence in Ferguson et al. (2003) suggests that the market perception and pricing of industry expertise in Australia is primarily based on office-level rather than national-level industry leadership.

In sum, if the firm-wide view is correct—namely, industry expertise is transferred within a firm—then all offices of a particular accounting firm supply homogenous expertise.

4. Sample selection and research design

4.1 Sample selection

The data used in this study is hand-collected from hospitals' annual reports for fiscal years 2002 till 2006 from the web portal www.jaarverslagenzorg.nl as maintained by the *Centraal Informatiepunt Beroepen Gezondheidszorg* (CIBG). Hospitals are required by law to file their audited financial statements with the CIBG. I select 2002 data for the analysis to be sure that the majority of the claims filed for the year 2002 are settled and accordingly processed in the 2006 annual statement. Unreported analysis shows that the process from filing till final settlement with health insurers and/or the NZa takes on average two and a half years for my sample hospitals. Moreover, by selecting 2002 I avoid confounding events like the introduction of a new set of accounting standards (*Regeling Jaarverslaggeving Zorginstellingen*) in force from fiscal year

2000 or the transition to DBC (diagnosis treatment combinations) as the basis for a new funding system in 2005.[8]

I begin with all 124 not-for-profit hospitals in the Netherlands in 2002. All hospitals are privately owned and their legal form is a trust. Sample organizations must be of the category *hospital-care only* (i.e., purely providing hospital services). I exclude 32 mixed-healthcare organizations to create a relatively homogeneous sample in terms of industry knowledge required to perform a high-quality audit. After all, services other than hospital care are subject to different regulation and monitoring by other institutions. Including these organizations in the sample will confound the analysis. From this group, I eliminate 11 hospitals that lack sufficient data to compute the variables in Eq. (1) and 3 hospitals audited by Non-Big Four auditors. This creates a sample of 78 hospitals serviced by Big Four auditors. Table 2 summarizes the sample selection procedure.

4.2 Variable definitions

My hypothesis specifies three key variables. For each hospital, I must estimate claim-reserve bias and claim complexity. Differential industry expertise is expected to reveal itself conditional on the complexity of the claim (Bonner (1990); Bonner and Lewis (1990)). For each local audit office, I must determine industry-expertise status. Prior research indicates that reporting quality is determined by several other (hospital) characteristics. In fact, some of the main variables are also correlated with these characteristics. To alleviate concerns about correlated-omitted-variables

[8] DBCs were introduced by the Dutch government to stimulate market forces in the healthcare sector and encourage patients and insured parties to make more informed choices in their selection of care providers. After performing an experiment with a limited number of treatments in 2003, the DBC system was introduced in 2005. DBC registration gives the hospital greater insight into treatment costs. Moreover, hospitals and health insurance companies gain greater and more detailed insight into the cost of treatment.

bias, I control for these characteristics in the analysis. This section describes the measurement of these variables. Table 3 provides descriptive statistics for the sample hospitals.

I use a development period with a maximum of four years to determine the sign and level of claim-reserve bias. For each hospital in the sample, I subtract the original budget-deficit/surplus account in the 2002 annual report from the final settlement by health insurers/NZa or alternatively, if the claim process is still pending, the recent (updated) assessment by management as reported in the 2006 annual statement. In mathematical notations, I calculate the dependent variable as BIAS $\equiv A_{2002+t}\,(D/S_{2002}) - R_{2002}\,(D/S_{2002})$, where R_{2002} represents the budget-deficit/surplus (D/S) for fiscal year 2002 as reported in the 2002 annual report, and A_{2002+t} is the final assessment by health insurers/NZa in year t following 2002 (with $t \leq 4$ years). I then divide BIAS by a hospital's lagged PPE (property, plant, and equipment) as reported in the 2002 annual statement to control for variation in hospital size.[9] The Appendix explains the budget system in hospitals.

Audit research shows that auditors correct overstatements of financial performance more often than they correct understatements (Kinney and Martin (1994)). The reasoning behind this notion is that undetected overstatements are more costly to auditors than are undetected understatements because overstatements lead to out-of-pocket losses to financial statement users that can be recouped through a claim on the auditor (Barron et al. (2001)). In consequence, auditors have distinct primary audit objectives for assets (preventing overstatements) and liabilities (constraining understatements) (IAASB *International Standard on Auditing* (ISA) No. 500 *"Audit Evidence"*). Following this reasoning, I reverse-code BIAS for deficit-cases (BIAS*, hereafter). By changing signs of BIAS for deficit-cases, now a negative (positive) BIAS*

[9] Prior researchers in related studies also scale their dependent variables in an attempt to reduce heteroskedasticity problems and to improve cross-sectional comparability (e.g., Petroni and Beasley (1996), Gaver and Paterson (2007)). My results are not sensitive to the choice of scaling variable.

indicates relatively higher (lower) audit quality. As reported in Table 3, the average reverse-coded claim-reserve bias (BIAS*) is about 1.5 percent of lagged PPE (approximately €1,162,770). A simple t-test shows that the mean BIAS* is negative at $p < 0.01$ (two-sided).

To classify claim complexity I use the ex-post length of the claim cycle (LENGTH). If the claim process is still pending by the end of the four-year observation period, LENGTH is set equal to 5 (in only 1 case).

The auditor's report discloses the name and office location of the accounting firm that audited the annual statement. Review of these reports reveals that the 2002 audits of my 78 sample hospitals are conducted by 4 different audit firms in 32 offices (see Panel B in Table 3). The spread of clients among the Big Four audit firms is as follows: Ernst & Young 28, PricewaterhouseCoopers 22, Deloitte & Touche 14, and KPMG 14.

