Potential Errors in Detecting Earnings Management: Reexamining Studies Investigating the AMT of 1986*

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Abstract

In this paper we seek to document errors that could affect studies of earnings management. The book income adjustment (BIA) of the alternative minimum tax (AMT) created apparently strong incentives to manage book income downward in 1987. Five earlier papers using different methodologies and samples all conclude that earnings were reduced in response to the BIA. This consensus of findings offers an opportunity to investigate our speculation that methodological biases are more likely when there appear to be clear incentives for earnings management. A reexamination of these studies uncovers potential biases related to a variety of factors, including choices of scaling variables, selection of affected and control samples, and measurement error in estimated discretionary accruals. A reexamination of the argument underlying these studies also suggests that the incentives to manage earnings are less powerful than initially predicted, and are partially mitigated by tax and non-tax factors. As a result, we believe that the extent of earnings management that occurred in 1987 in response to the BIA remains an unresolved issue.

Keywords   Alternative minimum tax; Book income adjustment; Earnings management; Methodological biases

Condensé

La présente étude vise à réexaminer la façon dont les choix méthodologiques effectués par les chercheurs dans leurs études empiriques peuvent occasionner un biais systématique dans les conclusions des travaux qu'ils publient, en particulier lorsque leurs convictions de départ à l'égard des hypothèses sont fortes. Ces biais sont aussi davantage probables lorsque la mesure de la variable de réaction est entachée d'une erreur considérable. Consciemment ou inconsciemment, les chercheurs sapent plus ou moins les données en envisageant d'autres procédés d'expérimentation jusqu'à ce que les résultats qu'ils obtiennent confirment leurs convictions de départ. Cette tendance est souvent renforcée par leur perception selon laquelle les éditeurs de revues sont moins enclins à publier des documents qui n'aboutissent pas à des conclusions fermes et au rejet de l'hypothèse nulle.

Le contexte précis sur lequel les auteurs se penchent dans la présente étude est celui de la gestion du bénéfice en réponse au rajustement BIA que requiert l'impôt minimum de remplacement (AMT) exigé à la suite de la *Tax Reform Act* de 1986. La technologie utilisée pour déceler la gestion du bénéfice n'est pas encore au point et la mesure des produits et des charges discrétionnaires (ou éléments discrétionnaires) est très imprécise ; en outre, la réforme fiscale semble avoir incité fortement et indubitablement les entreprises touchées par la BIA à réduire le bénéfice comptable déclaré en 1987. En fait, l'on s'attendait à ce que la gestion du bénéfice dans le but d'atténuer la BIA ait une ampleur telle que les professionnels influents de la comptabilité avaient prévu que la BIA aurait des conséquences néfastes sur la neutralité et la comparabilité des rapports financiers.

Les faits consignés dans cinq études empiriques (Gramlich, 1991 ; Boynton, Dobbins et Plesko, 1992 ; Manzon, 1992 ; Dhaliwal et Wang, 1992 ; Wang, 1994) viennent appuyer le point de vue « traditionnel » selon lequel les entreprises touchées par la BIA ont constaté des éléments discrétionnaires réduisant le bénéfice, en 1987. Dans leurs premières recherches sur cette question (menées concurremment à ces travaux), les auteurs de la présente étude ont aussi consigné des données systématiques indiquant que les entreprises touchées par la BIA avaient réduit leur bénéfice comptable en 1987. Ils ont cependant découvert que les réductions du bénéfice comptable observées dans les entreprises étudiées découlaient de la méthode utilisée pour subdiviser l'échantillon. Cette méthode faussait les résultats de façon similaire, même dans les années ne faisant pas partie de la période d'événements. Une fois contrôlé cet effet mécanique, il devenait impossible pour les auteurs de recueillir la preuve que les entreprises touchées par la BIA géraient le bénéfice.

Les auteurs, déconcertés par la divergence de leurs conclusions révisées et de celles des cinq études précédentes, ont entrepris de réexaminer plus attentivement la logique qui sous-tend le point de vue traditionnel de même que la méthodologie employée par les auteurs précédents. L'examen de la logique a mis au jour deux catégories de raisons pour lesquelles les entreprises peuvent avoir choisi de ne pas réduire leur bénéfice comptable en 1987, en réponse à la BIA (ce point de vue inverse est appelé dans la suite « nouvel argument »). Premièrement, le point de vue ou argument traditionnel ne tient pas compte de toutes les conséquences d'une diminution des taux d'imposition en vigueur qui a pour effet d'encourager les entreprises à procéder au report de bénéfice *imposable* dans le sens opposé, de 1986 à 1987. Les chercheurs précédents ont reconnu que ces encouragements à procéder ainsi diminuent la probabilité d'observer des réductions du bénéfice comptable en 1987, étant donné que les mesures prises pour opérer le report de bénéfice imposable entraînent

habituellement aussi le report de bénéfice comptable dans le même sens. Ils n'ont cependant pas mis en relief le fait que ces encouragements sont plus marqués chez les entreprises touchées par la BIA, ce qui fait qu'il est plus difficile de détecter les reports de bénéfice comptable dans le sens opposé. Fait plus important encore, le report d'un dollar de bénéfice imposable et de bénéfice comptable de 1986 à 1987 donne lieu à des économies d'AMT plus importantes que le report d'un dollar de bénéfice comptable de 1987 à 1986, et le report d'une part importante de bénéfice imposable et de bénéfice comptable peut, en fait, supprimer l'AMT. Deuxièmement, les auteurs dressent la liste des raisons pour lesquelles le point de vue traditionnel peut entraîner la sous-estimation des coûts de la réduction du bénéfice comptable et la surestimation des avantages de l'évitement de l'AMT. Ils posent en effet l'hypothèse que d'autres réactions envisageables à la BIA pourraient être plus intéressantes pour les entreprises touchées que la gestion du bénéfice. Ils appuient cette hypothèse sur les constatations sommaires tirées des données relatives aux déclarations fiscales des entreprises pour 1987 et les exercices subséquents, en ce qui a trait à l'incidence de l'AMT et de la BIA.

Du côté des données empiriques, les auteurs réexaminent et reproduisent, lorsque la chose est possible, chacune des cinq études. Dans chaque cas, ils observent des effets méthodologiques susceptibles d'avoir biaisé les conclusions relatives au bénéfice en faveur du point de vue traditionnel. Outre l'effet mécanique relevé ci-dessus quant à la méthode de subdivision de l'échantillon, ces biais proviennent de la sélection des groupes de contrôle, du choix des déflateurs, et des procédés utilisés pour estimer les éléments discrétionnaires. Les auteurs proposent une série de contrôles que pourraient appliquer systématiquement les chercheurs qui étudient la gestion du bénéfice afin de s'assurer de la robustesse de leurs conclusions.

Dans la première des études réalisées sur le sujet, Gramlich (1991) utilise le ratio impôts payés/charge d'impôts (TP/TE) des années 1984 à 1986 pour subdiviser son échantillon en entreprises touchées par la BIA (quartile inférieur) et en groupes de contrôle (quartile supérieur). Bien que les conclusions de Gramlich confirment que les entreprises touchées par la BIA ont géré le bénéfice comptable de 1987 à la baisse, les auteurs de la présente étude montrent que les entreprises ayant un faible ratio TP/TE dans d'autres années ne faisant pas partie de la période d'événements ont également tendance à réduire le bénéfice de l'exercice suivant. Aucune observation ne permet de conclure que la constatation d'éléments discrétionnaires vient réduire le bénéfice de 1987, une fois contrôlé cet effet mécanique ou lorsque d'autres méthodes sont employées pour déterminer quelles sont les entreprises touchées par la BIA. L'analyse des auteurs met en relief l'importance générale de l'utilisation d'événements fictifs pour évaluer la mesure dans laquelle les méthodes expérimentales faussent les résultats. Elle confirme également la nécessité d'établir si les résultats résistent à l'application de différentes mesures des variables sous-jacentes de subdivision et de réaction.

