Auditor Conservatism, Asymmetric Monitoring, and Earnings Management*

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Abstract

In this paper, we investigate whether, and how, audit effectiveness differentiation between Big 6 and non–Big 6 auditors is influenced by a conflict or convergence of reporting incentives faced by corporate managers and external auditors. In so doing, we incorporate into our analysis the possibility that managers self-select both external auditors and discretionary accruals, using the two stage "treatment effects" model. Our results show that only when managers have incentives to prefer income-increasing accrual choices are Big 6 auditors more effective than non–Big 6 auditors in deterring/monitoring opportunistic earnings management. Contrary to conventional wisdom, we find Big 6 auditors are less effective than non–Big 6 auditors when both managers and auditors have incentives to prefer income-decreasing accrual choices and thus no conflict of reporting incentives exists between the two parties. The above findings are robust to different proxies for opportunistic earnings management and different proxies for the direction of earnings management incentives.

Keywords Audit effectiveness; Auditor conservatism; Earnings management; Reporting incentives

Condensé

Les travaux récents de Becker, DeFond, Jiambalvo et Subramanyam (1998) et de Francis, Maydew et Sparks (1999) démontrent que les vérificateurs des Six Grands cabinets d'expertise comptable font plus efficacement obstacle à la gestion opportuniste du résultat que les vérificateurs des autres cabinets. Becker *et al.* rapportent notamment que l'ampleur des constatations discrétionnaires (de produits et de charges) est de beaucoup inférieure chez les clients des Six Grands cabinets (les Quatre Grands désormais) que chez les clients des autres cabinets, une fois contrôlées plusieurs caractéristiques propres à la société. Cette observation laisse croire que la qualité supérieure de la vérification, dont atteste l'intervention des vérificateurs des Six Grands cabinets, est associée à un contrôle efficace restreignant la

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latitude dont jouissent les cadres dans le choix de constatations opportunistes. Ils ne tiennent cependant pas compte, dans leur analyse, de la possibilité que l'effet de censure de la vérification externe soit sensible à l'orientation de la propension des cadres à gérer le résultat, c'est-à-dire à hausser ou réduire le bénéfice.

Le résultat déclaré reflète l'interaction des motivations influant sur la déclaration du résultat (ci-après les motivations de déclaration) auxquelles sont soumis deux préparateurs d'états financiers, soit les cadres d'entreprise et les vérificateurs externes. Selon les auteurs, l'opposition ou la convergence des motivations de déclaration de ces préparateurs est un facteur déterminant de l'efficacité de la vérification externe à dissuader la direction de pratiquer la gestion opportuniste du résultat (ci-après l'efficacité de la vérification). Pour recueillir des données empiriques sur ce sujet inexploré, les auteurs poussent plus loin les recherches de Becker *et al.* (1998) dans les directions suivantes.

En premier lieu, les auteurs se demandent si l'efficacité de la vérification diffère — et, le cas échéant, quelles sont ces différences — dans deux situations distinctes avec deux motivations de déclaration différentes : 1) lorsque les cadres sont enclins à surévaluer le résultat déclaré en faisant des choix de constatations qui haussent le bénéfice ; et 2) lorsqu'ils sont enclins à sous-évaluer le résultat déclaré en faisant des choix de constatations qui réduisent le bénéfice. Selon les auteurs, la vérification externe joue un rôle dissuasif efficace en ce qui a trait à la gestion opportuniste du résultat uniquement lorsque les préférences des vérificateurs en matière de choix des constatations s'opposent à celles des cadres. Un élément clé sous-tend l'argument qui précède : les préoccupations des vérificateurs à l'égard du coût potentiel des litiges incitent ces derniers à privilégier les choix comptables prudents (qui réduisent le bénéfice), ce qui, en retour, incite les vérificateurs à contrôler les choix de constatations des cadres qui haussent le bénéfice de plus près que les choix de constatations qui réduisent le bénéfice (DeFond et Jiambalvo, 1993 ; Lys et Watts, 1994 ; Krishnan, 1994). Les auteurs appellent la préférence des vérificateurs pour les choix comptables qui réduisent le bénéfice « prudence des vérificateurs ».

D'une part, la prudence des vérificateurs crée une opposition entre les motivations de déclaration des cadres et des vérificateurs, lorsque les cadres sont enclins à surévaluer le résultat déclaré en faisant des choix de constatations qui haussent le bénéfice. D'autre part, la prudence des vérificateurs entraîne la convergence des motivations de déclaration des cadres et des vérificateurs lorsque lesdits cadres sont enclins à sous-évaluer le résultat déclaré en faisant des choix de constatations qui réduisent le bénéfice. Les données expérimentales portent à conclure que les vérificateurs externes sont susceptibles de faire preuve d'un degré plus (moins) élevé de scepticisme professionnel à l'égard des choix des cadres qui haussent le bénéfice lorsqu'il y a opposition (convergence) des motivations de déclaration (Hirst, 1994). Cela tient au fait que les vérificateurs sont davantage susceptibles de faire l'objet de poursuites et, par conséquent, que les coûts prévisibles des litiges sont plus élevés dans le cas de la surévaluation que dans celui de la sous-évaluation du résultat (St. Pierre et Anderson, 1984 ; Lys et Watts, 1994).

Les auteurs avancent, au surplus, que les vérificateurs des Six Grands cabinets d'expertise comptable sont enclins à faire preuve d'une plus grande prudence que les vérificateurs des autres cabinets dans la détermination du résultat qui doit être déclaré. Les coûts potentiels des litiges qui résulteraient d'une défaillance de la vérification (y compris les atteintes à la réputation) sont susceptibles d'être plus élevés pour les vérificateurs des Six Grands que

pour les vérificateurs des autres cabinets. Par exemple, lorsqu'une défaillance de la vérification est alléguée, les vérificateurs des Six Grands sont susceptibles de faire l'objet d'une plus grande publicité dans les médias financiers que les vérificateurs des autres cabinets, et, par conséquent, leur réputation risque d'en souffrir davantage. Les tiers intéressés (notamment les actionnaires et les créanciers) croient que les vérificateurs des Six Grands disposent de ressources plus importantes et sont mieux protégés sur le plan des assurances, et ils ont donc davantage de ressources pour dédommager les plaignants dans le cadre de règlements hors cour ou s'acquitter des dommages ou des frais imposés par le tribunal. Par opposition, les tiers intéressés sont moins susceptibles de poursuivre les vérificateurs des cabinets (de taille modeste ou moyenne) qui ne comptent pas au nombre des Six Grands, étant donné que les produits du règlement des poursuites risquent de ne pas suffire à couvrir les coûts connexes. C'est pourquoi les vérificateurs des Six Grands sont davantage susceptibles de faire l'objet de poursuites que les vérificateurs des autres cabinets et tendent à subir des pertes plus élevées lorsqu'ils sont poursuivis (Simunic et Stein, 1996 ; Chung, Firth et Kim, 2003). Compte tenu que le fait de n'avoir pas décelé une surévaluation (sous-évaluation) du résultat est plus (moins) susceptible de donner lieu à des poursuites, les vérificateurs des Six Grands sont plus enclins que les vérificateurs des autres cabinets à restreindre la latitude dont jouissent les cadres dans le choix de constatations qui haussent le bénéfice. Il est donc probable que les clients des Six Grands aient moins de latitude que ceux des autres cabinets dans le choix de constatations entraînant la hausse du bénéfice.

La prudence des vérificateurs, ainsi que l'absence de risque de litige advenant qu'une sous-évaluation du résultat ne soit pas décelée, suppose que les vérificateurs seront moins motivés à décourager ou contrôler les choix de constatations qui réduisent le bénéfice si les cadres (et les vérificateurs) privilégient des choix comptables réduisant le bénéfice. Les cadres qui privilégient les choix comptables réduisant le bénéfice jouiront donc d'une plus grande latitude dans la gestion opportuniste du résultat que ceux qui privilégient les choix comptables haussant le bénéfice. Déterminer s'il existe une différence dans l'efficacité de la vérification selon qu'elle est effectuée par des vérificateurs appartenant aux Six Grands ou des vérificateurs d'autres cabinets lorsque les cadres préfèrent des choix comptables prudents (qui réduisent le bénéfice) relève cependant de l'étude empirique. Les auteurs de la présente étude produisent, relativement à cette question, des données empiriques qui n'ont pas été analysées dans les travaux précédents.

En second lieu, les auteurs poussent plus loin les recherches de Becker *et al.* (1998) en tenant compte explicitement de la distorsion de « sélection personnelle » découlant du fait que les cadres choisissent eux-mêmes les vérificateurs externes (ainsi que les constatations discrétionnaires). Dans leur modèle empirique liant les constatations discrétionnaires aux choix des vérificateurs, Becker *et al.* supposent que le choix que font les cadres de vérificateurs appartenant ou non aux Six Grands est d'origine exogène. Toutefois, ce sont les cadres qui choisissent le vérificateur externe après avoir pris en compte les coûts et les avantages que suppose le recours à des vérificateurs qui appartiennent ou n'appartiennent pas aux Six Grands. Ainsi donc, les choix de constatations aussi bien que les choix de vérificateurs sont endogènes. Conformément au modèle de Becker *et al.*, une simple régression des moindres carrés classiques de l'ampleur des constatations discrétionnaires sur la variable assignée du choix du vérificateur (ainsi que d'autres variables propres à l'entreprise) peut donc donner lieu à une distorsion de sélection personnelle. Les auteurs de la présente étude se penchent

sur la question de la sélection personnelle en estimant un modèle des « effets du traitement » en deux étapes (Barnow, Cain et Goldberger, 1980 ; Greene, 1997 ; Leuz et Verrecchia, 2000 ; Leuz, 2001). Dans un premier temps, ils estiment un modèle probit de choix du vérificateur et calculent les rapports inverses de Mills. Dans un second temps, afin de contrôler la distorsion potentielle de sélection personnelle, ils intègrent les rapports inverses de Mills à leur modèle de choix des constatations, en liant les constatations discrétionnaires au choix des vérificateurs et à d'autres variables de contrôle propres à l'entreprise.

Enfin, Becker *et al.* (1998) utilisent les constatations discrétionnaires estimées à l'aide du modèle transversal de Jones (1991) comme seul substitut à la gestion opportuniste du résultat. Pour vérifier la robustesse de leurs résultats, les auteurs se servent également des constatations anormales relatives au fonds de roulement, c'est-à-dire les écarts entre les montants réels et prévus des constatations relatives au fonds de roulement, comme autre substitut à la gestion opportuniste du résultat. Ainsi qu'ils l'expliquent, leur évaluation des constatations anormales relatives au fonds de roulement n'a aucun lien avec les erreurs de mesure potentielles associées aux estimations des paramètres du modèle de Jones. Compte tenu des critiques dont a récemment fait l'objet l'estimation des constatations discrétionnaires à l'aide du modèle de Jones (par ex., Bernard et Skinner, 1996), l'utilisation de cet autre substitut accroît la validité des résultats obtenus par les auteurs.