I measure industry expertise using the method introduced in Zeff and Fossum (1967) by dividing the sum of the square root of revenues of clients per local office by the sum of the square root of revenues of all sample firms. In mathematical notations, EXPERT $\equiv \sum_{i=1}^{I_j}\sqrt{REV_{ij}} \Big/ \sum_{j}^{J}\sum_{i=1}^{I_j}\sqrt{REV_{ij}}$, where REV is revenue for hospital *i* as audited by local office *j*.[10] Values for EXPERT vary from 0.0056 (for the Ernst & Young Middelburg office) to 0.1324 (for the Ernst & Young Amsterdam office) (untabulated).

I include several hospital-specific variables in the regression model to alleviate concerns about correlated-omitted-variables bias. FTE is the number of a hospital's full-time employees and captures audit complexity. Large hospitals provide large-scale specialisms to their patients, increasing audit complexity. On the other hand, FTE also captures the amount of administrative resources available to conscientiously record processes. LPROFIT equals one if reported net

[10] In unreported analyses I employ hospital size, as measured by PPE as per beginning of fiscal year 2002, instead of revenue. Inferences are insensitive to this alternative variable.

income over 2002 (before adjustments to budgets from prior years) is positive and is larger than 2.5 percent of lagged PPE; zero otherwise. Given the not-for-profit status, managers may have (political-cost) incentives to manage earnings downwards to prevent reporting a large profit. Since 2002 Ernst & Young annually reviews the corporate governance standards in Dutch hospitals. Hospitals are ranked based on eighty criteria derived from corporate governance principles and best practices. GOV is measured as a hospital's rank in the 2003 Ernst & Young report (with lower ranks representing higher-quality governed hospitals). DLEV captures a hospital's financial condition and is computed as the ratio of total debt to equity. DWFZ is equal to 1 if a hospital participates in a garantee fund for hospitals (data from *Waarborgfonds voor de Zorgsector*) in 2002; 0 otherwise. A hospital participating in this fund can borrow under more favourable conditions. However, participation also brings about periodic evaluations by the *Waarborgfonds*. Finally, hospitals that intially over-reserved are found (untabulated) to have less pronounced reserve errors. Therefore, following Gaver and Paterson (2007), I include an indicator variable (UNDER) set to one if BIAS* is negative, and zero otherwise.

5. Empirical results

5.1 Univariate analysis

Table 4 provides univariate tests across the two main variables under study—LENGTH, as a proxy for claim complexity, and DEXPERT. Panel A in Table 4 reports the results for the full sample with LENGTH ≤ or > three years (the sample mean and median) as a cut-off. Two findings are worth mentioning. First, with budget-deficit and budget-surplus cases pooled, univariate analysis shows no evidence on systematic industry-knowledge transfers within audit firms. On the contrary, for more complex claims (i.e., claims with an ex-post cycle length of

more than three years) local-office based industry experts provide higher-quality audits as opposed to non-experts. The difference in BIAS*, however, is only marginally significant at $p < 0.10$ (two-sided). Second, increasing claim complexity for non-expert practice offices is negatively associated with audit quality at $p < 0.10$ (two-sided). That is, conditional on DEXPERT equal to 0 BIAS* decreases from -0.0197 to -0.0017 with increasing claim complexity. This is consistent with a growing demand for branch-specific expertise with more complex budget claims.

Panel B in Table 4 reports the results for budget-surplus cases only. A budget-*surplus* is distinct from a budget-*deficit*. As a rule, evidence to verify the existence assertion for a budget-deficit is relatively easier (i.e., requiring less subspecialty knowledge) to obtain than the completeness assertion for a budget-surplus. As a consequence, the relation between industry expertise and auditor oversight is arguably more apparent for surplus-cases than for deficit-cases. Although the magnitude of the reported means per cell increases, t-statistics in Panel B for differences in means across cells are not different from those reported in Panel A.

5.2 Multivariate analysis

Under the firm-wide view, all local practice offices of a particular accounting firm supply homogenous expertise. To test this hypothesis, I estimate the following regression model:

$$BIAS_i^* = \alpha_0 + \alpha_1 UNDER_i + \alpha_2 LENGTH_i + \alpha_3 DEXPERT_i + \alpha_4 \left(LENGTH_i \cdot DEXPERT_i\right) + \alpha_5 \text{Controls}_i + \varepsilon_i \quad (1)$$

In this formulation, α_4 captures the interplay between claim complexity (LENGTH) and local-office based auditor industry expertise (DEXPERT). All variables are defined as in Section 4.2. I allow for structural differences in industry expertise across the Big Four accounting firms by controlling for firm-specific fixed effects in Eq. (1). Table 5 reports the findings. Reported

coefficients are standardized to control for differences in scales of the independent variables. The p-values are based on White (1980)'s heteroskedasticity-corrected covariance matrix. One-tailed p-values are reported for DEXPERT and LENGTH · DEXPERT.

I estimate Eq. (1) using the pooled sample of budget-deficit and budget-surplus cases for 78 nonprofit hospitals in columns 1 through 3 and column 6. Column 6 reports results from a rank regression. To control for possible non-linearities or the effect of outliers on inferences, I transform BIAS* into ranks, with the smallest magnitude observation for BIAS* > 0 (BIAS* < 0) having a value of 1 (*minus* 1). For BIAS* = 0, rank BIAS* also equals zero.[11] Finally, columns 4 and 5 report estimation results for surplus-cases only. Since the findings are more or less alike in each column, below I discuss the results reported in column 3 of Table 5. This specification includes audit firm-specific intercepts and the control variables SIZE, LPROFIT, DWFZ, CORPGOV, and DLEV. The control variables are meant to capture influences on reserve bias that are unrelated to earnings management.