Boynton et al. (1992) abordent le sujet sous un angle innovateur, utilisant les données tirées des déclarations fiscales produites par les entreprises auprès de l'IRS (Internal Revenue Service). Ils évaluent d'abord la gestion du bénéfice de 1987 à l'aide du modèle Jones (1991) (approximation des éléments discrétionnaires ou DAP), et soustraient ensuite la DAP du bénéfice comptable déclaré dans le formulaire de l'AMT (formulaire 4626) pour recalculer l'AMT à partir du bénéfice comptable « antérieur à l'AMT » (évaluant ainsi la situation de l'entreprise sans tenir compte des éléments discrétionnaires nets estimés). Boynton et al. sont donc en mesure d'identifier non seulement les entreprises qui ont présenté la BIA dans leurs déclarations fiscales, mais également celles qui auraient été touchées par la BIA mais

qui ont géré leur bénéfice de manière à y échapper. Les auteurs de la présente étude n'ayant pas accès aux données provenant des déclarations fiscales utilisées par Boynton *et al.*, ils se contentent de réinterpréter les résultats qu'ont obtenus ces derniers.

Le plan de recherche de Boynton *et al.* soulève un problème potentiel : la *DAP* est utilisée à la fois pour déterminer quelles entreprises sont exposées à la BIA (la variable indépendante) et pour évaluer les réactions à la BIA des entreprises optant pour la constatation d'éléments discrétionnaires (la variable dépendante). Cette méthode est source de préoccupation du fait que la mesure de la *DAP* est erronée et que les entreprises constatent des éléments discrétionnaires pour des raisons autres que la BIA, ce qui crée en retour un biais en faveur de la constatation d'un lien de cohérence avec l'argument traditionnel. Les auteurs de la présente étude constatent que les autres méthodes permettant d'identifier les entreprises touchées par la BIA et qui ne sont pas contaminées par l'utilisation de la *DAP* ne permettent pas de tirer des conclusions qui appuieraient l'argument traditionnel. Ainsi, les entreprises assujetties à l'AMT ont constaté des éléments discrétionnaires positifs significatifs en 1987, ce qui corrobore non pas l'argument traditionnel, mais le nouvel argument. L'analyse des auteurs illustre combien il est important de confirmer les résultats obtenus en utilisant différentes mesures des variables dépendante et indépendante, en particulier lorsque ces mesures sont sujettes à l'erreur aléatoire dans l'estimation des éléments discrétionnaires.

Dhaliwal et Wang subdivisent leur échantillon d'entreprises ayant déclaré un bénéfice comptable de 1985 à 1987 en entreprises touchées par la BIA et en groupes de contrôle, à partir des taux d'imposition effectifs (*ETR*) de 1986 inférieurs ou supérieurs à 23 pour cent, l'*ETR* correspondant au ratio impôts payés/bénéfice comptable avant impôts. Les éléments discrétionnaires sont mesurés en fonction du *CTMD*, le changement d'exercice en exercice du ratio écarts temporaires et permanents/bénéfice comptable avant impôts (l'inversion de la moyenne relative à cette variable est explicitement vérifiée). L'écart temporaire et permanent, qui correspond à la différence entre le bénéfice comptable avant impôts et le bénéfice imposable, présente l'avantage de ne pas être influencé par les efforts de gestion du bénéfice comptable de même que du bénéfice imposable. Dhaliwal et Wang concluent que les entreprises de leur échantillon touchées par la BIA ont opéré un report de bénéfice de 1987 à 1986, en se basant sur la constatation des valeurs négatives significatives du *CTMD* en 1987 et de ses valeurs positives significatives en 1986.

Les observations des auteurs de la présente étude donnent à penser que les résultats obtenus par Dhaliwal et Wang sont sensibles au fait qu'ils utilisent le bénéfice comptable avant impôts comme déflateur pour le *CTMD*. L'examen des différents déflateurs possibles (actifs totaux et chiffre d'affaires) et de la variation chronologique du bénéfice comptable et des écarts temporaires et permanents (la variable d'intérêt) donne à penser que les résultats qu'obtiennent Dhaliwal et Wang sont attribuables à des augmentations en 1987 (et des diminutions en 1986) coïncidentes du bénéfice comptable des entreprises touchées par la BIA plutôt qu'aux changements prévus dans les écarts temporaires et permanents. Ces constatations mettent en relief combien il importe d'étudier à fond la mesure dans laquelle les résultats observés sont sensibles au déflateur utilisé.

Manzon (1992) subdivise son échantillon d'entreprises en trois groupes, en fonction du taux d'imposition marginal immédiat estimatif auquel est assujetti leur bénéfice comptable : 1) égal à 7,5 pour cent (entreprises assujetties à l'AMT-ITC) ou à 10 pour cent (entreprises assujetties à l'AMT intégral), 2) égal à 1 pour cent (entreprises assujetties à l'AMT-NOL et

à l'AMT-FTC) et 3) égal à 0 pour cent (entreprises dont le bénéfice comptable est inférieur au bénéfice imposable minimum potentiel. Manzon examine l'évolution de la charge liée à l'amortissement, à l'épuisement et à la dépréciation (ci-après l'« amortissement ») pour déterminer si les entreprises du premier groupe, assujetties aux taux d'imposition marginaux immédiats les plus élevés, réduisaient le bénéfice comptable (en augmentant l'amortisse-ment) davantage que les entreprises des deux autres groupes, assujetties à des taux d'impo-sition marginaux immédiats inférieurs. Comme celle de Dhaliwal et Wang, la mesure de la gestion du bénéfice prise par Manzon est axée sur les efforts visant à gérer le bénéfice comptable sans toucher le bénéfice imposable. Bien que Manzon ait constaté que les entre-prises des trois groupes modifiaient toutes l'amortissement de manière à augmenter le bénéfice en 1987 (c'est-à-dire que, contrairement à ses prévisions, elles diminuaient l'amortisse-ment), il a conclu que cette observation venait appuyer l'argument traditionnel, étant donné que les entreprises du premier groupe affichaient des réductions de l'amortissement plus faibles que les entreprises des deuxième et troisième groupes.

La conclusion de Manzon s'articule sur deux hypothèses : a) l'amortissement des entreprises assujetties à l'AMT aurait été moindre en 1987 sans la BIA et b) les entreprises assujetties à de faibles taux d'imposition marginaux immédiats présentent l'élément de con-trôle par rapport auquel il est approprié de comparer les entreprises « les plus susceptibles » d'être assujetties à un taux d'imposition marginal élevé. L'analyse approfondie des change-ments observés dans l'amortissement dans l'ensemble de l'économie et de l'échantillon des entreprises de contrôle débouche sur d'importantes questions relatives à la validité de ces deux hypothèses. Enfin, les auteurs de la présente étude croient que la gestion de l'amortisse-ment est une façon peu commode de modifier le bénéfice comptable aux fins de la BIA. L'examen des notes figurant dans les rapports annuels de 1987 des entreprises de l'échan-tillon de Manzon le confirme, puisqu'il révèle que peu d'entreprises modifient leur méthode d'amortissement ou leurs estimations, et rien n'indique dans ces notes que les entreprises assujetties à un taux d'imposition élevé aient procédé à des modifications systématiques dans le but d'augmenter l'amortissement. Contrairement à l'usage, selon lequel les entreprises peuvent préférer ne pas révéler qu'elles ont géré le bénéfice, les entreprises sont ici manifeste-ment encouragées à mettre en relief le fait que la réduction du bénéfice a pour but de diminuer les impôts et n'est pas le symptôme d'un déclin de la rentabilité. Compte tenu de ces faits, l'absence d'information sur les modifications apportées à l'amortissement soulève d'autres préoccupations encore au sujet de l'interprétation que fait Manzon des résultats qu'il obtient. La présente analyse indique pourquoi les échantillons de contrôle méritent une étude attentive, puisque les résultats comparatifs observés pourraient être induits par des caractéristiques inattendues de l'échantillon de contrôle.