En bref, ces résultats indiquent que ce n'est que lorsque les cadres sont motivés à hausser le bénéfice déclaré que les vérificateurs des Six Grands cabinets sont plus efficaces que les vérificateurs des autres cabinets pour ce qui est de dissuader leurs clients de pratiquer la gestion opportuniste du résultat ou d'en contrôler la pratique. Comme Becker *et al.* (1998), les auteurs constatent que lorsque les cadres privilégient les choix de constatations qui haussent le bénéfice, les vérificateurs appartenant aux Six Grands sont plus efficaces que les vérificateurs des autres cabinets à restreindre la latitude dont jouissent les cadres dans le choix de constatations qui haussent le bénéfice. Chose étonnante, les auteurs font remarquer que les vérificateurs appartenant aux Six Grands sont moins efficaces que les vérificateurs des autres cabinets lorsque les cadres (ainsi que les vérificateurs) privilégient les choix de constatations qui réduisent le bénéfice. Les résultats qui précèdent résistent à l'application d'une batterie de tests de sensibilité, notamment à l'utilisation de substituts différents pour la gestion opportuniste du résultat et pour l'orientation de la propension des cadres à gérer le résultat. Dans l'ensemble, les résultats auxquels arrivent les auteurs indiquent clairement que l'écart d'efficacité des vérificateurs appartenant aux Six Grands par rapport à ceux des autres cabinets est sensible à l'orientation de la propension des cadres à gérer le résultat.

La présente étude vient enrichir le fonds des travaux existants sur les écarts de qualité des services de vérification en définissant des circonstances précises dans lesquelles les vérificateurs des Six Grands sont moins efficaces que les vérificateurs des autres cabinets. Les études précédentes sur les écarts de qualité des services de vérification supposaient implicitement que les vérificateurs des Six Grands assuraient des services de vérification de qualité supérieure, sans égard à l'opposition ou à la convergence des motivations de déclaration des préparateurs des états financiers (par exemple, Beatty, 1989 ; Teoh et Wong, 1993 ; Becker *et al.*, 1998 ; Francis et Krishnan, 1999 ; Francis *et al.*, 1999). Les résultats obtenus par les auteurs révèlent cependant que les vérificateurs des Six Grands sont plus efficaces que les vérificateurs des autres cabinets uniquement lorsqu'il y a opposition entre les motivations de déclaration des deux préparateurs.

Les conclusions de la présente étude corroborent les données expérimentales de Hirst (1994) selon lesquelles les vérificateurs sont, en général, en mesure d'« établir la distinction entre les explications fournies par les clients dont la motivation à gérer le résultat diffère ». Bien que l'expérience de Hirst soit axée sur la sensibilité de l'ensemble des vérificateurs externes aux motivations des cadres à la gestion opportuniste du résultat, les auteurs arrivent à des résultats qui donnent en outre à penser que la capacité des vérificateurs externes de discriminer les différentes motivations de déclaration des cadres peut différer selon qu'il s'agit de vérificateurs des Six Grands ou de vérificateurs d'autres cabinets. Enfin, à la connaissance des auteurs, cette étude est la première dont le plan de recherche incorpore explicitement le facteur de la sélection personnelle ou de l'endogénéité dans l'analyse des choix discrétionnaires de constatations.

1. Introduction

Recent research by Becker, DeFond, Jiambalvo, and Subramanyam 1998 and Francis, Maydew, and Sparks 1999 provides evidence that Big 6 auditors are more effective in constraining opportunistic earnings management than non–Big 6 auditors. In particular, Becker et al. (1998) report that the level of discretionary accruals is significantly lower for Big 6 (now Big 4) clients than for non–Big 6 clients after controlling for several firm-specific characteristics. Their finding suggests that high audit quality, proxied by Big 6 auditors, is associated with effective monitoring, which limits managers' ability to make opportunistic accrual choices. However, their analysis does not take into account the possibility that the monitoring effect of external auditing is sensitive to the direction of managerial incentives for earnings management — namely, income-increasing versus income-decreasing incentives.

Reported earnings reflect the interaction of income-reporting incentives (hereafter "reporting incentives") faced by two issuers of financial statements — corporate managers and external auditors. In this paper, we contend that the conflict or convergence of reporting incentives between the two issuers is an important factor determining the effectiveness of external auditing for deterring opportunistic earnings management (hereafter "audit effectiveness"). To provide empirical evidence on this unexplored issue, we extend Becker et al. 1998 in the following ways.

First, we investigate the question whether, and how, audit effectiveness differs between two distinct situations with two different reporting incentives: (1) when managers have incentives to overstate reported earnings through income-increasing accrual choices; and (2) when they have incentives to understate reported earnings through income-decreasing accrual choices. We argue that external auditing acts as an effective deterrent to opportunistic earnings management only when auditors' preferences over accrual choices conflict with managers' preferences. A key point underlying this argument is that auditors' concerns over potential litigation costs motivate them to prefer conservative (or income-decreasing) accounting choices, which in turn creates auditors' incentives to monitor managers' income-increasing accrual choices more closely than income-decreasing accrual choices (DeFond and Jiambalvo 1993; Lys and Watts 1994; Krishnan 1994). We call auditors' preference for income-decreasing accounting choices "auditor conservatism".

On the one hand, auditor conservatism creates a conflict of reporting incentives between managers and auditors, when managers have incentives to overstate reported earnings through income-increasing accrual choices. On the other hand, auditor conservatism leads to a convergence of reporting incentives between the two issuers of financial statements when managers have incentives to understate reported earnings through income-decreasing accrual choices. Experimental evidence suggests that external auditors are likely to exercise a more (less) heightened degree of professional skepticism on managers' income-increasing choices when a conflict (convergence) of reporting incentives arises (Hirst 1994). This occurs because auditors are more likely to be sued, and thus expected litigation costs are higher, for income overstatement than for income understatement (St. Pierre and Anderson 1984; Lys and Watts 1994).

We further argue that Big 6 auditors have incentives to be more conservative than non–Big 6 auditors in determining reported earnings. Potential litigation costs associated with audit failure (including a loss of reputation capital) are likely to be higher for Big 6 auditors than for non–Big 6 auditors. For example, when the allegation of audit failure arises, Big 6 auditors are likely to face greater publicity in the financial media, and thus they are likely to bear a greater reputation loss, than non–Big 6 auditors. Outside stakeholders (e.g., shareholders and creditors) believe that Big 6 auditors have "deeper pockets" and higher insurance coverage, and thus they have more resources to recompense the plaintiffs through out-of-court settlements or through court-awarded damages or costs. In contrast, outside stakeholders are less likely to sue non–Big 6 (small or medium-sized) auditors, since expected benefits from the settlement of lawsuits may be insufficient to cover the associated costs. For these reasons, Big 6 auditors are more likely to be sued, and they tend to suffer from larger potential losses if sued, than non–Big 6 auditors (Simunic and Stein 1996; Chung, Firth, and Kim 2003). Given that a lawsuit is more (less) likely for failure to detect income overstatement (understatement), Big 6 auditors have a greater incentive to limit managers' ability to choose income-increasing accruals than non–Big 6 auditors. It is thus likely that Big 6 clients are allowed less flexibility for income-increasing accrual choices than non–Big 6 clients.

Auditor conservatism, along with the lack of litigation risk in the event of failure to detect income understatement, implies that auditors' incentives to deter/monitor income-decreasing accrual choices diminish when managers (as well as auditors) prefer income-decreasing accounting choices. This leaves more room and flexibility for opportunistic earnings management when managers prefer income-decreasing accounting choices than when managers prefer income-increasing accounting choices. It is an empirical question, however, whether there exists any difference in audit effectiveness between Big 6 and non–Big 6 auditors when managers prefer conservative (income-decreasing) accounting choices. In this paper, we provide empirical evidence on the above question that has not been explored in the extant literature.

Second, we further extend Becker et al. 1998 by explicitly taking into account the issue of self-selection bias arising from the fact that managers self-select external auditors (as well as discretionary accruals). In their empirical model linking

discretionary accruals with auditor choices, Becker et al. (1998) assume that managers' choice of Big 6 or non–Big 6 auditors is exogenously given. However, it is managers who choose the external auditor after considering the costs and benefits of hiring Big 6 or non–Big 6 auditors. This indicates that both accrual choices and auditor choices are endogenous. As modeled in Becker et al., a simple ordinary least squares (OLS) regression of the level of discretionary accruals on the auditor choice dummy (along with other firm-specific variables) may thus create self-selection bias. In this paper, we address the issue of self-selection by estimating a two-stage "treatment effects" model (Greene 1997; Leuz and Verrecchia 2000; Leuz 2001). In the first stage, we estimate a probit auditor-choice model, and compute inverse Mills ratios. In the second stage, in an attempt to control for potential self-selection bias, we include the inverse Mills ratios in our accrual choice model, linking discretionary accruals with auditor choices and other firm-specific control variables.

Finally, Becker et al. (1998) use discretionary accruals estimated from the cross-sectional Jones 1991 model as a single proxy for opportunistic earnings management. As a robustness check, we also use abnormal working capital accruals (i.e., differences between actual and expected amounts of working capital accruals as an additional proxy for opportunistic earnings management). As will be further explained later, our measure of abnormal working capital accruals is independent of potential measurement errors associated with estimates of the Jones 1991 model parameters. Given recent criticisms of the Jones 1991 model estimation of discretionary accruals (e.g., Bernard and Skinner 1996), the use of this alternative proxy enhances the validity of our findings.

Briefly, our results show that only when managers have incentives to boost reported earnings are Big 6 auditors more effective than non–Big 6 auditors in deterring/monitoring opportunistic earnings management. Consistent with Becker et al. 1998, we find that when managers prefer income-increasing accrual choices, Big 6 auditors are better able to limit managers' ability to choose income-increasing accruals than non–Big 6 auditors. Surprisingly, we find that Big 6 auditors are less effective than non–Big 6 auditors when managers (as well as auditors) prefer income-decreasing accrual choices. These findings are robust to a battery of sensitivity tests, including different proxies for opportunistic earnings management and different proxies for the direction of managerial incentives for earnings management. Overall, our results strongly indicate that audit effectiveness differentiation between Big 6 and non–Big 6 auditors is sensitive to the direction of managers' incentives for earnings management.

This paper adds to the existing literature on audit quality differentiation by identifying a certain condition under which Big 6 auditors are less effective than non–Big 6 auditors. Previous research on audit quality differentiation implicitly assumes that Big 6 auditors provide high-quality audits, regardless of whether there exists the divergence or convergence of reporting incentives between the two issuers of financial statements (e.g., Beatty 1989; Teoh and Wong 1993; Becker et al. 1998; Francis and Krishnan 1999; Francis et al. 1999). Our results indicate, however, that Big 6 auditors are more effective than non–Big 6 auditors only when the conflict of reporting incentives exists between the two issuers.

Our results corroborate Hirst's 1994 experimental evidence that auditors are, in general, able to "distinguish between explanations provided by clients with different incentives to manage earnings" (407). While his experiment focuses on the sensitivity of external auditors in general to managerial incentives for opportunistic earnings management, our results further suggest that the ability of external auditors to discriminate between managers' different reporting incentives may differ between Big 6 and non–Big 6 auditors. Finally, to our knowledge, this paper is the first that explicitly incorporates into the research design the issue of self-selection or endogeneity in the context of discretionary accrual choices.

The rest of the paper is structured as follows. In section 2, we discuss measurements of variables, including empirical proxies for the level of earnings management and managerial incentives for earnings management. In section 3, we describe test procedures, including sample selection and empirical models for auditor choices and accrual choices. In section 4, we present our main empirical results. In section 5, we report results of various sensitivity tests. The final section provides a summary and concluding remarks.

2. Measurement of variables

Proxies for earnings management

In this paper, we use two different proxies for earnings management: (1) discretionary accruals (*DAC*); and (2) abnormal working capital accruals (*AWCA*). To estimate *DAC*, we first compute total accruals as the change in noncash current assets minus the change in operating current liabilities minus depreciation and amortization expenses. Formally,

$$TAC_{jt} = (\Delta CA_{jt} - \Delta Cash_{jt}) - (\Delta CL_{jt} - \Delta STD_{jt} - \Delta TP_{jt}) - Dep_{jt} \quad (1)$$

where, for firm j in year t:

TAC = total accruals;

ΔCA = change in current assets;

$\Delta Cash$ = change in cash and cash equivalents;

ΔCL = change in current liabilities;

ΔSTD = change in long-term debt included in current liabilities;

ΔTP = change in taxes payable; and

Dep = depreciation and amortization expenses.