I observe the following. First, the coefficient on LENGTH is significantly positive. Consistent with univariate evidence reported in Table 4, this means that audit quality deteriorates in increasing claim-cycle length. Second, the coefficient on DEXPERT is negative, albeit statistically insignificant at conventional levels. Third, the coefficient on LENGTH · DEXPERT is significantly negative (at p = 0.02; one-sided). This implies that industry specialists are more effective than other auditors in constraining claim-reverse bias among hospitals with complex claims. This finding does not support the existence of positive network externalities across local-practice offices, as assumed under the firm-wide view of industry expertise. Rather, this result supports the office-level view arguing that industry expertise resides in individual office-based

[11] BIAS* equals zero in 16 out of 78 cases, of which 9 observations from clients of industry specialists. The principal results reported in the paper remain materially unaffected when excluding observations for which BIAS* = 0 (results are not reported in a table).

engagement partners. Finally, only LPROFIT is significantly associated with BIAS*. Hospitals reporting a positive net income over 2002 (before adjustments to budgets from prior years) larger than 2.5 percent of lagged PPE are less likey to under-reserve than hospitals reported lower profits or a loss. Reserve bias does not differ appreciably between smaller and larger nonprofit hospitals (SIZE), participating in a guarantee fund for hospitals or not (DWFZ), having a high or lower quality corporate governance system installed (CORPGOV), or facing a high versus low leverage ratio (DLEV).

Within-firm analysis

In the firm-wide view all offices of a *particular* accounting firm are suppliers of homogenous expertise. The principal findings reported thusfar are based on a pooled regression of client-firm observations from the four largest audit firms in the hospital sector. Prior research considers audits of Big Four firms *as a group* to be of higher quality than other (smaller) accounting firms (Francis (2004)).[12] This does not necessitate that the Big Four provide a uniform level of audit quality, however. In consequence, albeit all four audit firms list the hospital sector as a key industry on their websites, it is conceivable that systematic differences in expertise exist across the accounting firms. Although I allow for such differences across firms by including firm-specific fixed effects in Eq. (1), I alternatively estimate a reduced form of Eq. (1) separately for two individual accounting firms (namely, Ernst & Young and PricewaterhouseCoopers with 28 and 22 client observations, respectively). Results are reported in Table 5 and support the principal findings as reported in Table 4.[13]

[12] This view is supported by the statistical insignificance of the audit firm-specific intercepts in Eq. (1).

[13] Following Petersen (2008), I also control for potential cluster dependence (at the audit-firm level and local-office level) by two-way cluster analysis. Untabulated analyses show that inferences remain materially unaffected.

Local competition and office-level industry expertise

The importance of considering local-market structure effects on audit-quality differentiation is supported by several studies. After all, pricing consequences of local-market competition can influence auditors' incentives to supply differential audit quality (above the legal minimum). Using *nation-wide* market share data, Pearson and Trompeter (1994) document an inverse audit fee-concentration relation for a group of U.S. insurance companies, suggesting that higher industry-segment concentration is associated with increased price competition. Bandyopadhyay and Kao (2004) adopt a *local-office* approach to investigate the relation between market structure and audit fees. They find no relation between local-market concentration and price discrimination among Big Six clients. In a recent study, Numan and Willekens (2008) contend that auditors specialize in response to increased price competition in the local audit market.

To investigate the impact of heterogeneous city-level competition as a potential correlated-omitted variable on my findings, I compute COMP as an indicator variable set to 1 in case more than one auditor services not-for-profit hospitals from a certain city; 0 otherwise.[14] Inferences remain almost unchanged. The coefficients for DEXPERT and LENGTH · DEXPERT remain negative, with the latter statistically significant at $p < 0.02$ (one-sided).

5.3 Robustness and specification tests

The principal findings in the previous section are a significantly positive coefficient on α_2, indicating that the tendency to under-reserve (over-reserve in case of a budget-deficit) with increasing claim complexity, coupled with a significantly negative coefficient on α_4, indicating that this tendency is attenuated when a hospital's financial statement is audited by an industry

[14] Using the number of clients per local practice office as reported in Panel B of Table 3, COMP is set to 1 for hospitals retaining an auditor located in Amstelveen, Amsterdam, Den Haag, Eindhoven, Enschede, Groningen, Middelburg, and Utrecht. Most of these cities are provincial capitals.

specialist. Inconsistent with the firm-wide view (presuming no audit-quality differentiation exists across local offices of accounting firms), the collective evidence supports the office-level view on auditor industry specialization. These findings are robust to a number of robustness tests.

Measurement of auditor-industry expertise. The appropriate measurement of auditor industry expertise is an unresolved issue in audit research (Gramling and Stone (2001)). To alleviate concerns of possible spurious results, I employ two alternative measures for auditor industry expertise. The first measure, OLCLIENTS, is based on the number of hospitals audited by each local office. As opposed to EXPERT, this alternative proxy does not make the assumption that expertise is determined by engagement characteristics (namely, size of the client in EXPERT). On the contrary, each client is treated equally in OLCLIENTS. Panel A of Table 3 reports an average of 4 clients per local office. The Spearman correlation between EXPERT and OLCLIENTS is 0.9275 ($p < 0.01$).