L'échantillon de Wang (1994) est constitué d'entreprises qui ont présenté à la fois un bénéfice comptable plus élevé (à la suite des modifications comptables apportées) et des élé-ments d'impôts reportés relatifs aux provisions et réserves dans les notes aux états financiers de 1987. Les éléments d'impôts reportés reliés aux provisions et réserves sont divisés par le taux d'imposition prévu par la loi, ce qui permet d'obtenir les écarts temporaires. La variable dépendante, soit la modification des estimations comptables ($CHGACC$), est mesurée au moyen des écarts temporaires reliés aux modifications des estimations comptables, le bénéfice comptable retraité étant utilisé comme déflateur. L'on mesure le bénéfice comptable retraité en ajoutant au bénéfice comptable net l'effet net des impôts fédéraux et étrangers, des activités

abandonnées et des postes extraordinaires, et l'effet cumulatif des modifications des méthodes comptables. Wang constate que le *CHGACC* est significativement négatif (positif) en 1987 (1986), mais qu'aucun profil semblable ne ressort chez les entreprises de contrôle classées en fonction du secteur d'activité et de la taille. Comme celle de l'étude de Dhaliwal et Wang, la reproduction de cette étude mène à la conclusion que les résultats sont sensibles au choix du déflateur : les résultats observés ne sont pas les mêmes lorsque les actifs totaux ou le chiffre d'affaires servent de déflateur, plutôt que le bénéfice comptable retraité. Il convient cependant de souligner que cette préoccupation est moins bien étayée que les préoccupations que soulève l'étude de Dhaliwal et Wang, étant donné que les auteurs n'ont pas été en mesure de reproduire avec autant de précision les résultats de l'étude initiale.

Il importe de noter que les auteurs de la présente étude ont pour but de mettre en lumière des biais méthodologiques potentiels. Ils cherchent donc systématiquement à diagnostiquer l'origine de ces biais dans les recherches antérieures portant sur la gestion du bénéfice par suite de la BIA. Les auteurs ont également fait ressortir méthodiquement les arguments contraires au point de vue traditionnel. Il faut donc lire la présente étude en parallèle avec les travaux de recherche initiaux avant de pouvoir conclure que la BIA a provoqué la gestion du bénéfice. Malgré cette mise en garde, les observations des auteurs laissent croire que rien ne permet d'établir clairement le bien-fondé de l'argument traditionnel selon lequel les entreprises touchées par la BIA ont constaté des éléments discrétionnaires ayant eu pour effet de réduire le bénéfice en 1987. Si tel est le cas, cette conclusion peut être interprétée de plusieurs façons différentes. Il est possible que les économies d'AMT n'aient pas été suffisamment importantes pour encourager les gestionnaires à gérer le bénéfice comptable. Il est aussi possible que divers coûts non liés à l'impôt aient été suffisamment élevés pour dissuader les entreprises de gérer leur bénéfice ou que les entreprises touchées par la BIA aient trouvé d'autres façons de réduire l'incidence de l'AMT. Une autre explication, tout à fait différente, serait le faible pouvoir des techniques actuellement utilisées pour détecter la gestion du bénéfice.

1. Introduction

The technology used for detecting earnings management is still emerging. It continues to identify discretionary accruals with considerable error (for examples, see Dechow, Sloan, and Sweeney 1995; Kang and Sivaramakrishnan 1995; Guay, Kothari, and Watts 1996; Healy 1996; Thomas and Zhang 2000). We speculate that in cases where strong prior beliefs about hypotheses exist, it is possible that results confirming those beliefs are in practice accepted more often than they should be by chance. Specifically, some portion of the documented results could be caused by methodological choices that happen to skew results in the expected direction.[1]

The book income adjustment (BIA) of the alternative minimum tax (AMT) imposed by the Tax Reform Act of 1986 offers an opportunity to investigate our speculation. This feature of the tax law change appeared to create strong and unambiguous incentives for firms affected by the BIA (hereafter referred to as "affected firms") to reduce reported 1987 book income. In fact, the extent of earnings management designed to mitigate the BIA was expected to be so widespread that leading accounting professionals predicted that the BIA would have a damaging effect on the neutrality and comparability of financial reports (Financial

Accounting Standards Board [FASB] 1986a-c; Shapiro 1986; General Accounting Office 1986; Haspel and Wertlieb 1987).

Five empirical studies support this "traditional" view (Gramlich 1991; Boynton, Dobbins, and Plesko 1992; Manzon 1992; Dhaliwal and Wang 1992; Wang 1994). Notwithstanding some caveats, the overall conclusions in all five papers are unequivocal: statistically and economically significant income-decreasing accruals were made in 1987 by affected firms.[2] Some also find evidence of anticipatory income-*increasing* accruals in 1986, before the BIA came into effect, providing further confirmation that the BIA caused earnings management.

As explained in more detail in section 3 under the heading "Gramlich 1991", in our initial efforts to investigate this issue (begun concurrently with these studies), we too documented systematic evidence of affected firms reducing book income in 1987. However, we discovered that the book income decreases we observed among our treatment firms resulted from the procedure used to partition the sample. This procedure caused similar spurious results to be observed even in nonevent years. Once we controlled for this mechanical effect, our statistically significant results disappeared; we could no longer find evidence of affected firms managing earnings in response to the BIA.

Perplexed by the inconsistency between our revised conclusions and those of the five earlier papers, we undertook a more careful reexamination of both the logic underlying the traditional view and the methodology used by the earlier studies. Regarding the underlying logic, we find two sets of reasons why firms might not decrease 1987 book income in response to the BIA (this opposing view is referred to hereafter as the "new" argument). First, the traditional view does not consider all of the implications of a contemporaneous tax rate decline, which creates incentives for firms to shift *taxable* income in the opposite direction, from 1986 to 1987 (Scholes, Wilson, and Wolfson 1992; Omer 1992). Prior research has recognized that these incentives to shift taxable income reduce the likelihood of observing book income decreases in 1987, since actions taken to shift taxable income typically shift book income in the same direction (Dhaliwal and Wang 1992, 19). That research has not emphasized, however, that these incentives are more severe for affected firms, making it harder to detect book income shifts in the opposite direction. More important, shifting one dollar of taxable and book income from 1986 to 1987 results in greater AMT savings than shifting one dollar of book income from 1987 to 1986, and large shifts in taxable and book income can actually eliminate exposure to the AMT.[3] Second, we compile a list of reasons why the traditional view might understate the costs of reducing book income and overstate the benefits of avoiding the AMT. In effect, we speculate that other responses available to affected firms might be more attractive than managing earnings in response to the BIA.

Turning to the empirical evidence, we reexamine each of the five studies and, where possible, replicate them. In each case, we find methodological effects that potentially bias the results in favor of the traditional view. In addition to the mechanical effect noted above relating to sample partition procedures, the factors generating these biases are the selection of control groups, the choice of scaling variables, and procedures used to estimate discretionary accruals.