Note that in (1), the first two terms on the right-hand side represent working capital accruals that are short-term in nature.

To decompose total accruals (*TAC*) into two parts (i.e., nondiscretionary and discretionary accruals), we estimate an extended version of the modified Jones 1991 model given below:

$$TAC_{jt}/A_{jt-1} = \alpha[1/A_{jt-1}] + \beta[(\Delta REV_{jt} - \Delta AR_{jt})/A_{jt-1}] + \gamma[PPE_{jt}/A_{jt-1}] + \delta[\Delta CFO_{jt}/A_{jt-1}] + e_{jt} \quad (2)$$

where, for firm j in year t:

A_{jt-1} = lagged total assets;

ΔREV_{jt} = change in sales revenues;

ΔAR_{jt} = change in account receivables;

PPE_{jt} = property, plant, and equipment;

ΔCFO_{jt} = change in cash flow from operations; and

e_{jt} = unspecified random factors.

Note that (2) differs from the modified Jones 1991 model that has been widely used in previous research (e.g., DeFond and Park 1997; Bartov, Gul, and Tsui 2000). Similar to Kaznik 1999, we include in (2) the change in operating cash flows as an additional explanatory variable because it is negatively correlated with total accruals (Dechow 1994). We estimate regression parameters in (2) using cross-sectional observations for each year and industry (based on two-digit Standard Industrial Classification [SIC] codes), because Bartov et al. (2000) show that cross-sectional estimation is superior to time-series estimation in detecting opportunistic earnings management. Nondiscretionary accruals (*NDAC*) are defined as the fitted value from (2). Discretionary accruals (*DAC*) are defined as the residual of (2) — that is, the difference between *TAC* and its fitted value from (2).[1] As in many other studies, *DAC* is considered to be an outcome of managers' opportunistic accrual choices. As will be further explained later, the inclusion of ΔCFO in (2) alleviates a concern that our measure of *DAC* is mechanically correlated with cash flows from operations.

Proxies for income-increasing and income-decreasing incentives

To examine how the direction of managers' reporting incentives affects audit effectiveness, we need to identify two distinct situations with different incentives, that is, the situations in which managers have incentives to prefer income-increasing versus income-decreasing accrual choices. For this purpose, we rely on a theory of income smoothing by Fudenberg and Tirole 1995. Their theory predicts that managers whose tenures are subject to current performance have an incentive to boost current earnings in poor times by borrowing against future earnings to mitigate the likelihood of dismissal. However, managers have an incentive to save current earnings in good times for use in future poor times because current good performance does not necessarily compensate for future poor performance due to the so-called information decay phenomenon. Building on Fudenberg and Tirole's 1995 theory, we measure managerial incentives to boost or reduce reported earnings on the basis of current relative performance. We assume that managers have income-increasing

(income-decreasing) incentives if the current period's performance relative to the benchmark performance (hereafter current relative performance) is poor (good).

Current relative performance is measured by the difference between operating cash flows in the current year divided by lagged total assets (*CFO*) and the median *CFO* performance of samples in the same year in each industry to which a firm belongs. Industry is defined using the two-digit SIC. A firm's current relative performance is considered to be good (*Cg*) or poor (*Cp*), respectively, if the current period's *CFO* is greater or less than its industry median.[2]

3. Test procedures

Sample selection

The initial sample consists of all firms included in 1999 COMPUSTAT PC-Plus Active and Research files during the 1983–98 period. As in other studies, we exclude firms in regulated industries and financial institutions with SIC between 4000 and 4999 and between 5999 and 7000, respectively. We also exclude unclassified firms with a SIC of 9999. We delete firms that have changed fiscal-year ends during the sample period. We further delete firms with total assets less than $1 million or with negative book value of equity. To obtain meaningful cross-sectional estimates of the parameters in (2), we require that at least 20 firms be available for each SIC two-digit industry in each year. We delete firms with insufficient data to estimate the Jones 1991 model parameters. Finally, to mitigate potential problems of outliers, we winsorize observations that fall in the top and bottom 1 percent of the empirical distribution for each research variable. These sample selection procedures yield a sample of 33,163 firm-year observations, with sufficient data for the 15-year sample period, 1984–98, which consist of 4,810 non–Big 6 observations and 28,353 Big 6 observations.[3]

Empirical specification: Models for auditor and accrual choices

Auditor choice model

As mentioned earlier, managers not only select a Big 6 or non–Big 6 auditor but also make accrual choices. Therefore, simply regressing discretionary accruals on auditor type (along with other control variables) may create a self-selection bias. To address the issue of self-selection, we rely on a two-stage "treatment effects" model (Barnow et al. 1980; Maddala 1983; Greene 1997). In the first stage, we estimate a multivariate probit model in which the dependent variable, $Pr(B6)$, is the probability that managers choose a Big 6 auditor:

$$Pr(B6)_{jt} = \delta_0 + \delta_1 AbsSAC_{jt} + \delta_2 AbsLAC_{jt} + \delta_3 SALES_{jt} + \delta_4 LEV_{jt} + \delta_5 P/E_{jt} + \delta_6 ShrIncr_{jt} + \delta_7 LOSS_{jt} + \nu_{jt} \quad (3)$$

where, for firm j in year t:

AbsSAC = magnitude of short-term accruals measured by the absolute value of working capital accruals scaled by sales;

AbsLAC = magnitude of long-term accruals measured by the absolute value of depreciation and amortization scaled by sales;

SALES = firm size measured by natural log of total sales;[4]

LEV = leverage measured by debt-to-total assets ratio;

P/E = price-to-earnings ratio at fiscal year-end;

ShrIncr = dummy variable equal to one if the number of shares outstanding increases by more than 10 percent during the current fiscal year, and zero otherwise;

LOSS = dummy variable having the value of one if net income divided by lagged assets is negative and the absolute values of changes in net income divided by lagged assets are greater than 10 percent, and zero otherwise; and

v = unspecified random factors.

In estimating (3), the dependent variable is set equal to one for Big 6 audited firms, and zero otherwise. Our independent variables — namely, cross-sectional determinants of auditor choice — are similar to those considered by Francis et al. 1999. Francis et al. (1999) find that the absolute values of both short-term and long-term accruals, scaled by sales revenue, are positively associated with Big 6 auditor choice and the associations are, overall, significant at the 1 percent level. We thus include in (3) the absolute values of short-term accruals and long-term accruals scaled by sales (*AbsSAC* and *AbsLAC*, respectively), and predict a positive sign for both the *AbsSAC* and *AbsLAC* coefficients.[5] Based on Francis et al.'s 1999 findings, we also predict that Big 6 auditor choice is positively associated with firm size, growth opportunities (proxied by *P/E*), and new equity issues (proxied by *ShrIncr*), and is negatively associated with financial leverage. While Francis et al. (1999) find that Big 6 auditor choice is not significantly associated with the existence of a loss, we include *LOSS* to make our model comparable with theirs.

Accrual choice model

Once (3) is estimated in the first stage, we estimate in the second stage the following regression linking managers' accrual choices with the auditor choice and other firm-specific variables, including inverse Mills ratios obtained from estimation of the auditor choice model in (4):[6]

$$DAC_{jt} = \delta_0 + \delta_1 B6_{jt} + \delta_2 LEV_{jt} + \delta_3 SIZE_{jt} + \delta_4 CFO_{jt} + \delta_5 ShrDecr_{jt} + \delta_6 ShrIncr_{jt} + \delta_7 NewAud_{jt} + \delta_8 OldAud_{jt} + \delta_9 DAC\_1_{jt-1} + \delta_{10} Lamda_{jt} + \varepsilon_{jt} \quad (4)$$

where *LEV* and *ShrIncr* are as defined earlier, and for firm *j* in year *t*:

DAC = the level of opportunistic accrual choices estimated from (2);

B6 = dummy variable equal to one if a firm has a Big 6 auditor, and zero otherwise;[7]

CFO = operating cash flows deflated by total assets;

SIZE = firm size measured by the natural log of total assets;

ShrDecr = dummy variable equal to one if the total number of shares outstanding decreases by more than 10 percent during current fiscal year, and zero otherwise;

NewAud = dummy variable equal to one if a sample year is the first year in which a new auditor starts auditing a new client firm, and zero otherwise;

OldAud = dummy variable equal to one if a sample year is the last year in which an incumbent auditor finishes auditing an old client firm, and zero otherwise;

DAC_1 = lagged *DAC*;

Lamda = inverse Mills ratio obtained from estimating the probit model in (3); and

ε = unspecified random factors.

Note that we do not include the variable representing the level of opportunistic earnings management (i.e., *DAC*) in the auditor choice model in (3). Including the *DAC* variable in (3) would be inconsistent with the fact that the decision to choose a Big 6 or non–Big 6 auditor precedes the opportunistic accrual choice. We obtain the inverse Mills ratio (i.e., *Lamda*) from the probit model in (3) and include it as an independent variable in (4) to account for potential self-selection bias. A simple OLS estimation of (4) without including the inverse Mills ratio would create self-selection bias, yielding inconsistent parameter estimates of (4) (Barnow et al. 1980; Maddala 1983).

In (4), we include financial leverage (*LEV*) and firm size (*SIZE*) as control variables because previous research suggests that they may affect discretionary accounting choices in the current period (Becker et al. 1998; DeFond and Park 1997). Recall that we include *ΔCFO* in (2) to control for possible effects on our test results of a mechanical correlation between *DAC* and the change in operating cash flows. As a further control, we include the *CFO* variable in (4) because our univariate analysis (as reported in Table 1) shows that the level of operating cash flows (*CFO*) differs significantly between Big 6 and non–Big 6 clients. Previous research reports that managers have incentives to boost reported earnings prior to share-increasing transactions such as seasoned equity offerings, while they have incentives to reduce reported earnings prior to share-decreasing transactions such as share repurchases and management buyouts (Perry and Williams 1994; Teoh, Welch, and Wong 1998; Teoh, Wong, and Rao 1998; Rangan 1998; Becker et al. 1998; Shivakumar 2000). To control for potential earnings-management effects of the incentives related to stock transactions, we include two dummy variables representing share-

decreasing and share-increasing transactions, *ShrDecr* and *ShrIncr*, respectively. Previous research reports that auditor-switching firms are likely to have negative *DAC* during the last year with their predecessors and the first year with their new auditors (DeFond and Subramanyam 1998). Similar to Becker et al. 1998, we include the dummy variables, *NewAud* and *OldAud*, to control for potential effects of auditor changes on earnings management. Given that discretionary accruals are expected to be zero over time, managers' ability to borrow or save earnings in the current period could be affected by the extent to which earnings were borrowed or saved in previous periods (Stein 1989; DeFond and Park 1997; Becker et al. 1998). In an attempt to control for the effect of this possibility on our test results, we also include *DAC_1* as an additional control variable.[8]

4. Empirical results

Descriptive statistics and univariate comparisons

Table 1 presents descriptive statistics for financial variables using the full sample. Sections A and B of the table present the mean, median, and standard deviation of variables for the Big 6 and non–Big 6 samples, respectively, and section C presents the results of parametric *t*-test and nonparametric Wilcoxon *z*-test comparing the two groups. In panel A of the table, we compare all the explanatory variables included in our accrual choice model in (2) between the Big 6 and non–Big 6 samples. In panel B, we compare *DAC* and three measures of economic performance (*NI*, *RET*, and *XRET*) between the two samples.