The second alternative measure is derived from EXPERT and weighs each audit client in the local-office client portfolio based on the length of the client-auditor relationship (TENURE). That is, W_EXPERT $\equiv \sum_{i=1}^{I_j}\left(\sqrt{REV_{ij}} \times TENURE_{ij}\right) \Big/ \sum_{j}^{J}\sum_{i=1}^{I_j}\left(\sqrt{REV_{ij}} \times TENURE_{ij}\right)$. The intuition is that EXPERT potentially *understates* the impact of client-specific *experience* on industry *expertise*. To illustrate, in computing EXPERT client W (REV = 100; TENURE = 1) is weighted equally as client X (REV = 100; TENURE = 3). Client Y (REV = 100; TENURE = 1), on the other hand, receives relatively more weight in EXPERT than client Z (REV = 30; TENURE = 3). To compute W_EXPERT, I searched the 2000 and 2001 annual reports to find whether a hospital has recently changed auditors. TENURE is set to 3 if a hospital did not switch auditors during any of the three years (i.e., during 2000 through 2002). In case of an auditor change in 2000 (2001), TENURE is set to 2 (1). As reported in Table 3, the average score for W_EXPERT is

0.0127. The Spearman correlation between EXPERT and W_EXPERT is 0.9595 ($p < 0.01$). The principal findings are unaffected by replacing EXPERT by either OLCLIENTS or W_EXPERT (results are untabulated).[15]

Winsorizing and quantile regression. Although a rank regression controls for non-linearities or the effect of outliers on inferences, as a robustness test I winsorize BIAS at the 2.5th and 97.5th percentiles and then re-estimate Eq. (1). Inferences remain materially unaffected (results are untabulated). Quantile regression models the relation between one or more covariates and the conditional quantiles (OLS regression uses the mean) of the response variable, so it is especially useful in applications where extremes are important. Estimation results from quantile regressions at the 50th quantile (not reported in a table) show that some observations do inflate (downward and upward) the coefficients of the regressors as reported in Table 5 using the OLS estimation. The coefficients on α_2 and α_4, however, remain statistically significant, both at $p < 0.05$ (two-sided).

*Scaling factor for BIAS**. To investigate whether my findings are sensitive to the choice of lagged PPE to scale BIAS*, I employ two alternative scaling factors—namely, a hospital's revenue for fiscal year 2002 and the average number of full-time emloyees (FTE) during 2002. Estimation results (not reported in a table) are similar to those reported in Table 5. Using 2002 revenue as a scaling factor, the coefficient on α_2 is positive 0.3577 (0.3231 with FTE), both significant at $p < 0.06$ (two-sided) or better. The coefficient on α_4 is negative 0.2590 (0.2362 with FTE) at $p < 0.03$ (two-sided) or better, when using revenue as a scaling factor.

Bootstrapping procedure. OLS estimates are asymptotically normal under the assumption of the central limit theorem. However, given the relatively small sample size (78 observations) I

[15] The coefficient for TENURE (as a main effect) in Eq. (1) is insignificantly different from zero at conventional levels.

cannot safely expect OLS estimators to have a normal distribution and standard errors to be based on strong distributional assumptions. Hence, I employ bootstrapping as an alternative to asymptotic approximations for obtaining standard errors and p-values for test statistics (Wooldridge (2002)). To investigate the finite-sample properties of my OLS statistics, I employ a non-parametric bootstrap (500 draws with replacement). As expected, the bootstrapped standard errors are on average 7 percent higher than the regular OLS standard errors. Nonetheless, the coefficients of interest (α_2 and α_4) remain statistically significant at $p < 0.06$ and $p < 0.05$, respectively (two-sided tests).

Firm-wide industry specialization effects. Ernst & Young is market leader in the healthcare sector in the Netherlands in 2002. To investigate the effect of nation-wide industry leadership on audit quality, I create an indicator variable DUMEY set to 1 in case of an Ernst & Young client; 0 otherwise. After replacing EXPERT by DUMEY in Eq. (1), I find a significantly negative coefficient for DUMEY · LENGTH at $p < 0.07$ (one-sided). The coefficient for DUMEY (main effect) is positive albeit insignificantly different from zero at conventional levels. However, after excluding observations of Ernst & Young local offices with five or more clients, DUMEY · LENGTH is no longer statistically significant. This finding is consistent with evidence in Ferguson et al. (2003) suggesting that national leadership rankings are driven by the specific offices where accounting firms are city leaders.[16]

Is auditor oversight sufficient to eliminate claim-reserve bias? As in Gaver and Paterson (2007), I investigate whether auditor oversight is sufficient to *eliminate* (not just mitigate) claim-

[16] To further rule out the possibility that clients of Ernst & Young are driving my findings, I re-estimate Eq. (1) while excluding Ernst & Young observations. Results remain materially unchanged from those reported with a pool of observations from Deloitte, KPMG, and PricewaterhouseCoopers (results are untabulated). The coefficient for EXPERT · LENGTH remains significantly negative at $p < 0.07$ (one-sided). The coefficient for EXPERT is negative albeit insignificantly different from zero at conventional levels.

reserve bias. I address this question by evaluating the linear combination LENGTH · (α_2 + α_3 DEXPERT) at the mean value of LENGTH (2.8717) with DEXPERT equal to 1, and α_2 and α_3 equal to their estimated values from column 3 of Table 5. The point estimate (0.0032) is insignificantly negative, which means that industry experts eliminate claim-reserve bias. In my sample of 78, 16 observations have BIAS* equal to zero with 6 observations for which LENGTH is larger than the sample mean (i.e., the more complex claims). Each of these 6 observations is serviced by a local-office based industry expert.

Self-selection problem. A central problem with the approach of linking the selected type of auditor (in this study, having selected a local expert or not) to a hospital's third-party claim reserve practice is self-selection bias: simply put, specialists' clients differ systematically in audit complexity from their counterparts. If so, then an outcome-based measure for audit quality will have at least two components: one component reflecting (perceived) client complexity and the other component reflecting audit quality provided by the industry-specialist auditor. In econometric terms, if specialist choice is correlated with a client's unobserved heterogeneity, then standard regression analysis will give inconsistent estimates of the contribution of the specialist auditor to a client's reporting quality.