Our paper should be read in conjunction with the original research before concluding whether the BIA caused earnings management. Given our motivation of highlighting *potential* methodological biases, we systematically emphasize counter-arguments to the traditional view and focus on providing empirical support for those counterarguments. Thus, we do not claim that earnings were not managed in response to the BIA; rather, we wish only to provide sufficient argument and evidence to convince readers that the case is not closed. In the next section, we describe the BIA and explain the different tax and nontax factors that create incentives to manage book income. In section 3, we review the five previous studies on earnings management in response to the BIA. In section 4, we offer our conclusions.

2. Managers' incentives to adjust book income to mitigate AMT

The traditional view underlying prior research is summarized in the first subsection, and the two main features of our new argument are described in the next two subsections. In the fourth subsection, we summarize evidence obtained from tax return data for 1987 and subsequent years, to describe the extent to which the AMT and the BIA affected firms.

The traditional argument

The following is a simplified overview of the AMT and the BIA; relevant details are introduced later. (For a more comprehensive description, see General Accounting Office 1995.) Concerned that many firms with strong economic profitability paid little or no corporate income tax, the U.S. Congress created a corporate alternative minimum tax in 1986. Relative to the regular tax, the AMT is computed at a lower rate applied to a larger base (preliminary AMT income) that is obtained by reversing some of the tax preferences (such as rapid depreciation) available under the regular tax. The BIA, which equals 50 percent of the difference between book and preliminary AMT income, is then added back to get AMT income (see column 2 in Table 1 for an illustration). The tentative minimum tax is 20 percent of AMT income, and the AMT is the excess of this amount over the regular tax. AMT paid is carried forward as a credit (the AMT credit) that is available to reduce regular tax in future years when the regular tax exceeds the tentative minimum tax (see column 3 in Table 1).[4] Although the AMT is a parallel tax system, the presence of the AMT credit makes it resemble a prepayment of tax rather than a permanent increase in tax liability, at least in cases where the regular tax subsequently exceeds the tentative minimum tax. The incentives to manage book income remained in place for only three years. As scheduled when the corporate AMT was created in 1986, the BIA was replaced in 1990 by the adjusted current earnings (ACE) adjustment, which is similar in concept to the BIA but is unrelated to book income.

The traditional argument is based on the taxes saved by reducing book income in 1987 (see Table 1, columns 4 and 5 versus columns 1 and 2). Under the AMT, affected corporations paid 10 cents less in taxes in 1987 for each dollar of income-decreasing book-only accruals made, since the 20 percent AMT only taxed one-half of the excess of book income over taxable income. That is, they incurred an "imme-

diate marginal tax rate" of 10 percent on each additional dollar of book-only income. Note that these immediate marginal tax rates are different from typical marginal tax rates: they represent the tax impact when one dollar of income is shifted, and they capture only the current or immediate impact, not taking into account the expected value of the AMT credit. For discussion purposes, firms facing a 10 percent immediate marginal tax rate in 1987 are referred to as "full-AMT firms".

Many firms affected by the AMT face an immediate marginal tax rate on book income that is lower than 10 percent. Some firms with investment tax credit (ITC) carryovers face an immediate marginal tax rate of 7.5 percent ("AMT-ITC firms"), and AMT restrictions on the use of net operating tax loss (NOL) carryovers and foreign tax credits (FTCs) reduce the rate to 1 percent for some firms ("AMT-NOL firms" and "AMT-FTC firms", respectively). These lower immediate marginal tax rates, which reflect the lower benefits of shifting book income, are due to limits on the extent to which NOLs can be used to reduce AMT income (not below 10 percent of AMT income) and FTC and ITC carryovers can be used to reduce AMT (not below 10 percent and 75 percent of AMT, respectively). (See General Accounting Office 1995, 29–31.) For example, reducing 1987 book income by $1 for AMT-FTC firms reduces the AMT before FTC by $0.10, but the net AMT saved is $0.01 because 90 percent of the AMT saved had been protected by FTCs.

In the literature (e.g., Manzon 1992), firms with lower immediate marginal rates are collectively labeled "limit" firms, and are used as a control sample for affected firms with higher marginal tax rates. That is, the AMT-NOL and AMT-FTC firms are included in the limit category, and the AMT-ITC set is combined with the full-AMT firms to form the sample of affected firms.

Review of reasons why managers might not reduce book income, despite the tax savings

To illustrate the competing forces not captured by the traditional argument, we discuss one tax-based incentive and three possible nontax incentives for firms to avoid reducing book income in 1987 in response to the BIA. While empirical investigation of the extent to which affected firms were swayed by these incentives is beyond the scope of this paper, we cite relevant evidence gathered by others.

First, the cost of paying the AMT could be reduced substantially by the present value of the expected AMT credit used in future years. In effect, the cost of paying the AMT is the interest forgone during the period before it is recovered as a credit. Column 6 of Table 1 describes the case when the AMT credit is used in the following year, 1988, by a full-AMT firm. In this extreme case, the present value effect of the savings generated by reducing book income in 1987 by a dollar falls to below 1 percent, assuming a 10 percent after-tax cost of capital. While we do not know when managers expected to recover AMT paid in 1987, studies of tax returns filed during these years (see "Evidence from tax returns on the incidence of AMT and BIA", below) find that a large fraction of AMT paid was recovered fairly quickly, and that most firms switched between paying the AMT and not paying the AMT (e.g., Gerardi, Milner, and Silverstein 1993). In other words, if observed outcomes reflect expectations, managers of full-AMT firms may have assumed in

TABLE 1

Effects of shifting book and tax income around 1987 in response to the BIA of the AMT

The base case (columns 1–3) is constructed to illustrate the simplest case of a "full-AMT" firm, which faces a 10 percent immediate marginal tax rate. The numbers for 1988 are selected to utilize a credit in 1988 for any AMT paid in 1987. The traditional argument for shifting book income from 1987 to 1986 is developed in columns 4–6. The new argument, which suggests shifting of taxable (and book) income from 1986 to 1987, is illustrated in columns 7–9. The effect of shifting book and taxable income on BIA exposure is shown in columns 10–12.

	Base case			Effect of shifting $1 of book income *only* from 1987 to 1986			Effect of shifting $1 of book and taxable income from 1986 to 1987			Effect of shifting $100 of book and taxable income from 1986 to 1987		
	1	2	3	4	5	6	7	8	9	10	11	12
Year	1986	1987	1988	1986	1987	1988	1986	1987	1988	1986	1987	1988
Pre-tax book income	800.00	1,075.00	400.00	801.00	1,074.00	400.00	799.00	1,076.00	400.00	700.00	1,175.00	400.00
Taxable income before												
NOL deduction	300.00	300.00	300.00	300.00	300.00	300.00	299.00	301.00	300.00	200.00	400.00	300.00
+ Non-BIA adj. & pref.		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00
= Preliminary AMT												
income		300.00	300.00		300.00	300.00		301.00	300.00		400.00	300.00
+ BIA		387.50	50.00		387.00	50.00		387.50	50.00		387.50	50.00
= AMT income		687.50	350.00		687.00	350.00		688.50	350.00		787.50	350.00
Tax base												
Regular taxable income	300.00	300.00	300.00	300.00	300.00	300.00	299.00	301.00	300.00	200.00	400.00	300.00
AMT income		687.50	350.00		687.00	350.00		688.50	350.00		787.50	350.00

(The table is continued on the next page.)