As shown in the table, Big 6 clients are more leveraged than non–Big 6 clients. The mean and median of financial leverage, measured by the ratio of total debts to total assets, are 0.475 and 0.482, respectively, for Big 6 clients and 0.439 and 0.438, respectively, for non–Big 6 clients. These differences are significant at the 1 percent level. Firm size, measured by the natural log of total assets, is significantly larger for the Big 6 sample compared with the non–Big 6 sample. The mean (median) cash flow from operations is 4.6 percent (7.9 percent) of lagged assets for the Big 6 sample, and −0.6 percent (4.1 percent) for the non–Big 6 sample, suggesting that Big 6 clients are better able to generate operating cash flows than non–Big 6 clients. There is no significant difference in firms' involvement in share-decreasing transactions between the two groups. It is noteworthy that a relatively small portion of Big 6 and non–Big 6 clients (1.9 percent for Big 6 and 1.7 percent for non–Big 6) are, on average, involved in share-decreasing transactions leading to a reduction of shares outstanding by more than 10 percent. Unlike share-decreasing transactions, a relatively large portion of Big 6 and non–Big 6 clients (14.8 percent for Big 6 and 18.8 percent for non–Big 6) are, on average, involved in share-increasing transactions leading to an increase in shares outstanding by more than 10 percent. Non–Big 6 clients are more heavily involved in share-increasing transactions than Big 6 clients. Descriptive statistics on the two dummy variables, *NewAud* and *OldAud*, strongly suggest that non–Big 6 clients are more frequently involved in auditor switching than Big 6 clients. Similar to current *DAC*, lagged *DAC* is significantly lower for the Big 6 sample than for the non–Big 6 sample. Note also that the

inverse Mills ratios (*Lamda*) differs significantly between the Big 6 and non–Big 6 samples.

As shown in panel B of the table, both the mean and median of discretionary accruals (*DAC*) are significantly lower for the Big 6 sample than for the non–Big 6 sample, a finding consistent with Becker et al. 1988 and Francis et al. 1999. The descriptive statistics on three measures of economic performance, measured by *NI*, *RET*, and *XRET*, indicate that Big 6 clients, on average, perform better than non–Big 6 clients in terms of both earnings and capital market performance. Mean (median) net income is 1.8 percent (4.9 percent) of lagged assets for the Big 6

TABLE 1
Descriptive statistics for major research variables

	Section A Observations with Big 6 auditors (*B6*: $n = 28{,}353$)			**Section B** Observations with non–Big 6 auditors (*NB6*: $n = 4{,}810$)			**Section C** For mean and median differentials (*B6* – *NB6*)	
	Mean	Median	σ	Mean	Median	σ	*t*	*Z*
Panel A: Differences in firm characteristics between Big 6 and non–Big 6 samples								
Financial leverage (*LEV*)	0.475	0.482	0.206	0.439	0.438	0.228	10.22	8.29
Firm size (*SIZE*)	5.038	4.876	2.063	2.925	2.608	1.657	78.68	51.99
Cash flows (*CFO*)	0.046	0.079	0.199	−0.006	0.041	0.245	13.81	16.43
ShrDecr	0.019	0.000	0.135	0.017	0.000	0.129	0.86	0.84
ShrIncr	0.148	0.000	0.355	0.188	0.000	0.391	−6.59	−7.04
NewAud	0.059	0.000	0.235	0.110	0.000	0.313	−10.92	−13.32
OldAud	0.061	0.000	0.239	0.131	0.000	0.338	−13.87	−17.55
DAC_1	−0.002	−0.003	0.088	0.006	0.004	0.109	−4.82	−5.40
Lamda	0.222	0.176	0.182	−1.307	−1.231	0.438	238.84	75.00
Panel B: Differences in *DAC* and economic performance between Big 6 and non–Big 6 samples								
Discretionary accruals (*DAC*)	−0.004	−0.004	0.085	0.003	0.003	0.106	−4.48	−5.37
Net income deflated by lagged assets (*NI*)	0.018	0.049	0.393	−0.024	0.031	0.297	8.59	11.35
Annual returns (*RET*)	0.177	0.080	0.614	0.154	0.000	0.761	1.92	9.44
Annual excess returns (*XRET*)	−0.007	−0.091	0.602	−0.032	−0.180	0.751	2.21	10.00

(The table is continued on the next page.)

TABLE 1 (Continued)

Notes:

LEV = ratio of total debt to total assets;

SIZE = firm size measured by natural log of total assets (in millions of dollars);

CFO = cash flows from operations deflated by lagged assets;

ShrDecr = one if the number of shares outstanding decreases by more than 10 percent during a given sample year, and zero otherwise;

ShrIncr = one if the number of shares outstanding increases by more than 10 percent during a given sample year, and zero otherwise;

NewAud = one if it is the first year a new auditor starts auditing the client firm, and zero otherwise;

OldAud = one if it is the last year an old auditor finishes auditing the client firm, and is followed by an auditor change, and zero otherwise;

DAC_1 = the level of *DAC* in year $t - 1$;

Lamda = inverse Mills ratios obtained from estimation of (3);

DAC = discretionary accruals measured as the difference between actual total accruals and the fitted values of (2);

NI = income before extraordinary items deflated by lagged assets;

RET = annual stock return for fiscal year; and

XRET = market-adjusted return measured by difference between annual stock return for the fiscal year minus annual equally weighted market return for the same period.

sample, and −2.4 percent (3.1 percent) for the non−Big 6 sample. Mean (median) annual return is 17.7 percent (8.0 percent) for the Big 6 sample and 15.4 percent (0.0 percent) for the non−Big 6 sample. These mean and median differences are significant at the 5 percent level. Consistent with *NI* and *RET*, we also observe that both mean and median of annual excess returns are significantly greater for the Big 6 sample than for the non−Big 6 sample.

In summary, the results reported in Table 1 show that Big 6 clients are significantly different from non−Big 6 clients in their sample characteristics, thereby confirming the importance of an explicit control for differences in firm-specific characteristics between the two samples. In the next two sections, we first extend our univariate comparison of *DAC* and three measures of economic performance (*NI*, *RET*, and *XRET*) between the two samples by controlling for managers' incentives for opportunistic earnings management. We then conduct multivariate tests that control not only for the direction of earnings-management incentives but also for firm characteristics that differ between Big 6 and non−Big 6 clients.

Table 2 gives a correlation matrix for the major research variables included in (4) using all firms in the full sample. The upper half of Table 2 reports Pearson correlation coefficients (r_p), while the lower half of the same table reports Spearman correlation coefficients (r_s). *DAC* is negatively correlated with the Big 6 dummy at

the less than 1 percent level ($r_p = -0.029$ and $r_s = -0.033$), suggesting that Big 6 clients are involved in income-increasing accrual choices to a lesser degree than non–Big 6 clients. *DAC* is significantly negatively correlated with financial leverage (*LEV*), suggesting that as a firm becomes more highly leveraged, its ability to boost reported earnings through income-increasing *DAC* becomes weaker. The Pearson (Spearman) correlation between *DAC* and *SIZE* is positively significant (insignificant with a negative sign). *DAC* is significantly negatively correlated with operating cash flow performance (*CFO*), a finding consistent with previous research (e.g., Dechow 1994; Dechow, Kothari, and Watts 1998). A significant positive correlation between *DAC* and *DAC_1* suggests that discretionary accruals are mean-reverting over time. *B6* is significantly positively correlated with *SIZE* (with

TABLE 2
Correlation matrix ($n = 33{,}163$)*

	DAC	*B6*	*LEV*	*SIZE*	*CFO*	*DAC_1*	*Lamda*
DAC		−0.029	−0.076	0.011	−0.201	0.136	−0.022
		(<0.001)	(<0.001)	(0.038)	(<0.001)	(<0.001)	(<0.001)
B6	−0.033		0.058	0.346	0.087	−0.031	0.910
	(<0.001)		(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
LEV	−0.066	0.056		0.277	−0.033	−0.055	0.000
	(<0.001)	(<0.001)		(<0.001)	(<0.001)	(0.001)	(1.000)
SIZE	−0.006	0.358	0.273		0.280	0.016	0.024
	(0.278)	(<0.001)	(<0.001)		(<0.001)	(0.003)	(<0.001)
CFO	−0.311	0.102	−0.108	0.281		0.050	−0.020
	(<0.001)	(<0.001)	(<0.001)	(<0.001)		(<0.001)	(<0.001)
DAC_1	0.166	−0.035	−0.054	0.013	0.011		−0.035
	(<0.001)	(<0.001)	(<0.001)	(0.015)	(0.040)		(<0.001)
Lamda	0.011	0.611	−0.136	−0.434	−0.131	−0.009	
	(0.048)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(0.097)	

Notes:

* The table reports Pearson product-moment correlation coefficients (Spearman rank correlation coefficients).

DAC = discretionary accruals = differences between actual total accruals and the fitted values of (2);

B6 = dummy variable equal to one for Big 6 clients, and zero otherwise;

LEV = ratio of total debt to total assets;

SIZE = firm size measured by natural log of total assets;

CFO = cash flows deflated by lagged assets;

DAC_1 = the level of *DAC* in year $t - 1$; and

Lamda = inverse Mills ratios obtained from estimation of (3).

$r_p = 0.346$ and $r_s = 0.358$), indicating that large firms are more likely to hire Big 6 auditors than small firms.

Univariate tests after controlling for earnings-management incentives

In this section, our analysis focuses on the question whether, and how, the degree of a firm's involvement in opportunistic earnings management is differentially affected by auditor type (Big 6 versus non–Big 6) and the direction of earnings-management incentives. To see whether Big 6 auditors are more effective than non–Big 6 auditors in deterring opportunistic earnings management, we conduct univariate tests for differences in various characteristics between Big 6 and non–Big 6 clients after controlling for the direction of earnings-management incentives. In panel A of Table 3, we report the mean, median, and standard deviation of discretionary accruals (*DAC*), net income deflated by lagged assets (*NI*), annual returns (*RET*), and annual excess returns (*XRET*)[9] for firms with income-increasing incentives (i.e., firms with *Cp*). Panel B of the table reports the same information for firms with income-decreasing incentives (i.e., firms with *Cg*).[10]

A cross-panel comparison of the results presented in panels A and B reveals that, regardless of auditor type (Big 6 versus non–Big 6), the mean and median levels of *DAC* are greater when managers have income-increasing incentives (panel A) than when they have income-decreasing incentives (panel B). This is consistent with the notion that managers of firms with relatively poor current performance (*Cp*) are likely to boost reported earnings through income-increasing accrual choices, while those with relatively good current performance (*Cg*) are likely to reduce reported earnings through income-decreasing accrual choices. These results suggest that it is necessary for researchers to control for the direction of earnings-management incentives when evaluating audit effectiveness differentiation between Big 6 and non–Big 6 auditors.

A cross-panel comparison also reveals that, regardless of auditor type, three measures of economic performance (i.e., *NI*, *RET*, and *XRET*) are substantially lower in panel A where managers have income-increasing incentives (i.e., current relative performance is poor) than in panel B where managers have income-decreasing incentives (i.e., current relative performance is good). This suggests that our measure of current relative performance (i.e., *Cp* or *Cg*) effectively captures the direction of earnings-management incentives associated with a firm's economic performance as modeled in Fudenberg and Tirole 1995.