This selection problem is known as 'treatment effect' in econometrics (Greene (2003)). That is, hospitals self-select into a certain state in a nonrandom way on the basis of economic arguments. In general, hospitals that benefit the most from being in a certain state are more likely to be in this state. In consequence, it is conceivable that hospitals with complex claims self-select specialist auditors. To shed some light on this, I re-estimate a reduced form of Eq. (1) for hospitals with LENGTH ≤ 3 only (the sample mean and median) reporting a budget surplus. With 32 observations, the coefficients for EXPERT ($p > 0.10$) and LENGTH · EXPERT ($p < 0.07$;

one-sided tests) are negative. In sum, results from this test suggest that self-selection bias is not driving inferences.

6. Conclusion

A large literature documents evidence of differential audit quality (above *and* below the legal minimum) along dimensions such as industry specialization and accounting firm size. Reynolds and Francis (2001) extend this stream of research by considering auditor behavior *within* accounting firms at the practice-office level. Like Reynolds and Francis (2001), recent audit pricing studies test if the audit market prices firm-wide and/or local-office industry expertise. These studies find that auditor reputation for industry expertise is neither strictly national (i.e., firm-wide) nor strictly local in character.

This study provides guidance to this literature by investigating the degree to which such expertise is transferred within the accounting firm. In the highly-regulated hospital sector (offering a powerful research setting, where branch-specific expertise is expected to matter a priori) I analyze third-party claim reserve practices (providing an objective measure of financial-reporting quality). The empirical findings do not support the existence of positive network externalities across offices, as assumed under the firm-wide view of industry expertise. Rather, my study consistently shows across all tests that industry-specific knowledge resides decentralized (i.e., at the local practice office) in accounting firms. Specifically, I find that hospitals' tendency to under-reserve third-party claim reserves is increasing in claim complexity (using ex-post length of the claim cycle as a proxy). However, this behavior is attenuated when the hospital is audited by a local-office based industry specialist. These findings provide support for the view that auditor expertise is tied to individual professionals and is not readily captured and distributed by the firm to other offices and clients.

There are several limitations in my analysis. First, there is the question whether my findings can be generalized to other institutional settings—that is, to other countries or industries. Although I examine Dutch hospitals, my findings should be generalizable to nonprofit hospitals in the U.S. in state-level analysis. I caution the reader, however, not to (directly) generalize my findings to other industries. For instance, Dunn and Mayhew (2004) contend that the industry specialist's monitoring role depends on the level of regulation in the industry. Second, rational economics suggests accounting firms will spread industry-specific knowledge throughout the firm to offer higher-quality services *or* to reduce audit production costs (Hogan and Jeter (1999)). I do not investigate the latter effect for lack of data but focus on differential audit quality instead. It is conceivable, though, that industry-knowledge transfer within an accounting firm shows in reduced audit production costs. I leave this riveting question to future research.

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TABLE 1
Summary of Estimated Budget Deficit/Surplus for *Universitair Medisch Centrum* (UMC) Utrecht

	Fiscal year					
	2002		2003		2004	
Annual report						
2002	**16,956**	a				
2003	4,569	b	**-7,939**	a		
2004	1,483	b	8,133	b	**6,929**	a
2005		c	1,567	b	1,534	b
2006				d	566	b
2007				d	-1,335	b
2002-reserve bias						
$R_{2002}(D/S_{2002})$	16,956					
$A_{2005}(D/S_{2002})$	23,008	*				
BIAS	6,052					

Excerpted from the 2002-2007 *Universitair Medisch Centrum* (UMC) Utrecht Annual Statements. UMC is among the largest hospitals in the Netherlands with nearly 10,000 employees. All Euro amounts are in thousands. A negative (positive) sign indicates a budget surplus (deficit).

*) 16,956 (i.e., initial claim reserve; $R_{2002}(D/S_{2002})$) plus the sum of subsequent corrections (4,569 + 1,483).

Stages of settlement

a Initial internal estimation
b Correction (updated internal estimation)
c Definitively settled by NZa and health insurers
d Claim still pending (not definitively settled yet by NZa and/or health insurers)

TABLE 2
Sample Selection

Not-for-profit hospitals in the Netherlands [a]		124
Less:		
Mixed-healthcare organizations	32	
Observations with missing data	11	
Non-Big Four auditors' clients	3	
		(46)
Final sample		78

[a] The healthcare market in the Netherlands for calendar year 2002.

TABLE 3
Descriptive Statistics for the Sample of 78 Not-For-Profit Hospitals in 2002

Panel A: Hospital-specific, firm-wide, and office-level variables

	Mean	Std. Dev.	Median	Lower quartile	Upper quartile
Hospital-specific					
BIAS*	-0.0147	0.0417	-0.0021	-0.0174	0
UNDER	0.5384	0.5017	1	0	1
LENGTH	2.8717	1.1882	3	2	4
FTE	1,591	1,444	1,102	745	2,016
LPROFIT	0.3205	0.4696	0	0	1
CORPGOV	49.2	28.1	48.5	27	74
DLEV	0.4871	0.5030	0	0	1
DWFZ	0.3717	0.4864	0	0	1
Firm-wide					
OFFICES	8	3.1622	7.5	5.75	9.75
FWCLIENTS	15.8	10.1587	14	14	23.5
Office-level					
EXPERT	0.0547	0.0391	0.0520	0.0160	0.0756
OLCLIENTS	4.2051	2.5141	4	2	7
W_EXPERT	0.0127	0.0080	0.0126	0.0054	0.0196

Continued on next page

TABLE 3 (Continued)