TABLE 1 (Continued)

	Base case			Effect of shifting $1 of book income *only* from 1987 to 1986			Effect of shifting $1 of book and taxable income from 1986 to 1987			Effect of shifting $100 of book and taxable income from 1986 to 1987		
	1	2	3	4	5	6	7	8	9	10	11	12
Year	1986	1987	1988	1986	1987	1988	1986	1987	1988	1986	1987	1988
Tax												
Regular tax (46%, 40%, and 34% in 1986, 1987, and 1988)	138.00	120.00	102.00	138.00	120.00	102.00	137.54	120.40	102.00	92.00	160.00	102.00
Tentative minimum tax		137.50	70.00		137.40	70.00		137.70	70.00		157.50	70.00
Analysis of tax liability												
Regular tax	138.00	120.00	102.00	138.00	120.00	102.00	137.54	120.40	102.00	92.00	160.00	102.00
AMT (credit used)		17.50	(17.50)		17.40	(17.40)		17.30	(17.30)		0.00	0.00
Total tax	138.00	137.50	84.50	138.00	137.40	84.60	137.54	137.70	84.70	92.00	160.00	102.00
Tax saved (paid) relative to base case				0.00	0.10	(0.10)	0.46	(0.20)	(0.20)	46.00	(22.50)	(17.50)

1987 that the cost of paying the AMT *at the margin* (by not decreasing book income) was considerably less than the 10 percent immediate marginal tax rate, because they expected to recover it fairly soon.

Second, managers may have perceived that there were political costs to be avoided (or benefits to be gained) by paying the AMT. The AMT was a direct outgrowth of assertions by such groups as Citizens for Tax Justice (e.g., see their 1986 report) that middle-class individuals often paid more income tax than large corporations (*Wall Street Journal* 1986). Given the political climate at the time, corporations that were successful in avoiding tax under the broad AMT might be targeted for further tax reform in the future. Hence, to avoid further repercussions, and to demonstrate their support for the new, business-favorable, broad-base, low-rate tax regime, firms faced incentives to pay the minimum tax rather than find ways to avoid that tax.[5]

Third, altering the pattern of income recognition, relative to an optimal pattern set before considering the BIA, creates potential contracting costs. For example, decreases in 1987 book income may lead to higher debt-contracting costs and technical default, since many loan agreements specify minimum levels of ratios such as times-interest-earned (e.g., Watts and Zimmerman 1990). Similarly, altering the pattern of reported book income will affect the pattern of managerial compensation, via explicit or implicit links between compensation and reported income (Healy 1985).

Finally, managers may be concerned that reduced book earnings in 1987 could result in an undervalued stock price. At a minimum, managers would need to make as explicit as possible that the decrease in book income is transitory and occurred for tax-saving purposes only. Weiss (1999) provides evidence that firms reporting book income decreases caused by the 1993 increase in statutory tax rates tend to concentrate that charge in one year, and also highlight its transitory nature. (Weiss also finds that firms facing income increases, on the other hand, are less likely to discuss the issue, and tend to smooth it over more years.) Our inability to find any such explicit communication in the footnotes of 1987 annual reports of firms paying the AMT (identified from the National Automated Accounting Research System [NAARS]) suggests that such book income decreases did not occur.[6]

The incentive to transfer taxable income from 1986 to 1987

As noted in prior research, the 1986 tax law change also created an incentive to transfer taxable income from 1986 to 1987 and subsequent years. Scholes et al. (1992) discuss and analyze this incentive, caused by a two-year phased-in reduction of statutory maximum corporate tax rates from 46 percent to 34 percent. Firms seeking to shift taxable income between periods are likely to use techniques that cause book income to be similarly affected (e.g., Cloyd, Pratt, and Stock 1996; Guenther 1994; Guenther, Maydew, and Nutter 1997). Firms engaged in ongoing multiperiod tax planning are limited in their ability to shift only taxable income between adjacent periods in response to unexpected changes in incentives. The options available to them include altering the timing of certain discretionary revenue and expense streams, and these items typically affect both taxable and book income.

In other words, the decline in regular tax rates created a general incentive to shift taxable income that results in both book and taxable income being shifted from 1986 to 1987 and from 1987 to 1988.

While prior research has discussed these general incentives to transfer taxable income associated with the tax rate decline, it has not emphasized the two implications that form the basis of our new argument. First, affected firms have stronger incentives than others to increase taxable (and book) income in 1987 because of the greater decline in tax rates. Specifically, shifting a dollar of book and tax income reduces taxes of affected firms in 1986 by $0.46 and increases taxes in 1987 by $0.20 (compare columns 7 and 8 with columns 1 and 2 in Table 1). In effect, the immediate marginal tax rate for AMT firms declines from 46 percent in 1986 to 20 percent in 1987, and this decline dwarfs the regular tax rate decline from 46 to 40 percent. Also, while regular tax firms faced a second tax rate decline from 1987 to 1988, AMT firms had no further incentives to shift taxable income from 1987 to 1988. Finally, shifting both book and taxable income from 1986 reduces the AMT burden, relative to shifting book income alone (AMT paid in column 8 of Table 1 is lower than that in column 5), and could even eliminate it when sufficient income shifting occurs (no AMT is paid in columns 10–12).[7] The discussion here represents a summary of comprehensive spreadsheet simulations we conducted to identify explicitly the net benefits of shifting book and/or taxable income. As illustrated by the cases covered in Table 1, the simulations must consider different features of the tax law such as the different regular tax and AMT bases and the expected present value of subsequent recovery of AMT paid via credits.[8]

Second, after such transfers, the BIA would no longer be a factor for some affected firms (group A). Other firms that are still affected by the BIA (group B) may or may not seek to reduce book income in 1987, because of (a) the minimal tax benefits once AMT credits are considered, (b) the competing nontax incentives described earlier, or (c) the alternative tax-based approaches available. From an empirical perspective, studies that do not focus on book-only accruals, and include accruals that may have shifted both book and tax income, will not observe a decline in 1987 book income for firms in group A, and are unlikely to observe a decline in 1987 book income for firms in group B because of the more powerful incentives to shift taxable income in the opposite direction. Studies that are able to isolate book-only accruals have the potential to document a reduction in 1987 book income, but only for group B firms. The open empirical issues that remain are the number of firms in group B, the extent to which those firms made book-only accruals to reduce 1987 income, and the ability of current methodologies to detect such accruals.

Evidence from tax returns on the incidence of AMT and BIA

Evidence of the actual incidence of the AMT and BIA during 1987 to 1989 from studies that examined tax-return data offers some insights into expectations held by managers as they planned to mitigate the AMT tax burden. It is important to note that this evidence may be biased against the traditional argument, because it represents the situation after firms had undertaken actions to respond to the tax change. Specifically, many affected firms may have managed earnings to the point

where they did not pay any AMT in 1987, and the firms that elected to pay the AMT could be an unrepresentative sample of affected firms (because they perceived the AMT as being low cost or earnings management as being high cost). The evidence in Boynton et al. 1992, discussed further in section 3 under the heading "Boynton et al. 1992", supports this concern. In fact, four of the five prior studies recognized this concern and did not use the reported tax status as of 1987 to identify affected firms; only Manzon 1992 examined firms that paid the AMT in 1987.

Notwithstanding these concerns, the evidence gleaned from a variety of studies (e.g., General Accounting Office 1995; Lyon 1997; Gill and Treubert 1992; Gerardi, Milner, and Silverstein 1992, 1993) can be used to evaluate the validity of the assumptions underlying our new argument. Note that these studies generally cover all corporate taxpayers rather than the subset of larger, publicly held firms examined in prior earnings management studies, and they investigate a longer period (typically from 1987 to 1991) that includes years after the BIA period, when exposure to the AMT increased considerably. Where possible, we focus on the 1987 to 1989 BIA period and on the evidence gathered for the large-firm panel (defined as firms with total assets greater than $50 million).