We now compare the results between Big 6 and non–Big 6 samples for each panel. As shown in panel A of Table 3, for firms with income-increasing incentives (i.e., *Cp*), both means and medians of *DAC* are significantly lower for the Big 6 sample than for the non–Big 6 sample at the 1 percent level ($t = -2.35$ and $z = -3.69$). The mean and median differences in *DAC* between the two samples are 0.5 percent and 0.7 percent, respectively, of lagged assets. The mean and median of *NI* are −7.7 percent and 1.0 percent, respectively, for the Big 6 sample while they are −11.2 percent and −1.1 percent, respectively, for the non–Big 6 sample.[11] In other words, *NI* is higher for Big 6 clients than non–Big 6 clients, although *DAC* is lower for Big 6 clients than for non–Big 6 clients. Note here that similar to *NI*,

TABLE 3
Tests for differences in means and medians of various characteristics by auditor type and by earnings-management incentives

	Section A: Observations with Big 6 auditors (*B6*)			Section B: Observations with non–Big 6 auditors (*NB6*)			Section C: For mean and median differentials (*B6* – *NB6*)	
	Mean	Median	σ	Mean	Median	σ	*t*	*Z*
Panel A: Firms with income-increasing incentives (i.e., *Cp*)								
Discretionary accruals (*DAC*)	0.021	0.016	0.100	0.026	0.023	0.118	−2.35	−3.69
Net income deflated by lagged assets (*NI*)	−0.077	0.010	0.404	−0.112	−0.011	0.358	4.69	6.81
Annual returns (*RET*)	0.100	−0.014	0.757	0.065	−0.095	0.766	1.97	5.15
Annual excess returns (*XRET*)	−0.118	−0.036	0.399	−0.155	−0.054	0.355	4.84	6.89
# of firms (*n*)	12,839			2,748				
Panel B: Firms with income-decreasing incentives (i.e., *Cg*)								
Discretionary accruals (*DAC*)	−0.027	−0.017	0.068	−0.023	−0.015	0.082	−1.77	−0.90
Net income deflated by lagged assets (*NI*)	0.091	0.077	0.438	0.088	0.074	0.131	0.71	1.78
Annual returns (*RET*)	0.242	0.142	0.579	0.254	0.089	0.742	−0.76	3.90
Annual excess returns (*XRET*)	0.051	0.034	0.438	0.049	0.031	0.131	−0.50	1.23
# of firms (*n*)	15,514			2,062				

Notes:

DAC = discretionary accruals measured as the difference between actual total accruals and the fitted values of (2);

NI = income before extraordinary items deflated by lagged assets;

RET = annual stock return for fiscal year; and

XRET = market-adjusted return measured by difference between annual stock return for the fiscal year minus annual equally weighted market return for the same period.

both annual returns (*RET*) and annual excess returns (*XRET*) are significantly greater for the Big 6 sample than for the non–Big 6 sample. To the extent that high (low) *NI*, *RET*, and *XRET* capture good (bad) performance, the significant differences in the three measures of economic performance between the Big 6 and non–Big 6 samples suggest that Big 6 clients outperform non–Big 6 clients, and thus, that the relatively high *NI* for Big 6 clients is likely to be a result of relatively good performance of Big 6 clients, rather than a result of opportunistic earnings management.

Lower *DAC* for Big 6 clients observed for firms with income-increasing incentives (*Cp*) may be interpreted in such a way that when managers of audit clients have income-increasing incentives (i.e., when a conflict of reporting incentives exists between auditors and their clients), Big 6 auditors are more effective than non–Big 6 auditors in deterring their clients from income-increasing accrual choices. Put differently, Big 6 clients are more constrained in boosting reported earnings through income-increasing accrual choices, compared with non–Big 6 clients.

If Big 6 auditors are more effective than non–Big 6 auditors in deterring their clients from choosing income-decreasing accruals, the (signed) level of *DAC* should be greater for Big 6 clients than for non–Big 6 clients, other things being equal. The results reported in panel B of Table 3 show, however, that for firms with income-decreasing incentives (i.e., *Cg*), the mean (signed) level of *DAC* is lower for the Big 6 sample than for the non–Big 6 sample, though the difference is only marginally significant with a *t*-statistic of −1.77. The median level of *DAC* is also slightly lower for Big 6 clients than for non–Big 6 clients, though the median difference in *DAC* between the two samples is insignificant with $Z = -0.90$. This suggests that when managers have income-decreasing incentives (i.e., *Cg*), Big 6 auditors are less effective, or not more effective, than non–Big 6 auditors in controlling managers' income-decreasing incentives. The differences in firm performance measures (i.e., *NI*, *RET*, and *XRET*) between Big 6 and non–Big 6 clients are not significant at the 5 percent level with an exception that the median difference in *RET* is significant with a *Z*-statistic of 3.90. In sum, the results reported in panel B support the argument that when managers of audit clients have income-decreasing incentives (i.e., when a convergence of reporting incentives exists between auditors and their clients), Big 6 auditors are not more effective than non–Big 6 auditors in deterring their clients from income-decreasing accrual choices.

Earnings-management incentives and multivariate tests

As explained in section 3, we apply the two-stage estimation procedure for our multivariate tests. In the first stage, we estimate parameters of the auditor choice model in (3) in two different ways. First, we estimate (3) using the pooled sample of 33,163 firm-year observations over the 15-year period, 1984–98. Second, we also estimate it using procedures similar to Fama and MacBeth's 1973 regression. In other words, we estimate (3) in each year using yearly cross-sectional observations, and then compute the mean of the 15 yearly coefficient estimates for each variable and the associated *t*-values.

Section A of Table 4 reports coefficient estimates of the probit auditor-choice model, along with χ^2 statistics and *p*-values, for the pooled regression, while section

B presents mean annual coefficient estimates and *t*-values for the Fama-MacBeth (hereafter FM) type regression. As shown in Table 4, the results of the pooled regression are qualitatively similar with those of the FM-type regression with a few exceptions. The likelihood ratio statistics in both panels indicate that our auditor-

TABLE 4
Results of probit model estimation for auditor choice

$$Pr(B6)_{jt} = \delta_0 + \delta_1 AbsSAC_{jt} + \delta_2 AbsLAC_{jt} + \delta_3 SALES_{jt} + \delta_4 LEV_{jt} + \delta_5 P/E_{jt} + \delta_6 ShrIncr_{jt} + \delta_7 LOSS_{jt} + v_{jt} \quad (3)$$

	Section A Pooled estimate			**Section B** Mean of annual estimates (1984–98)	
	Coefficient	χ^2	*p*-value	Coefficient	*t*-value
Constant	−0.311	76.39	<0.01	−0.310	−6.64
AbsSAC	0.089	2.11	0.15	0.149	2.18
AbsLAC	1.691	246.89	<0.01	1.577	8.44
SALES	0.340	2,565.42	<0.01	0.331	21.32
LEV	−0.432	72.29	<0.01	−0.352	−5.19
P/E	0.000	3.25	0.07	0.000	2.84
ShrIncr	−0.010	0.10	0.75	0.078	3.03
LOSS	0.215	45.28	<0.01	0.291	7.39
	LR statistics		22,802	Average LR	
	d.f.		33,155	statistics*	1,403
	p-value for LR statistics		<0.01		

Notes:

* It represents an average of annual LR statistics computed from estimation of (3) using yearly cross-sectional observations during the 15-year period, 1984–98.

B6 = one for Big 6 audited firms, and zero otherwise;

AbsSAC = absolute value of short-term accruals deflated by sales dollars;

AbsLAC = absolute value of long-term accruals deflated by sales dollars;

SALES = firm size measured by natural log of total sales (in millions of dollars);

LEV = ratio of total debt to total assets;

P/E = price-to-earnings ratio;

ShrIncr = one if the number of shares outstanding increases by more than 10 percent during the current fiscal year, and zero otherwise; and

LOSS = one if net income divided by lagged assets is negative and the absolute values of changes in net income divided by lagged assets are greater than 0.1, and zero otherwise.

choice model has significant explanatory power. Both *AbsSAC* and *AbsLAC* are positively associated with the Big 6 auditor choice. The pooled results (section A) show that *AbsSAC* is insignificant ($p = 0.15$) while *AbsLAC* is highly significant ($p < 0.01$). In contrast, the FM results (section B) show that both *AbsSAC* and *AbsLAC* are significant at the less than 5 percent level ($t = 2.18$ and 8.44, respectively). Consistent with Francis et al. 1999, Big 6 auditor choice is positively associated with firm size proxied by total sales (*SALES*) ($p < 0.01$) and growth opportunities (P/E) ($p < 0.07$), while it is negatively associated with financial leverage (*LEV*) ($p < 0.01$). Francis et al. (1999) predict a negative sign for *LOSS* (a proxy for financial distress), and find that the coefficient on *LOSS* is insignificantly positive. In contrast, we find that *LOSS* is positively associated with Big 6 auditor choice, and is significant at $p < 0.01$, regardless of the estimation method used. Francis et al. 1999 find that Big 6 auditor choice is positively associated with new equity offerings. Our results show that *ShrIncr* is insignificant for the pooled regression ($p = 0.75$), while it is positively significant for the FM-type regression ($t = 3.03$).[12]

In the second stage, we estimate our accrual choice model in (4) using the inverse Mills ratios obtained from estimation of the probit auditor-choice model in (3) to account for potential problems of self-selection bias associated with managers' auditor choices. Our objective here is to test whether the effect of audit quality (Big 6 versus non–Big 6) on audit effectiveness differs between two distinct situations with different incentives (income-increasing versus income-decreasing). To do so, we construct two distinct subsamples: (1) the sample of firms with poor current relative performance (hereafter "the *Cp* sample"); and (2) the sample of firms with good current relative performance (hereafter "the *Cg* sample"). Recall that managers of firms in the *Cp* sample are assumed to have incentives to overstate reported earnings through income-increasing accrual choices, while those in the *Cg* sample are assumed to have incentives to understate reported earnings through income-decreasing accrual choices. Therefore a conflict (convergence) of reporting incentives between managers and external auditors arises in the *Cp* (*Cg*) sample.

Table 5 presents the results from a second-stage estimation of our accrual choice model (i.e., (4)). We run the FM regression for (4) separately for the full sample ($n = 33{,}163$), the *Cp* sample ($n = 15{,}587$), and the *Cg* sample ($n = 17{,}576$) in an attempt to alleviate potential problems of residual correlation across sample firms (Bernard 1987). Regression coefficients reported in Table 5 are obtained as follows. We first estimate the probit regression in (3) annually and obtain the inverse Mills ratios in each year. Using annual estimates of the inverse Mills ratios, we then estimate (4) cross-sectionally for each of the 15 years, 1984–98, and compute *t*-statistics using the variability of annual coefficient estimates over the 15-year estimation period. Each reported coefficient in Table 3 represents an average of 15 yearly coefficient estimates for each independent variable.