Panel B: Number of clients per local practice office

	Deloitte	E&Y	PWC	KPMG
Amersfoort	2			
Amstelveen	1			8
Amsterdam		7	5	
Arnhem		2		
Den Bosch			1	
Den Haag		7	6	3
Dordrecht		1		
Eindhoven	4	1		1
Enschede	1			1
Groningen	2	2	3	1
Leeuwarden		1		
Maastricht	1			
Middelburg	1	1	2	
Nijmegen		1		
Roermond	1			
Tilburg		3		
Utrecht		1	5	
Venlo		1		
Zwolle	1			
	14	28	22	14

BIAS* is the reverse-coded version of BIAS for deficit-cases. BIAS is the four-year claim-reserve estimation error as a percentage of lagged PPE (property, plant, and equipment; as reported in the 2002 annual report). By changing signs of BIAS for deficit-cases, now all negative (positive) biases are in (not in) accordance with auditors' incentives to plan higher levels of audit investment to detect and correct overstatements. UNDER is an indicator variable set to one if BIAS* is negative, and zero otherwise. LENGTH is a proxy for claim complexity, measured as the ex-post length of the claim cycle. If the claim process is still pending by the end of the four-year observation period, LENGTH is set equal to 5. FTE is the number of full-time employees over 2002. LPROFIT equals one if reported net income over 2002 (before adjustments to budgets from prior years) is positive and larger than 2.5 percent of lagged PPE; zero otherwise. CORPGOV is measured as a hospital's rank in the 2003 Ernst & Young report (with lower ranks representing higher-quality governed hospitals). DLEV is computed as the ratio of total debt to equity. DWFZ is equal to 1 if a hospital participates in a garantee fund for hospitals (data from *Waarborgfonds voor de Zorgsector*) in 2002; 0 otherwise. OFFICES is the number of local practice offices per audit firm. FWCLIENTS is the number of sample-nonprofit hospital clients per audit firm. EXPERT $\equiv \sum_{i=1}^{I_j}\sqrt{REV_{ij}} \Big/ \sum_{j}^{J}\sum_{i=1}^{I_j}\sqrt{REV_{ij}}$, where REV is revenue for hospital *i* as audited by local audit office *j*. OLCLIENTS is the number of sample-nonprofit hospital clients per local practice office. W_EXPERT is $\equiv \sum_{i=1}^{I_j}\left(\sqrt{REV_{ij}} \times TENURE_{ij}\right) \Big/ \sum_{j}^{J}\sum_{i=1}^{I_j}\left(\sqrt{REV_{ij}} \times TENURE_{ij}\right)$, where TENURE is an indicator variable set to 3 if a hospital did not switch auditors during any of the three years (i.e., during 2000 through 2002). In case of an auditor change in 2000 (2001), TENURE is set to 2 (1). E&Y is Ernst & Young. PWC is PricewaterhouseCoopers.

TABLE 4
Summary Statistics and Breakdown by Claim Complexity and Auditor Industry Expertise

Panel A: *Budget-deficit and surplus cases* (N = 78)

DEXPERT	LENGTH ≤ 3 years	LENGTH > 3 years	
0	-0.0197 (30)	-0.0017 (7)	*
1	-0.0038 (16)	-0.0152 (25)	NS
	NS	*	

Panel B: *Budget-surplus cases* (N = 56)

DEXPERT	LENGTH ≤ 3 years	LENGTH > 3 years	
0	-0.0238 (23)	-0.0045 (5)	*
1	-0.0116 (9)	-0.0224 (19)	NS
	NS	*	

BIAS* is the reverse-coded version of BIAS for deficit-cases. BIAS is the four-year claim-reserve estimation error as a percentage of lagged PPE (property, plant, and equipment; as reported in the 2002 annual report). By changing signs of BIAS for deficit-cases, now all negative (positive) biases are in (not in) accordance with auditors' incentives to plan higher levels of audit investment to detect and correct overstatements. Differences in means are tested using a simple t-test. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

Variable definitions

LENGTH is a proxy for claim complexity, measured as the ex-post length of the claim cycle. If the claim process is still pending by the end of the four-year observation period, LENGTH is set equal to 5. DEXPERT is an indicator variable set to one if EXPERT is larger than the sample mean, zero otherwise. EXPERT $\equiv \sum_{i=1}^{I_j}\sqrt{REV_{ij}} \Big/ \sum_{j}^{J}\sum_{i=1}^{I_j}\sqrt{REV_{ij}}$, where REV is revenue for hospital *i* as audited by local audit office *j*.

TABLE 5
Results of Regressing Reserve Bias on Claim Cycle Length, Auditor Expertise, and Control Variables

$$BIAS_i^* = \alpha_0 + \alpha_1 UNDER_i + \alpha_2 LENGTH_i + \alpha_3 DEXPERT_i + \alpha_4 (LENGTH_i \cdot DEXPERT_i) + \alpha_5 \text{Controls}_i + \varepsilon_i$$