The consensus view is that both the AMT and the BIA were important for a substantial fraction of large firms. On average, about 20 percent of the large firms paid the AMT in each year (over half paid the AMT in at least one year), and about 40 percent of all firms paying the AMT paid no regular taxes. The BIA had a bigger dollar impact than any other adjustment, and was second only to the depreciation adjustment in terms of the number of firms affected (between 41 percent of AMT firms in 1989 and 48 percent in 1987 were exposed to the BIA).

While a quick recovery of all the AMT paid, through AMT credits, was not guaranteed, quick recovery of a portion of the AMT paid occurred quite frequently, especially for large firms. For example, estimates of the proportion of AMT paid in 1987 that had been recovered by 1991 vary between 43.6 percent for all firms (General Accounting Office 1995, 44) and 65.8 percent for large firms (Gerardi et al. 1993, 49). A related piece of evidence is that exposure to AMT was transitory for many firms. For example, among the large firms that paid the AMT in any year within the 1987–91 five-year period, 70.7 percent paid AMT in only one or two years. This evidence is consistent with the view that the cost of the AMT was mitigated substantially by the AMT credit for many firms.

The evidence on AMT firms facing limit positions suggests that a significant portion of firms were subject to the 90 percent NOL limit, but very few firms were faced with the 90 percent FTC limit (General Accounting Office 1995, 49–50). Among all firms that paid AMT in 1987, 36 percent reported an AMT-NOL deduction and 65 percent of these taxpayers claimed the full AMT-NOL deduction, subject to the 90 percent limit. In contrast, only 2 percent of the 1987 AMT payers claimed the AMT-FTC, and only 27 percent of these corporations claimed the full AMT-FTC, subject to the 90 percent limit. While this evidence suggests that the total number of NOL limit firms is less than 25 percent of AMT payers, the evidence in Boynton et al. 1992, which is more representative of large firms, suggests a much higher fraction of limit firms. Of the 85 firms in their sample paying the AMT, 54

firms are limit firms (48 firms are subject to the NOL limit and 6 firms are subject to the FTC limit).

This evidence from tax returns, especially for large firms, suggests that both the costs of paying the AMT and the benefits of reducing 1987 book income implied by the traditional argument are mitigated by other features of the AMT. The costs of paying the AMT were sharply lower for the many firms that were able to quickly recover some of the AMT paid through AMT credits, and the benefits of reducing book income were sharply lower for the significant fraction of limit firms. Evidence indicating that many tax returns reported the BIA suggests that these firms either were unable to or elected not to reduce book income sufficiently to eliminate the BIA.

3. Review of five previous studies on the BIA and earnings management

We summarize first the main features and results of each of the five previous studies on BIA-related earnings management, and then provide our comments and reasons why the results could be biased toward supporting the traditional argument. Generally speaking, these studies have grappled with two difficult issues: identifying firms affected by the BIA and isolating book-only accruals.
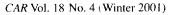
Gramlich 1991

Summary

In the earliest study on the topic, Gramlich ranked 703 COMPUSTAT firms based on the ratio of tax paid to tax expense (TP/TE) during the years 1984 to 1986. Firms in the lowest quartile of this ranking were identified as most likely to be affected by the BIA (the affected group). Firms in the highest TP/TE quartile (the control group) were expected to be the least likely to be affected by the BIA. The dependent variable, discretionary accruals, was measured as the year-to-year change in the difference between pre-tax book income and pre-tax cash flows. His evidence indicated that firms in the affected group made more income-decreasing accruals in 1987 than control firms. Also, he found evidence suggesting that affected firms made income-*increasing* accruals in 1986, before the BIA took effect, presumably to enhance further their ability to reduce book income in 1987.

Comment

Although Gramlich's evidence is consistent with the BIA causing earnings management in the direction predicted by the traditional argument, it is also consistent with the result being driven by his sample selection procedure. Gramlich attempted to identify firms that expected to face the AMT before engaging in earnings management. However, by selecting firms with low TP/TE ratios, his affected sample consisted of firms that reported high book income in 1984–86, relative to taxable income. To the extent that book income is likely to mean-revert in subsequent years (e.g., Ramakrishnan and Thomas 1998), such firms are likely to exhibit decreases in book income in 1987 that are unrelated to the BIA (see related discussion in section 3 below under the heading "Dhaliwal and Wang 1992").

Following Gramlich, we used the ratio of taxes paid (COMPUSTAT item #16 minus the change in item #74) to tax expense (COMPUSTAT item #16), labeled TP/TE, to partition the sample into firms more and less likely to be affected by the BIA.[9] Only firms that report positive pre-tax income (COMPUSTAT items #172 + #16) and positive tax expense are included in the sample. To mitigate transitory effects, we use the three-year sum, from 1984 to 1986, for all of the above variables. The lower (or more negative) the value of TP/TE, the greater the difference between book and tax income due to timing differences in that period, and therefore the greater the likelihood of being affected by the BIA. Hence, firms in the lowest TP/TE quartile are classified in the "affected" category, and all other firms are grouped in the "unaffected" category.[10]

We use the following specification of the Jones 1991 model to measure discretionary accruals in a manner similar to the approach used by Gramlich:

$$\frac{TA_{it}}{A_{it-1}} = a_i \frac{1}{A_{it-1}} + b_{1i} \frac{\Delta REV_{it}}{A_{it-1}} + b_{2i} \frac{PPE_{it}}{A_{it-1}} + u_{i85}D85 + u_{i86}D86 + u_{i87}D87 + u_{i88}D88 + e_{it} \quad (1).$$

The subscripts i and t refer to firm i and year t, ΔREV represents change in revenues (COMPUSTAT item #12), PPE is the gross property, plant, and equipment (COMPUSTAT item #7), A is total assets (COMPUSTAT item #6), and $D85$ through $D88$ are dummy variables that equal 1 for the years 1985 through 1988, respectively, and 0 otherwise.[11] Total accruals (TA) is the change in noncash working capital accounts (COMPUSTAT items (#4 − #1) − #5) minus the expense amount from the funds flow statement for depreciation/depletion/amortization (COMPUSTAT item #125).[12] The four coefficients, u_{i85} to u_{i88}, on the dummy variables provide estimates for the discretionary accruals over the 1985–88 period.

We estimate the Jones 1991 accrual model described in (1) for each firm using up to 13 years of data, from 1972 to 1984 (1971 is not available since the model requires annual *changes* for revenues and current accounts), and discretionary accruals are projected for the last four years, from 1985 to 1988.[13] The results are reported in panel A of Table 2. Consistent with the BIA causing earnings management, firms in the first quartile exhibit significant negative discretionary accruals in 1987, with mean and median values of −1.97 and −0.97 percent of total assets. Significant negative accruals are also observed for affected firms in 1988. However, there is virtually no evidence of positive accruals for affected firms in 1986; the mean is positive while the median is negative, but both are insignificant.

While these results generally are consistent with the traditional argument, there are two sets of subtle biases relating to the sample employed. The first is an overall tendency to observe negative accruals after 1986, which is observed for both affected and unaffected firms. Since the TP/TE sample includes only firms that reported a book profit during 1984–86, and since there is a tendency for income to mean-revert, both groups of firms are likely to exhibit book income decreases in the post-1986 period. A comparison of the affected and unaffected

groups' accruals provides a simple way to examine a general mean-reversion effect. Consistent with the presence of such a bias, unaffected firms also exhibit statistically significant negative accruals in 1987 and 1988. They are, however, not as large as those for the affected group. The bottom row in panel A of Table 2 shows that the accruals for the two groups remain statistically different in 1987, with the mean (median) accruals for affected firms being more negative at the 17 percent (6 percent) level, using two-tailed tests.[14] That is, even after adjusting for the first bias, the results are consistent with the traditional argument. Although our results are not as significant as those reported by Gramlich (probably as a result of differences in the sample and variables used), they are still qualitatively similar, with affected firms making more negative accruals in 1987.