Section A of Table 5 presents the results for the full sample. Sections B and C report the results for the *Cp* and *Cg* samples, respectively. The full-sample result shows that, as in Becker et al. 1998, the coefficient on the Big 6 dummy (*B6*) is significantly negative (−0.066 with $t = -5.91$). Big 6 clients report discretionary

accruals that are lower than non–Big 6 clients by an average of 6.6 percent of lagged assets, suggesting that Big 6 auditors are more effective than non–Big 6 auditors in constraining managers' ability to make income-increasing *DAC*

TABLE 5
Results of Fama-MacBeth regressions using discretionary accruals as the dependent variable

$$DAC_{jt} = \delta_0 + \delta_1 B6_{jt} + \delta_2 LEV_{jt} + \delta_3 SIZE_{jt} + \delta_4 CFO_{jt} + \delta_5 ShrDecr_{jt} + \delta_6 ShrIncr_{jt} + \delta_7 NewAud_{jt} + \delta_8 OldAud_{jt} + \delta_9 DAC\_1_{jt-1} + \delta_{10} Lamda_{jt} + \varepsilon_{jt} \quad (4)$$

	Section A Full sample	**Section B** The *Cp* sample with income-increasing incentives	**Section C** The *Cg* sample with income-decreasing incentives
Constant	0.048*	0.069	0.062
	(6.41)	(5.22)	(7.80)
Big 6 auditor (dummy: *B6*)	−0.066	−0.076	−0.046
	(−5.91)	(−3.69)	(−4.31)
Financial leverage measured by the ratio of total debts to total assets (*LEV*)	−0.052	−0.049	−0.093
	(−10.84)	(−7.31)	(−23.31)
Firm size measured by the natural log of total assets (*SIZE*)	0.008	0.009	0.008
	(12.45)	(6.93)	(16.33)
Cash flows from operations deflated by lagged total assets (*CFO*)	−0.141	−0.056	−0.291
	(−8.27)	(−3.26)	(−31.05)
Greater than 10% decrease in shares outstanding (dummy: *ShrDecr*)	−0.006	0.006	−0.008
	(−2.03)	(1.12)	(−1.73)
Greater than 10% increase in shares outstanding (dummy: *ShrIncr*)	−0.002	0.004	0.003
	(−1.16)	(1.75)	(1.06)
First year with a new auditor (dummy: *NewAud*)	−0.003	−0.003	−0.001
	(−0.82)	(−0.45)	(−0.41)
Last year with an old auditor (dummy: *OldAud*)	−0.012	−0.015	−0.006
	(−4.63)	(−4.49)	(−1.53)
One-period lagged discretionary accruals (*DAC_1*)	0.136	0.135	0.107
	(16.01)	(12.20)	(9.50)

(The table is continued on the next page.)

TABLE 5 (Continued)

	Section A Full sample	Section B The *Cp* sample with income-increasing incentives	Section C The *Cg* sample with income-decreasing incentives
Inverse Mills ratio obtained from estimation of the auditor choice model in (4) (*Lamda*)	0.032	0.037	0.025
	(5.74)	(3.40)	(4.54)
Number of observations over the entire sample period	33,163	15,587	17,576
Average of adjusted R^2 for 15 yearly regressions	0.123	0.071	0.200

Notes:

* Annual regressions are estimated for each year from 1984 to 1998 (15 years). On the basis of the 15-year estimates, the average regression coefficient (*t*-value) is presented.

choices. This magnitude of Big 6 and non–Big 6 differences in *DAC* is economically significant, given that the mean (median) net income is 1.8 percent (4.9 percent) of lagged assets for Big 6 clients and −2.4 percent (3.1 percent) for non–Big 6 clients as reported in Table 1. The coefficient on the inverse Mills ratio (*Lamda*) is significant with a *t*-value of 5.74. The significance of *Lamda* indicates that it is important to explicitly control for self-selection bias.

In addition, the full-sample result (section A) shows that several of the control variables are significantly associated with *DAC*. Consistent with Becker et al. 1998, *DAC* is negatively associated with financial leverage (*LEV*) and operating cash flows (*CFO*) at the 1 percent level. In contrast to Becker et al. 1998, who find firm size to be insignificant (with a positive sign), our results show that the coefficient on *SIZE* is significantly positive at the 1 percent level. The coefficient on *DAC_1* is significantly positive at the 1 percent level, which is consistent with DeFond and Park 1997. While both auditor-switch variables (*NewAud* and *OldAud*) are negative, only the coefficient on *OldAud* is significant. This suggests that auditor-switching firms are more likely to have negative *DAC* during the last year with old auditors than during other years, a finding consistent with DeFond and Subramanyam 1998. The coefficient on *ShrIncr* is insignificantly negative. In contrast, the coefficient on *ShrDecr* is significantly negative, suggesting that firms with share-decreasing transactions are engaged in income-decreasing earnings management. Note that Becker et al. (1998) report that both *ShrDecr* and *ShrIncr* are insignificant. The full-sample results reported in section A of Table 5 should be interpreted cautiously, however, because it fails to incorporate into the analysis the possibility that the effect of audit quality on earnings management interacts with the direction of earnings-management incentives.

To test whether, and how, the conflict or convergence of reporting incentives between managers and external auditors interacts with audit effectiveness differentiation, we estimate our accrual choice model in (4) separately for the *Cp* sample and for the *Cg* sample. The results for the *Cp* and *Cg* samples are reported in sections B and C of Table 5, respectively.

If Big 6 auditors are more effective than non−Big 6 auditors in deterring opportunistic earnings management, regardless of whether managers have income-increasing or income-decreasing incentives, the coefficient on *B6* in (4) should be negative for the *Cp* sample (of firms with income-increasing incentives), and it should be positive for the *Cg* sample (of firms with income-decreasing incentives). However, if Big 6 auditors are more effective than non−Big 6 auditors *only* when managers prefer income-increasing accrual choices, we will observe that the *B6* coefficient in (4) is either insignificantly different from zero or significantly negative for the *Cg* sample. An insignificant coefficient on *B6* for the *Cg* sample is consistent with no audit effectiveness differentiation between Big 6 and non−Big 6 auditors when managers prefer income-decreasing accrual choices. A significantly negative coefficient on *B6* for the *Cg* sample is consistent with Big 6 auditors being less effective than non−Big 6 auditors when managers prefer income-decreasing accrual choices.

As shown in section B of Table 5, the coefficient on *B6* is significantly negative for the *Cp* sample (−0.076 with a *t*-value of −3.69). This indicates that Big 6 auditors are more effective than non−Big 6 auditors in constraining managers' abilities to overstate reported earnings through income-increasing accrual choices when managers have income-increasing incentives (i.e., *Cp*). If Big 6 auditors are more effective than non−Big 6 auditors in limiting flexibility allowed for income-decreasing accrual choices, one should observe the positive coefficient on *B6* for the *Cg* sample (of firms with income-decreasing incentives). We observe, however, that the coefficient on *B6* is significantly negative for the *Cg* sample (−0.046 with *t*-value of −4.31), as shown in section C of Table 5. This indicates that, contrary to conventional wisdom, when managers have income-decreasing incentives (i.e., *Cg*), and thus there is no conflict of reporting incentives between the two issuers of financial statements, namely auditors and their clients, Big 6 auditors are less effective than non−Big 6 auditors in deterring their clients from understating reported earnings through income-decreasing accrual choices. Put differently, the result reported in section C of Table 5 is consistent with the argument that Big 6 auditors allow their clients to have more flexibility for income-decreasing accrual choices than non−Big 6 auditors, when their clients have incentives to understate reported earnings (and thus there is no reporting incentive conflict between the two issuers of financial statements).

A comparison of the results in sections A, B, and C of Table 5 reveals that, across all sections, the level of discretionary accruals is inversely (positively) associated to *LEV* and *CFO* (*SIZE* and *DAC_1*), and the associations are highly significant. These associations are robust to the presence or absence of reporting incentive conflicts between managers and auditors. As shown in sections B and C, *ShrDecr* is significant ($t = -1.73$) with an expected negative sign for the *Cg* sample (with

income-decreasing incentives), but not for the *Cp* sample (with income-increasing incentives). The negative coefficient on *ShrDecr* suggests that firms with share-decreasing transactions (e.g., share repurchases) tend to reduce reported earnings through income-decreasing accrual choices. In contrast, *ShrIncr* is marginally significant ($t = 1.75$) with an expected positive sign for the *Cp* sample, but not for the *Cg* sample.[13] The positive coefficient on *ShrIncr* suggests that firms with share-increasing transactions (e.g., new issues offerings) tend to boost reported earnings through income-increasing accrual choices, a finding consistent with previous research (Rangan 1998; Shivakumar 2000). The auditor-switching dummies (*NewAud* and *OldAud*) are not as robust as the other control variables (e.g., *LEV*, *CFO*, *SIZE*, and *DAC_1*). The coefficient on *NewAud* is insignificant for both the *Cp* and *Cg* samples. The coefficient on *OldAud* is significant ($t = -4.49$) with an expected negative sign for the *Cp* sample, but not for the *Cg* sample. The coefficient on *Lamda* is highly significant for both *Cp* and *Cg* samples, indicating that the failure to explicitly control for potential self-selection biases may lead to erroneous conclusions.

In conclusion, the results reported in Table 5 indicate that audit effectiveness differentiation between Big 6 and non–Big 6 auditors depends critically on the existence of reporting incentive conflicts (or the lack thereof) between the two issuers of financial statements — auditors and their clients. Big 6 auditors have incentives to be more (less) conservative than non–Big 6 auditors in determining reported earnings when their clients have incentives to overstate (understate) reported earnings through income-increasing (income-decreasing) accrual choices. This occurs because litigation risk associated with audit failure to detect income overstatement is significantly higher than that associated with audit failure to detect income understatements, and potential litigation costs associated therewith, including a loss of reputation capital, are likely to be much greater for Big 6 auditors than for non–Big 6 auditors. In short, our results reported in Table 5 suggest that the asymmetry in litigation risk between the two distinct situations with income-increasing and income-decreasing incentives leads to the asymmetry in audit effectiveness for deterring opportunistic earnings management. Absent a conflict of reporting incentives between auditors and their clients and litigation risk associated with audit failure (as is the case for income understatement), one cannot rule out the possibility that auditors may collude with their clients, which in turn reduces audit effectiveness.

5. Sensitivity analysis

Alternative proxies for earnings management

While a measure of discretionary accruals using the Jones 1991 model or its variants has been widely used in previous research, it has been criticized because the Jones 1991 model parameter estimates are biased and measurement errors associated therewith could potentially induce erroneous conclusions about the existence of earnings management (Bernard and Skinner 1996; Guay, Kothari, and Watts 1996; Healy 1996). We therefore consider an alternative proxy for opportunistic

earnings management that is independent from potential measurement errors associated with the Jones 1991 model parameters. In particular, we estimate abnormal working capital accruals (*AWCA*), which is the difference between the current year's realized working capital accruals and the expected level of working capital accruals. Similar in spirit to DeFond and Park 2001, we compute *AWCA* for firm *j* in year *t* as follows:

$$AWCA_{jt} = WCA_{jt} - [(WCA_{jt-1}/REV_{jt-1})^{*}REV_{jt}] \quad (5)$$

where *WCA* denotes non-cash working capital accruals that are defined as the first two terms of (1); and *REV* denotes sales revenue.[14] *AWCA* in (5) captures the deviation of the current year's working capital accruals from the normal level of working capital accruals required to support current sales activities. We interpret *AWCA* as an outcome of opportunistic earnings management.[15]

To enhance the validity of our findings, we re-estimate our accrual choice model in (4), using *AWCA* as the dependent variable. In so doing, the dependent variable, *DAC*, and an independent variable, *DAC_1*, of (4) are replaced by *AWCA* and *AWCA_1* (i.e., lagged *AWCA*), respectively. To save space, Table 6 reports a summary of regression results, focusing only regression coefficients for the Big 6 dummy (*B6*) and the inverse Mills ratio (*Lamda*), δ_1 and δ_{10}, respectively, in (4). Though not reported, the coefficients for other variables (δ_2 to δ_9) remain qualitatively similar to those reported in Table 5.