Model specification [a]	(1)	(2)	(3)	(4)	(5)	(6)
Variables [b]						
UNDER	-0.4723	-0.4923	-0.4967	-0.4088	-0.4263	-0.8256
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
LENGTH	0.2870	0.3754	0.3580	0.4426	0.3540	0.2053
	(0.134)	(0.093)	(0.072)	(0.074)	(0.103)	(0.049)
DEXPERT	-0.0227	-0.0837	-0.0646	-0.1188	-0.0753	-0.0794
	(0.382)	(0.243)	(0.279)	(0.167)	(0.285)	(0.346)
LENGTH · DEXPERT	-0.2505	-0.3041	-0.2846	-0.3440	-0.2911	-0.2079
	(0.033)	(0.023)	(0.024)	(0.020)	(0.046)	(0.031)
SIZE	-	-	-0.0222	-	0.0759	-0.0088
			(0.769)		(0.437)	(0.894)
LPROFIT	-	-	-0.2852	-	-0.3125	-0.0656
			(0.028)		(0.060)	(0.397)
DWFZ	-	-	0.0614	-	0.0499	0.0087
			(0.435)		(0.640)	(0.893)
CORPGOV	-	-	-0.0608	-	0.0745	-0.0355
			(0.635)		(0.626)	(0.674)
DLEV	-	-	-0.1423	-	-0.1502	-0.0722
			(0.256)		(0.293)	(0.377)
Intercept	Included	Included	Included	Included	Included	Included
Fixed auditor effects	-	Included	Included	Included	Included	Included
R-squared	0.2758	0.3345	0.4301	0.3266	0.4416	0.7038
F-statistic	6.60	5.06	2.76	3.19	1.96	15.89
	(0.000)	(0.000)	(0.004)	(0.007)	(0.052)	(0.000)
N	78	78	78	56	56	78

TABLE 5 (Continued)

Reported are standardized coefficients and p-values in parentheses. The p-values (two-sided) are based on the White (1980)'s heteroskedasticity-corrected covariance matrix. One-tailed p-values are reported for DEXPERT and LENGTH · DEXPERT.

a Model specification DEXPERT is an indicator variable set to one if EXPERT is larger than the sample mean, zero otherwise. EXPERT ≡ $\sum_{i=1}^{I_j}\sqrt{REV_{ij}} \Big/ \sum_{j}^{J}\sum_{i=1}^{I_j}\sqrt{REV_{ij}}$, where REV is revenue for hospital *i* as audited by local audit office *j*. Column (6) reports results from a rank regression. I transform BIAS* into ranks, with the smallest (largest) magnitude observation having a value of 1 (N[pos] ≡ number of observations for which BIAS > 0). Similarly, negative values of BIAS* are transformed into ranks with the smallest (largest) magnitude observation having a value of *minus* 1 (*minus* 1 times N[neg] ≡ number of observations for which BIAS < 0). For BIAS* = 0, rank BIAS* also equals zero. Columns (4) and (5) report results for surplus cases only (i.e., in case of a liability).

b Variable definitions BIAS* is the reverse-coded version of BIAS for deficit-cases. BIAS is the four-year claim-reserve estimation error as a percentage of lagged PPE (property, plant, and equipment; as reported in the 2002 annual report). By changing signs of BIAS for deficit-cases, now all negative (positive) biases are in (not in) accordance with auditors' incentives to plan higher levels of audit investment to detect and correct overstatements. UNDER is an indicator variable set to one if BIAS* is negative, and zero otherwise. LENGTH is a proxy for claim complexity, measured as the ex-post length of the claim cycle. If the claim process is still pending by the end of the four-year observation period, LENGTH is set equal to 5. FTE is the number of full-time employees over 2002. LPROFIT equals one if reported net income over 2002 (before adjustments to budgets from prior years) is positive and larger than 2.5 percent of lagged PPE; zero otherwise. CORPGOV is measured as a hospital's rank in the 2003 Ernst & Young report (with lower ranks representing higher-quality governed hospitals). DLEV is computed as the ratio of total debt to equity. DWFZ is equal to 1 if a hospital participates in a garantee fund for hospitals (data from *Waarborgfonds voor de Zorgsector*) in 2002; 0 otherwise.

TABLE 6
Results of Regressing Reserve Bias on Claim Cycle Length and Auditor Expertise within Accounting Firms

$$BIAS_i^* = \alpha_0 + \alpha_1 UNDER_i + \alpha_2 LENGTH_i + \alpha_3 EXPERT_i + \alpha_4 \left(LENGTH_i \cdot EXPERT_i\right) + \varepsilon_i$$

	Ernst & Young			PricewaterhouseCoopers		
Model specification [a]	(1)	(2)	(3)	(4)	(5)	(6)
Variables [b]						
Intercept	Included	Included	Included	Included	Included	Included
UNDER	-0.5757	-0.8923	-0.7890	-0.4352	-0.6855	-0.6518
	(0.008)	(0.000)	(0.000)	(0.027)	(0.000)	(0.000)
LENGTH	-0.0713	0.0257	0.1567	0.5269	0.5098	0.5641
	(0.834)	(0.438)	(0.232)	(0.147)	(0.026)	(0.025)
EXPERT	-0.0240	-0.0035	-0.0771	-0.0550	-0.1209	-0.2318
	(0.429)	(0.485)	(0.262)	(0.371)	(0.243)	(0.126)
LENGTH · EXPERT	-0.1243	-0.2032	-0.2315	-0.4102	-0.3663	-0.4269
	(0.281)	(0.064)	(0.090)	(0.121)	(0.075)	(0.081)
R-squared	0.3328	0.7739	0.7087	0.3520	0.6342	0.6378
F-statistic	2.84	20.58	19.35	1.99	11.34	9.83
	(0.047)	(0.000)	(0.000)	(0.141)	(0.000)	(0.000)
N	28	28	18	22	22	17

Reported are standardized coefficients and p-values in parentheses. The p-values (two-sided) are based on the White (1980)'s heteroskedasticity-corrected covariance matrix. One-tailed p-values are reported for DEXPERT and LENGTH · DEXPERT.

a Model specification Columns (2)-(3) and (5)-(6) report results from rank regressions. Specifically, I transform BIAS* into ranks, with the smallest (largest) magnitude observation having a value of 1 (N[pos] ≡ number of observations for which BIAS* > 0). Similarly, negative values of BIAS* are transformed into ranks with the smallest (largest) magnitude observation having a value of *minus* 1 (*minus* 1 times N[neg] ≡ number of observations for which BIAS* < 0). For BIAS* = 0, rank BIAS* also equals zero. Columns (3) and (6) include only surplus cases.

b Variable definitions BIAS* is the reverse-coded version of BIAS for deficit-cases. BIAS is the four-year claim-reserve estimation error as a percentage of lagged PPE (property, plant, and equipment; as reported in the 2002 annual report). By changing signs of BIAS for deficit-cases, now all negative (positive) biases are in (not in) accordance with auditors' incentives to plan higher levels of audit investment to detect and correct overstatements. UNDER is an indicator variable set to one if BIAS* is negative, and zero otherwise. LENGTH is a proxy for claim complexity, measured as the ex-post length of the claim cycle. If the claim process is still pending by the end of the four-year observation period, LENGTH is set equal to 5.