The second bias is the mechanical effect mentioned earlier, which also creates a tendency to generate negative accruals after 1986, but its effect is stronger for the low TP/TE group. Affected firms, with low TP/TE during 1984–86, have large timing differences that revert in subsequent years, and cause affected firms to exhibit more income-decreasing accruals than do unaffected firms. To examine the mechanical effect underlying the second bias, where more negative accruals are generated for the low TP/TE affected firms, we replicate the analysis for a simulated tax event by shifting the tax event back by one year.[15] In the presence of a mechanical effect, "simulated affected" firms should exhibit more negative accruals after 1985 (since the 1983–85 period is used to rank firms into quartiles of TP/TE), relative to "simulated unaffected" firms.

Again, because of the general mean-reversion effect possibly caused by selecting only firms that were profitable in the three years 1983–85, both simulated affected and simulated unaffected firms are likely to report negative accruals after 1985. The results reported in the first and second rows in panel B of Table 2 confirm this prediction: significant negative accruals are observed for both groups of firms after 1985. As in panel A, the simulated affected firms exhibit more negative accruals relative to unaffected firms, although the difference between the two groups is not significant in 1986, the simulation year.

The results of our final comparison (affected versus simulated affected firms) designed to control for the mechanical effect are reported in the last row at the bottom of panel B of Table 2. We find that the negative accruals for affected firms in 1987 (reported in panel A) are not statistically different from the accruals for the simulated affected firms in 1986 (reported in panel B). On the basis of these results, we conclude that significant negative discretionary accruals for the affected firms during 1987 are likely driven by the mechanical effect.

We describe next the results of supplementary analyses. Boynton et al. (1992) examined the ability of different TP/TE measures to partition their sample firms into affected and unaffected firms. They employed two different proxies for this determination: (a) whether firms paid the AMT (ex post), and (b) whether they would have been classified as being affected using their ex ante approach.[16] The TP/TE measure that performs best under the ex ante approach is their measure (3) using a base period ending in 1984 (Boynton et al. 1992, 152, Table 7). It is similar in concept to the TP/TE measure we use above, but is based on an earlier three-

TABLE 2

Discretionary accruals for partitions considered in Gramlich 1991: Quartiles based on the ratio of tax paid to tax expense (TP/TE)

TP/TE is calculated over a three-year period, which varies across panels. Discretionary accruals are estimated based on the firm-specific Jones 1991 model and the pooled Kang and Sivaramakrishnan 1995 IV model. The Jones model is:

$$\frac{TA_{it}}{A_{it-1}} = a_i \frac{1}{A_{it-1}} + b_{1i} \frac{\Delta REV_{it}}{A_{it-1}} + b_{2i} \frac{PPE_{it}}{A_{it-1}} + u_{i85}D85 + u_{i86}D86 + u_{i87}D87 + u_{i88}D88 + e_{it} \quad (1)$$

Panel A: Test sample, based on TP/TE from 1984 to 1986

Discretionary accruals are estimated from the firm-specific Jones 1991 model (as a percentage of total assets), $\hat{u}_{it}$.[*] Each cell contains the mean/median discretionary accrual; p-values associated with the two-sided t-test/sign rank test are shown in parentheses;[†] and n is the sample size.

Quartiles of TP/TE	1985	1986	1987	1988
1 (affected firms)	0.76/−0.12	0.51/−0.17	−1.97/−0.97	−1.35/−0.74
	(0.25)/(0.96)	(0.49)/(0.95)	(0.00)/(0.00)	(0.04)/(0.02)
	$n = 360$	$n = 356$	$n = 334$	$n = 268$
2, 3, and 4 (unaffected firms)	−0.42/−0.01	−0.65/−0.35	−0.79/−0.48	−0.71/−0.18
	(0.26)/(0.73)	(0.13)/(0.18)	(0.06)/(0.07)	(0.12)/(0.05)
	$n = 1,318$	$n = 1,316$	$n = 1,230$	$n = 922$
p-values for tests that groups are similar[‡]	(0.14)/(0.87)	(0.20)/(0.50)	(0.17)/(0.06)	(0.48)/(0.27)

Panel B: Simulated sample, based on TP/TE from 1983 to 1985

Discretionary accruals are estimated from the firm-specific Jones 1991 model (as a percentage of total assets), $\hat{u}_{it}$.[*] Each cell contains the mean/median discretionary accrual; p-values associated with the two-sided t-test/sign rank test are shown in parentheses;[†] and n is the sample size.

Quartiles of TP/TE	1985	1986	1987	1988
1 (simulated affected firms)	0.06/−0.37	−1.84/−0.62	−2.07/−1.51	−1.55/−0.86
	(0.90)/(0.27)	(0.00)/(0.01)	(0.00)/(0.00)	(0.03)/(0.01)
	$n = 390$	$n = 361$	$n = 335$	$n = 267$
2, 3, and 4 (simulated unaffected firms)	−0.49/−0.26	−0.84/−0.54	−1.39/−0.73	−1.32/−0.40
	(0.17)/(0.18)	(0.05)/(0.02)	(0.00)/(0.00)	(0.00)/(0.00)
	$n = 1,456$	$n = 1,352$	$n = 1,275$	$n = 960$
p-values for tests that groups are similar[‡]	(0.45)/(0.90)	(0.25)/(0.29)	(0.42)/(0.11)	(0.80)/(0.40)
p-values for tests of similarity between 1987 accruals for affected firms in test sample and 1986 accruals for simulated affected firms in simulated sample		(0.88)/(0.65)[‡]		

(The table is continued on the next page.)

TABLE 2 (Continued)

Panel C: Test sample, based on TP/TE from 1982 to 1984, "measure 3", as suggested by Boynton et al. 1992 (152)

Discretionary accruals are estimated from the firm-specific Jones 1991 model (as a percentage of total assets), $\hat{u}_{it}$.[*] Each cell contains the mean/median discretionary accrual; p-values associated with the two-sided t-test/sign rank test are shown in parentheses;[†] and n is the sample size.

Quartiles of TP/TE	1985	1986	1987	1988
1 (affected firms)	−3.19/−1.77	1.33/−0.36	1.64/−0.14	1.43/1.12
	(0.00)/(0.00)	(0.27)/(0.87)	(0.17)/(0.84)	(0.25)/(0.18)
	n = 567	n = 517	n = 460	n = 313
2, 3, and 4 (unaffected firms)	−0.93/−0.40	−0.99/−0.62	−1.59/−0.78	−2.11/−0.90
	(0.00)/(0.01)	(0.01)/(0.00)	(0.00)/(0.00)	(0.00)/(0.00)
	n = 1,991	n = 1,842	n = 1,725	n = 1,288

Panel D: Test sample based on TP/TE from 1982 to 1984, "measure 3", as suggested by Boynton et al. 1992 (152)

Discretionary total accruals are estimated from the Kang and Sivaramakrishnan IV model:[§]

$$\frac{CAL_t}{A_{t-1}} = \alpha_0 + \alpha_1 \left(\frac{ART_{t-1}}{REV_{t-1}}\right)\frac{REV_t}{A_{t-1}} + \alpha_2 \left(\frac{OCAL_{t-1}}{EXP_{t-1}}\right)\frac{EXP_t}{A_{t-1}} + \alpha_3 \left(\frac{DEP_{t-1}}{GPPE_{t-1}}\right)\frac{GPPE_t}{A_{t-1}}$$

$$+ \sum_{k=85}^{90} u_k D_k + e_t \quad (2)$$

Coefficient	Affected firms (lowest quartile of TP/TE) adjusted R^2 = 70% (n = 1,219)		Unaffected firms (top 3 quartiles of TP/TE) adjusted R^2 = 70% (n = 12,970)	
	coefficient	t-statistic	coefficient	t-statistic
α_0	−0.005	−0.29	0.015	4.24
α_1	0.961	26.56	0.955	123.32
α_2	0.970	44.55	0.928	154.74
α_3	−0.949	−4.92	−1.206	−26.52
u_{85}	0.008	0.69	0.007	2.32
u_{86}	0.020	1.61	0.003	1.05
u_{87}	−0.014	−1.08	−0.003	−0.93
u_{88}	0.001	0.07	−0.004	−1.25
u_{89}	0.012	0.86	−0.004	−1.34
u_{90}	−0.016	−1.11	−0.007	−2.12

(The table is continued on the next page.)