Panels A, B, and C of Table 6 present the results of FM regressions for the full sample, the *Cp* sample, and the *Cg* sample, respectively. Overall, we find that our results reported in Table 6 remain qualitatively similar to those in Table 5, indicating that our results are robust to the use of alternative proxies for earnings management. The coefficient on *B6* is significantly negative across all panels. This confirms our interpretation of Table 5 results: Big 6 auditors are more effective than non–Big 6 auditors in limiting managers' abilities to choose income-increasing accruals when managers have income-increasing incentives (panel B). The reverse is true, however, when managers have income-decreasing incentives (panel C). The coefficient on the inverse Mills ratio (*Lamda*) is highly significant across all panels, confirming the importance of an explicit control for self-selection bias.

Alternative proxies for managerial incentives for earnings management

In Tables 5 and 6, we measure current relative performance (i.e., *Cp* or *Cg*) using the industry performance in the same year as a benchmark performance. As a robustness check, we also use the last year's performance as the benchmark performance. In other words, we measure a firm's current relative performance as the difference between the current period's *CFO* and the last period's *CFO* for each firm. Here, we consider current relative performance to be good (ΔCg) or poor (ΔCp), respectively, if the current period *CFO* is greater or less than the last year's *CFO* (i.e., $\Delta CFO > 0$ or $\Delta CFO < 0$, respectively). We assume that managers have incentives to overstate (understate) reported earnings when the current year's *CFO* performance deteriorates (improves) relative to the last year's *CFO* performance

(i.e., when $\Delta CFO < 0$ [$\Delta CFO > 0$]). We then partition the full sample into two subsamples: (1) the ΔCp sample with income-increasing incentives ($\Delta CFO < 0$); and (2) the ΔCg sample with income-decreasing incentives ($\Delta CFO > 0$).

Panels A and B of Table 7 present a summary of the regression results for the ΔCp sample ($\Delta CFO < 0$) and the ΔCg samples ($\Delta CFO > 0$), respectively. Overall, the results reported in Table 7 are qualitatively similar to those reported in Tables 5 and 6. In other words, the coefficient on *B6* is significantly negative across all cases. This supports the argument that Big 6 auditors are more (less) effective than non−Big 6 auditors when managers have income-increasing (income-decreasing) incentives and thus a conflict (convergence) of reporting incentives exists between managers and external auditors. The above conclusion is robust to alternative measures of earnings management (*DAC* versus *AWCA*) and alternative ways of measuring the benchmark performance (the industry performance versus the past performance). The coefficient on *Lamda* is again highly significant across all cases, confirming the importance of an explicit control for self-selection bias.

6. Potential selection bias arising from mechanical correlation

In Tables 5, 6, and 7, we partition the full sample on the basis of whether current relative performance is poor or good. When measuring current relative performance, we use two different benchmarks: (1) the industry median *CFO*; and (2)

TABLE 6
Results of Fama-MacBeth regressions using abnormal working capital accruals as the dependent variable

$$AWCA_{jt} = \delta_0 + \delta_1 B6_{jt} + \delta_2 LEV_{jt} + \delta_3 SIZE_{jt} + \delta_4 CFO_{jt} + \delta_5 ShrDecr_{jt} + \delta_6 ShrIncr_{jt} + \delta_7 NewAud_{jt} + \delta_8 OldAud_{jt} + \delta_9 AWCA\_1_{jt-1} + \delta_{10} Lamda_{jt} + \varepsilon_{jt}$$

	δ_1	δ_{10}	Average of adjusted R^2 for 15 yearly regressions
Panel A: The full sample (n = 33,163)			
Coefficient	−0.083*	0.049	0.156
(t-value)	(−3.02)	(3.46)	
Panel B: The *Cp* sample of firms with income-increasing incentives (n = 15,587)			
Coefficient	−0.106	0.062	0.125
(t-value)	(−2.66)	(2.91)	
Panel C: The *Cg* sample of firms with income-decreasing incentives (n = 17,576)			
Coefficient	−0.053	0.035	0.188
(t-value)	(−1.73)	(2.20)	

(The table is continued on the next page.)

TABLE 6 (Continued)

Notes:

* Annual regressions are estimated for each year from 1984 to 1998 (15 years). On the basis of the 15-year estimates, the average regression coefficient (*t*-value) is presented.

AWCA = abnormal working capital accruals measured by differences between actual working capital accruals and expected working capital accruals estimated from (5);

B6 = dummy variable equal to one for Big 6 clients, and zero otherwise;

LEV = ratio of total debt to total assets;

SIZE = firm size measured by natural log of total assets (in millions of dollars);

CFO = cash flows deflated by lagged assets;

ShrDecr = one if the number of shares outstanding decreases by more than 10 percent during a given sample year, and zero otherwise;

ShrIncr = one if the number of shares outstanding increases by more than 10 percent during a given sample year, and zero otherwise;

NewAud = one if it is the first year a new auditor starts auditing the client firm, and zero otherwise;

OldAud = one if it is the last year an old auditor finishes auditing the client firm, and is followed by an auditor change, and zero otherwise;

AWCA_1 = the level of *AWCA* in year $t - 1$; and

Lamda = inverse Mills ratios obtained from estimation of (3).

the last year's *CFO*. Selection bias may arise when our proxies for earnings management (i.e., *DAC* in (4)) and our proxies for current relative performance (i.e., the partitioning variables) are mechanically correlated through the same measurement error such as errors associated with the Jones 1991 model estimation. As noted by Elgers, Pfeiffer, and Porter 2000, this selection bias could be a major concern, for example, if both *DAC* and current relative performance are measured through the use of the Jones 1991 model, and thus share the same measurement error, as is the case for DeFond and Park 1997.[16]

In an effort to avoid the confounding effect of potential mechanical correlation on our findings, we do the following. First, our dependent variable in (4) is proxied by two alternative proxies for earnings management — *DAC* and *AWCA*. While our measure of *DAC* is subject to the Jones 1991 model error, our measure of *AWCA* is free from that error. Thus a comparison of the results using the two different proxies helps us to check the robustness of our results to potential selection bias problems. As reported in Tables 5 and 6, our findings hold regardless of which proxy for earnings management was used.

Second, we measure current relative performance in two ways by subtracting either the industry median *CFO* or the last period's *CFO* from the current period's

CFO. Note here that *CFO* is measured independently of *DAC*, and thus it is not correlated with potential measurement errors associated with the Jones 1991 model estimation of *DAC*. It is possible, however, that the *CFO*-based sample partitioning may introduce a different type of mechanical relation between *DAC* and *CFO* because *CFO* is negatively correlated with total accruals, which in turn are positively correlated with *DAC*.[17]

To address this concern, we include an additional term (i.e., ΔCFO) in our extended version of the modified Jones 1991 model as in (2). This yields our measure of *DAC* (i.e., residuals from (2)) being orthogonal to ΔCFO.[18] Thus, the use of *DAC* as the dependent variable and ΔCFO as the partitioning variable alleviates a concern that the results reported in Table 5 may be driven by a mechanical correlation between the dependent variable and the partition variable. A comparison between Table 7 and Tables 5 and 6 shows that our regression results are robust to the use of different partitioning variables, and suggests that they are unlikely to be an artifact of mechanical correlation between our measures of earnings management and the sample partitioning variables.

Finally, an alternative way to circumvent potential selection bias arising from mechanical correlation is to partition the sample using a different measure of the direction of managerial incentives for earnings management that is not based on

TABLE 7

Summary results of various regressions when managerial incentives for earnings management are measured using the last year's performance as the benchmark performance

$$EM_{jt} = \delta_0 + \delta_1 B6_{jt} + \delta_2 LEV_{jt} + \delta_3 SIZE_{jt} + \delta_4 CFO_{jt} + \delta_5 ShrDecr_{jt} + \delta_6 ShrIncr_{jt} + \delta_7 NewAud_{jt} + \delta_8 OldAud_{jt} + \delta_9 EM\_1_{jt-1} + \delta_{10} Lamda_{jt} + \varepsilon_{jt}$$

Dependent variable (*EM*)	Coefficient for the Big 6 dummy (δ_1)	Coefficient for *Lamda* (δ_{10})	Average of adjusted R^2 for 15 yearly regressions
Panel A: The ΔCp sample with income-increasing incentives ($\Delta CFO < 0$) (n = 15,187)			
Discretionary accruals (*DAC*)	−0.073*	0.037	0.125
	(−3.13)	(2.97)	
Abnormal working capital accruals (*AWCA*)	−0.063	0.040	0.115
	(−1.71)	(2.00)	
Panel B: The ΔCg sample with income-decreasing incentives ($\Delta CFO < 0$) (n = 17,976)			
Discretionary accruals (*DAC*)	−0.062	0.032	0.139
	(−6.49)	(6.13)	
Abnormal working capital accruals (*AWCA*)	−0.123	0.073	0.131
	(−3.09)	(3.59)	

(The table is continued on the next page.)

TABLE 7 (Continued)

Notes:

* Annual regressions are estimated for each year from 1984 to 1998 (15 years). On the basis of the 15-year estimates, the average regression coefficient (*t*-value) is presented.

EM = either *DAC* or *AWCA*;

DAC = discretionary accruals measured as the difference between actual total accruals and the fitted values of (2);

AWCA = abnormal working capital accruals measured by differences between actual working capital accruals and expected working capital accruals estimated from (5);

B6 = dummy variable equal to one for Big 6 clients, and zero otherwise;

LEV = ratio of total debt to total assets;

SIZE = firm size measured by natural log of total assets (in millions of dollars);

CFO = cash flows deflated by lagged assets;

ShrDecr = one if the number of shares outstanding decreases by more than 10 percent during a given sample year, and zero otherwise;

ShrIncr = one if the number of shares outstanding increases by more than 10 percent during a given sample year, and zero otherwise;

NewAud = one if it is the first year a new auditor starts auditing the client firm, and zero otherwise;

OldAud = one if it is the last year an old auditor finishes auditing the client firm, and is followed by an auditor change, and zero otherwise;

EM_1 = either *DAC_1* or *AWCA_1*;

DAC_1 = *DAC* in year $t - 1$;

AWCA_1 = *AWCA* in year $t - 1$; and

Lamda = inverse Mills ratios obtained from estimation of (3).

current relative performance (and thus is not mechanically related to our measures of earnings management — *DAC* and *AWCA*). Previous research has found evidence that managers have incentives to overstate earnings through income-increasing accrual choices when they are engaged in share-increasing transactions such as initial public offerings, seasoned equity offerings, or stock financed mergers (e.g., Rangan 1998; Erickson and Wang 1999; Shivakumar 2000). Likewise, managers have incentives to understate earnings through income-decreasing accrual choices when they are engaged in share-decreasing transactions such as management buyouts (Perry and Williams 1994).

As a further robustness check for potential selection bias problems, we measure managerial incentives for upward or downward earnings management based on whether a firm is engaged in share-increasing transactions or share-decreasing

transactions. For this purpose, out of a total of 33,163 firm-year observations, we construct two distinct subsamples: (1) the share-increasing sample with income-increasing incentives; and (2) the share-decreasing sample with income-decreasing incentives. We classify a firm into the share-increasing (share-decreasing) sample if the total number of shares outstanding increases (decreases) by more than 2 percent but less than 50 percent in a given sample year.

We apply the lower and upper percentage criteria (i.e., 2 percent and 50 percent, respectively) for the following reasons. First, in general, share-increasing transactions for publicly traded firms may include not only seasoned equity offerings but also stock splits or stock dividends. Stock splits are likely to lead to a large increase in the number of shares outstanding, and it is not clear whether stock splits are associated with income-increasing or income-decreasing incentives. Likewise, share-decreasing transactions may include not only share repurchases but also reverse stock splits. Similar to stock splits, reverse stock splits, though rare, are likely to result in a large decrease in the number of shares outstanding, and their effects on managerial incentives for earnings management are unclear. To the extent that applying the 50 percent upper cutoff criterion excludes most, if not all, firms with stock splits or reverse stock splits, our sample would become relatively homogeneous with respect to earnings-management incentives.