APPENDIX A
Budget System and Financing Deficit/Surplus[17]

The healthcare sector in the Netherlands can be subdivided into a Care-sector and a Cure-sector. In general the costs of the Care-sector are defrayed on grounds of the *AWBZ* (a social insurance of medical expenses), whereas the Cure-sector is defrayed on the basis of the *ZFW* (medicaid).

Over 2002 (the year of observation in the study), payment of the Care-sector proceeds roughly as follows. A percentage *AWBZ* contribution (with a maximum) is deducted from employees' gross wages by the employer. The employer remits these contributions to an administrative body enforcing social insurances. This agency deposits the contribution into the *AWBZ* fund. A regional unit of health insurers (*Zorgkantoor*) annually allocates a (prima facie) external budget to hospitals certified by the *AWBZ*. As executor of the *AWBZ* this unit commissions a monthly payment of the annual budget to the hospital. By the end of the fiscal year the hospital submits a costing to the unit. Both the hospital and the unit file this costing to the NZa. The NZa determines the final external budget. Any difference between the final and prima facie budget needs to be settled in the near future.

Over 2002, the Cure-sector is financed as follows. Employers deduct a percentage *ZFW* contribution (with a maximum) from *ZFW*-insurants' gross wages and remit these contributions to an administrative body enforcing social insurances. These contributions are then allocated to health insurers in the Netherlands on the grounds of distribution codes. The health insurers collect so-called 'nominal contributions' from their insurants. Together these aforementioned contributions comprise a health insurer's receipts. Hospitals and/or insurants claim medical care from the health insurers. Tariffs for medical care are determined by the NZa.

The following example illustrates some elementary features of the external budgeting in hospitals. Suppose a hospital has 500 hospital beds at its disposal. With permission from the central government the hospital invested €100 million. Annual depreciations amount to €3 million.

Step 1—Production agreement

Health insurers and the hospital stipulate the following terms: (1) the hospital has 500 hospital beds at its disposal by the government; (2) the hospital will provide 150,000 hospital days; and (3) the hospital will perform 5,000 surgeries.

[17] This section is taken from Schaepkens (2000) with the author's permission.

Step 2—External budget

Terms	*Parameter value (in €)*	*Budget (in €)*
500 hospital beds	150,000	75,000,000
150,000 hospital days	200	30,000,000
Depreciations		3,000,000
Total budget		*108,000,000*

Step 3—Tariffs

Tariffs are determined on the basis of the external budget of €108 million. The tariff for surgeries is €5,600 a piece. Hence, 5,000 surgeries will yield €28 million. The residual external budget amounts to €80 million (€108 million -/- €28 million). Allocation to 150,000 hospital days leads to a hospital fee of €533 (rounded) per day. Now the hospital can employ two tariffs: €5,600 per surgery and €533 per hospital day.

Step 4—Performance and billing

The hospital provides 140,000 hospital days and performs 5,100 surgeries. The hospital will charge her patients:

Performance	*Tariff (in €)*	*'Covered' (in €)*
5,100 surgeries	5,600	28,560,000
140,000 hospital days	533	74,620,000
Total charged		*103,180,000*

Step 5—Financing deficit/surplus

The financing deficit is computed as the external budget (€108,000,000) less the value of hospital performance charged to patients (€103,180,000)—receipts to cover the external budget. The difference, €4,820,000, is called the 'financing deficit'.

Step 6—Settlement of claims by the health insurers and/or NZa

Audits by health insurers and/or the NZa after a hospitals' fiscal-year end can lead to denied or adjudicated claims. Further, the ultimate rates paid by health insurers are subject to retrospective adjustments.

Step 7—Processing the financing deficit/surplus

The deficit of €4,820,000 is used as a 'raise' on next year's tariffs. For instance, suppose that the number of hospital days for next year is settled at 145,000. Then next year's tariff per hospital day will increase by €33 (rounded) in addition to the regular fee. No adjustments are made for the number of hospital days and/or surgeries if those deviate in practice from the initial production agreement.

The figure in Appendix B illustrates the possible outcomes of the claim-settlement process and how it affects current and next year's earnings.

APPENDIX B
Accounting Trail of Third-Party Claim Reserve in a Hospital's Financial Statement

EARN represents a hospital's performance during period *t*. R(D/S) stands for the third-party claim reserve as reported in the annual statement. *D* represents a deficit (i.e., Budget > EARN) and *S* a surplus (i.e., Budget < EARN). BIAS is measured as Actual (i.e., the ex-post outcome) -/- R(D/S). BIAS* is the reverse-coded version of BIAS for deficit-cases. BIAS is the four-year claim-reserve estimation error as a percentage of lagged PPE (property, plant, and equipment; as reported in the 2002 annual report). By changing signs of BIAS for deficit-cases, now all negative (positive) biases are in (not in) accordance with auditors' incentives to plan higher levels of audit investment to detect and correct overstatements.