Second, as firms are, in general, less frequently involved in share-decreasing transactions than share-increasing transactions, applying the more stringent lower percentage criteria such as 10 percent (instead of 2 percent) reduces the number of firms with share-decreasing transactions in our sample drastically.[19] We thus apply the 2 percent lower cutoff criterion to ensure that the size of the share-decreasing sample is reasonable. We assume that managers of firms in the share-increasing (share-decreasing) sample have incentives to overstate (understate) reported earnings through income-increasing (income-decreasing) accrual choices. We then estimate the following regression separately for the two subsamples:

$$EM_{jt} = \delta_0 + \delta_1 B6_{jt} + \delta_2 LEV_{jt} + \delta_3 SIZE_{jt} + \delta_4 CFO_{jt} + \delta_5 NewAud_{jt} + \delta_6 OldAud_{jt} + \delta_7 EM\_1_{jt-1} + \delta_8 Lamda_{jt} + \varepsilon_{jt} \quad (6)$$

In the above model, *EM* denotes either *DAC* or *AWCA*, and *EM_1* denotes either *DAC_1* or *AWCA_1*. Note that while (4) includes the *ShrDecr* and *ShrIncr* variables, (6) excludes them because we partition the sample based on the variables representing share-decreasing and share-increasing transactions.

Panel A of Table 8 presents a summary of the various regression results for (6) for the share-increasing sample (of firms with income-increasing incentives), while panel B reports the same for the share-decreasing sample (of firms with income-decreasing incentives). Consistent with our earlier results presented in Tables 3, 4, and 5, the *B6* coefficient (δ_1) is significantly negative at the 5 percent level for all cases except that it is negative but insignificant ($t = -1.28$) when *AWCA* is used as the dependent variable for the share-decreasing sample.[20] Thus, the results reported in Table 6 alleviate a concern that our earlier results reported in Tables 5,

6, and 7 are possibly driven by potential selection bias arising from a mechanical correlation between the dependent variable (*DAC* and *AWCA*) and the sample partitioning variable (current relative performance).

It is also interesting to note that the explanatory power of the model, measured by the average adjusted R^2, is substantially higher for the share-decreasing sample than for the share-increasing sample. A plausible reason for this difference in the average adjusted R^2 between the two samples is that firms in the income-decreasing sample are more homogeneous than those in the income-increasing sample with respect to managerial incentives for opportunistic earnings management. Under the 2 percent and 50 percent criteria, share-decreasing cases are more likely to arise from share repurchases (which motivate managers to understate earnings) rather

TABLE 8
Summary results of various regressions when managerial incentives for earnings management are measured using a firm's involvement in share-increasing and share-decreasing transactions

$$EM_{jt} = \delta_0 + \delta_1 B6_{jt} + \delta_2 LEV_{jt} + \delta_3 SIZE_{jt} + \delta_4 CFO_{jt} + \delta_5 NewAud_{jt} + \delta_6 OldAud_{jt} + \delta_7 EM\_1_{jt-1} + \delta_8 Lamda_{jt} + \varepsilon_{jt}$$

Dependent variable (*EM*)	Coefficient for the Big 6 dummy (δ_1)	Coefficient for *Lamda* (δ_8)	Average of adjusted R^2 for 15 yearly regressions
Panel A: The sample with income-increasing incentives (2% < %*ShrIncr* < 50%) (n = 9,313)			
Discretionary accruals (*DAC*)	−0.056*	0.028	0.114
	(−2.33)	(2.17)	
Abnormal working capital accruals (*AWCA*)	−0.147	0.089	0.139
	(−2.32)	(2.72)	
Panel B: The sample with income-decreasing incentives (2% < %*ShrDecr* < 50%) (n = 3,209)			
Discretionary accruals (*DAC*)	−0.051	0.029	0.285
	(−2.17)	(2.37)	
Abnormal working capital accruals (*AWCA*)	−0.071	0.035	0.299
	(−1.28)	(1.15)	

Notes:

* Annual regressions are estimated for each year from 1984 to 1998 (15 years). On the basis of the 15-year estimates, the average regression coefficient (*t*-value) is presented.

Variables are as defined in Table 7.

than from reverse stock splits (which are, in general, rare). In contrast, share-increasing cases may be associated with not only seasoned equity offerings (which motivate managers to overstate earnings) but also other share-increasing transactions, the incentive effects of which are unclear (e.g., stock splits, stock dividends, exercises of executive stock options or stock appreciation rights, and conversion of convertible bonds into equities).

In short, to the extent that the 2 percent and 50 percent criteria, though ad hoc, effectively capture the direction of managerial incentives for earnings management, the results in Table 8 buttress our conclusion that Big 6 auditors are more (less) effective than non–Big 6 auditors in constraining opportunistic earnings management when managers have incentives to overstate (understate) earnings by exercising their discretion over accrual choices. Stated alternatively, a conflict or convergence of reporting incentives between the two issuers of financial statements is an important factor determining audit effectiveness differentiation between Big 6 and non–Big 6 auditors. This conclusion is robust to a battery of robustness checks, which alleviates a concern that our results are driven by possible mechanical correlation between our measures of earnings management and the sample partitioning variables.

7. Summary and concluding remarks

Accounting choices are the joint outcome of income-reporting incentives faced by corporate managers and external auditors (i.e., the two issuers of financial statements). In this paper, we investigate the question whether, and how, the divergence or convergence of reporting incentives between the two issuers differentially affects audit effectiveness, which in turn affects managers' ability to exercise their discretion over opportunistic earnings management. We argue that Big 6 auditors have incentives to be more conservative than non–Big 6 auditors in determining reported earnings because potential litigation costs associated with lawsuits against audit failure are likely to be greater for Big 6 auditors than for non–Big 6 auditors. Thus, a conflict (convergence) of reporting incentives arises when managers have incentives to overstate (understate) reported earnings.

Our results show that Big 6 auditors are more (less) effective than non–Big 6 auditors in the presence (absence) of reporting incentive conflicts between the two issuers of financial statements. Our results are robust to a battery of sensitivity tests, including alternative proxies for earnings management and alternative proxies for the direction of earnings-management incentives. Evidence reported in this paper is consistent with the notion that the conflict of reporting incentives over income-increasing accounting choices between the two issuers, coupled with high litigation risk associated with audit failure to detect income overstatement, motivates Big 6 auditors to exercise a heightened degree of professional skepticism on managers' income-increasing accounting choices. In contrast, the convergence of reporting incentives over income-decreasing accounting choices between the two issuers, coupled with the lack of litigation risk associated with income understatement, diminishes Big 6 auditors' incentives to deter/monitor income-decreasing accrual choices. Our evidence suggests that Big 6 clients are allowed to have more

flexibility for income-decreasing accrual choices than non–Big 6 clients and that the conflict of reporting incentives or the lack thereof is a key factor determining audit effectiveness differentiation between Big 6 and non–Big 6 auditors. Given the lack of theory, however, we leave still unresolved the question why Big 6 auditors are less effective than non–Big 6 auditors absent reporting incentive conflicts. Further research on this question is required.

As with many other studies on earnings management, this study has some limitations. In particular, the validity of our findings is subject to the condition that our measures of discretionary accruals and abnormal working capital accruals reasonably capture opportunistic earnings management and that our test designs effectively minimize potential selection bias problems. Potentially fruitful areas for further research include refinements of existing methods for estimating the level of earnings management and for measuring the direction of managerial incentives associated therewith.

Endnotes

1. Note that the residuals of (2) are orthogonal to changes in cash flows deflated by lagged assets (i.e., the last term of (2)).
2. As will be explained/reported in section 5 (sensitivity analysis), we have also used the direction of managers' reporting incentives using the previous period's *CFO* as a benchmark.
3. Our data period is 1983–98 but our sample period is 1984–98 as the 1983 observations are required to obtain empirical measures of lagged variables and the variables that take a change form.
4. In (3), we measure firm size as the natural log of sales (instead of total assets) to make our model comparable to that of Francis et al. 1999. We also measure firm size by the natural log of total assets, and *AbsSAC* (*AbsLAC*) as the absolute value of working capital accruals (depreciation and amortization) deflated by total assets, and then repeat the entire analysis. Though not reported, our results using the alternative measures of firm size, *AbsSAC* and *AbsLAC*, remain qualitatively identical to those reported in the paper.
5. We have also estimated (3) using one-period lagged *AbsSAC* and *AbsLAC* and repeated all regression analyses in the paper. Though not reported, the results using the lagged *AbsSAC* and *AbsLAC* remain qualitatively similar to those reported in the paper.
6. Leuz and Verrecchia (2000) provide a useful discussion of procedures for obtaining the inverse Mills ratios. TSP, an econometric software application, has a routine for computing the inverse Mills ratios from the probit model estimation.
7. COMPUSTAT provides names of 27 different auditor codes ranging from 1 to 27. Auditors with COMPUSTAT codes of 1 to 8 are considered to be "Big 6".
8. Becker et al. (1998) include the absolute of total discretionary accruals (*AbsTAC*) in their accrual choice model and report that *DAC* is significantly negatively associated with *AbsTAC*. However, we do not include *AbsTAC* in (5) because its inclusion may create a mechanical correlation between *DAC* and *AbsTAC* especially when managers choose negative *DAC*. As pointed out by Becker et al. 1998 (17), negative *DAC* is likely

to increase *AbsTAC*, in particular, because nondiscretionary accruals tend to be negative due to depreciation and amortization.

9. Annual excess return on a firm's common stock is measured by the difference between annual return over a fiscal year and the annual equally weighted market return for the same period.
10. Note that panel B of Table 1 presents the same information for all firms in the full sample with either income-increasing or income-decreasing incentives.
11. Given these differences in (post-managed) *NI* between the two samples, the magnitude of the *DAC* differential between Big 6 and non−Big 6 clients is nontrivial, and appears to have economic significance.
12. Some inconsistency in the pooled regression results may be due to differences in sample selection between this study and Francis et al. 1999. Francis et al. (1999) restrict their samples to firms listed on the NASDAQ while our samples are from firms listed on the NYSE and the AMEX. Note here that we are not much concerned about this inconsistency as we estimate the auditor choice model only as a means to control for self-selection bias, not for the purpose of testing any hypothesis.
13. This contrasts with Becker et al. 1998, who report insignificant coefficients for both *ShrDecr* and *ShrIncr.*
14. Implicit in (5) are the assumptions that sales revenue follows a random walk and that the normal level of working capital accruals is a fixed portion of sales revenue (Dechow et al. 1998).
15. Note that *AWCA* in (5) is free from potential errors associated with *DAC* estimated from the Jones 1991 model or its variants.
16. Elgers et al. (2000, 11) argue that DeFond and Park's 1997 research design is tautological in that their results are "guaranteed by errors in column classification that are related mechanically to the magnitude of discretionary accrual estimates".
17. We thank an anonymous referee for bringing this point to our attention.
18. Note here that (2) is estimated for each industry in each year, and thus *DAC* from (2) is estimated using industry-wide parameters of (2), while the sample partitioning on the basis of *ΔCFO* is done for each firm in each year, not on each industry category.
19. As shown in Table 1, in our full sample of 33,163 firm-year observations, only 1.9 percent of Big 6 audited firms and only 1.7 percent of non−Big 6 audited firms engaged in share-related transactions resulting in more than a 10 percent decrease in shares outstanding.
20. The coefficient on *Lamda* is also insignificant for this sample. *Lamda* is significant for the other samples at about the 5 percent level.

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