TABLE 2 (Continued)

Notes:

*   In (1), $\hat{u}_{it}$ is the coefficient on D_t estimated over 1972–88, and TA is total accruals (Δ#4 − Δ#1 − Δ#5 − #125), ΔREV is change in revenues (#12), PPE is gross property, plant, and equipment (#7), A is total assets (#6), and $D_t = 1$ in year t (between 1985 and 1988) and 0 otherwise. (COMPUSTAT data items are shown in parentheses and Δ indicates the current-year value minus the prior-year value for the variable.) The subscripts i and t refer to firm i and year t.

†   Our statistical tests assume that discretionary accruals are independent across firm-years. To the extent that this assumption is violated, standard errors may be understated and statistical significance may be overstated.

‡   The p-values are based on two-sided t-tests and rank sum tests for comparisons of the two respective groups.

§   In (2), CAL represents the level of current assets less liabilities [(#4 − #1) − #161 − (#5 − #71) − #14)], A is total assets (#6), REV is revenues (#12), ART is accounts receivable less taxes receivable (#2 − #161), $OCAL$ represents other current assets less liabilities [#4 − #2 − #1 − (#5 − #71)], EXP represents expenses (#12 − #13), DEP is depreciation expense (#14), and PPE is gross property, plant, and equipment (#7). (COMPUSTAT data items are shown in parentheses.) Lagged values of these three regressors are used as instrumental variables. The dummy variables D_{85} through D_{90} equal 1 for the years 1985 through 1990, respectively, and 0 otherwise. The six coefficients, u_{85} to u_{90}, on the dummy variables provide estimates for the discretionary accruals over each year in that period.

year period (1982–84), and two different adjustments are made. First, there is no requirement that book income and tax expense be positive; second, TP/TE is assumed to be equal to zero for cases where it is zero or missing. Not only does the Boynton et al. TP/TE measure (3) result in a different partition, it also increases the sample size. The results of using this measure are reported in panel C of Table 2. As shown, the affected firms exhibit no evidence of negative Jones 1991 model accruals during 1987 or 1988.[17] This result suggests that the two biases observed in panels A and B are driven largely by our TP/TE measure. Curiously, the unaffected firms exhibit negative accruals in all four years. Apparently, expanding the sample size results in average Jones model accruals that are negative in each year.

Our final analysis of Gramlich's sample is based on a more recent approach to detecting earnings management developed by Kang and Sivaramakrishnan 1995, as modified by Kang 1999 (herein referred to as "K-S" or "the K-S model") to mitigate some of the problems associated with the Jones 1991 model. K-S replace changes in working capital with the level of working capital, use slightly different regressors, and employ either the instrumental variables (IV) or the generalized method of moments (GMM) approach. Also, rather than estimating firm-specific

parameters using longer time-series data, the K-S model uses more recent data and estimates the same model for all sample firms. A formal description of their model follows.

$$\frac{CAL_t}{A_{t-1}} = a_0 + a_1\left(\frac{ART_{t-1}}{REV_{t-1}}\right)\frac{REV_t}{A_{t-1}} + a_2\left(\frac{OCAL_{t-1}}{EXP_{t-1}}\right)\frac{EXP_t}{A_{t-1}}$$

$$+ a_3\left(\frac{DEP_{t-1}}{GPPE_{t-1}}\right)\frac{GPPE_t}{A_{t-1}} + \sum_{k=85}^{90} D_k u_k + e_t \quad (2).$$

The subscript t refers to year t (firm subscripts are dropped on the coefficients since it is a pooled model), CAL represents the level of current assets less current liabilities and depreciation (COMPUSTAT items (#4 − #1) − #161 − (#5 − #71) − #14), A is total assets (COMPUSTAT item #6), REV is revenues (COMPUSTAT item #12), ART is accounts receivable less taxes receivable (COMPUSTAT items #2 − #161), $OCAL$ represents other current assets less liabilities (COMPUSTAT items #4 − #2 − #1 − (#5 − #71)), EXP represents expenses (COMPUSTAT items #12 − #13), DEP is depreciation expense (COMPUSTAT item #14), and $GPPE$ is gross property, plant, and equipment (COMPUSTAT item #7). (Data items #161 and #71 are set to zero if they are missing.) The dummy variables D_{85} through D_{90} equal 1 for the years 1985 through 1990, respectively, and 0 otherwise. The six coefficients, u_{85} to u_{90}, on the dummy variables provide estimates for the discretionary accruals over each year in that period. We selected the IV approach, since K-S find that it gives results similar to the GMM approach and is easier to implement, and used lagged values of the three regressors as instrumental variables.

The sample is separated into affected and unaffected firms using the TP/TE measure (3) suggested by Boynton et al. 1992 and the results are reported in panel D of Table 2. As in panel C, the results show no evidence of systematic attempts by affected firms to reduce reported book income in 1987, 1988, or 1989. As with the analysis in panel C, we do not conduct additional tests using simulated affected and unaffected firms, since the coefficient in 1987 (u_{87}) for affected firms (−0.014) is not significantly different from that for unaffected firms (−0.003), based on an examination of the two t-statistics. When these K-S model accruals are combined with our TP/TE partitions, instead of the TP/TE partitions suggested by the Boynton et al. measure (3), only a weak mechanical effect remains: negative but insignificant discretionary accruals are observed for affected firms in the three-year BIA period (results available upon request).

We also consider the year 1990 in panel D of Table 2 because it offers a different way to control for the level of discretionary accruals that affected firms would have made absent the BIA.[18] For years after 1989, the BIA was replaced by the ACE adjustment and no earnings management in response to the BIA is expected. The discretionary accruals associated with affected firms in 1990 serve as a benchmark for discretionary accruals estimated for affected firms over the prior three years when the BIA was applicable. Comparison of the coefficient for u_{90} for

affected firms with those for u_{87}–u_{89} offers no evidence suggesting that affected firms made more negative accruals in the 1987–89 period, relative to those in 1990, to mitigate the effect of the BIA.

In sum, the TP/TE measure proposed by Gramlich to partition the sample into affected and unaffected firms creates a mechanical effect consistent with the traditional argument: affected firms appear to make negative accruals in the years immediately after 1986 (especially when firm-specific Jones 1991 model estimates of discretionary accruals are used).[19] This evidence of negative accruals disappears when the mechanical effect is controlled and the improved measures of TP/TE suggested by Boynton et al. 1992 are considered. Even though our analysis focuses on earnings management, and considers only the potential bias due to specific ways of partitioning samples, these results highlight the general importance of using dummy events to gauge the extent to which experimental procedures create spurious results.

Boynton et al. 1992

Summary

Boynton et al. used an industry-based variant of the Jones 1991 model of discretionary accruals to estimate a discretionary accrual proxy (*DAP*) for a sample of manufacturing and transportation firms.[20] They then accessed a proprietary Internal Revenue Service (IRS) tax return file to remove *DAP* from the book income amount reported on the AMT form (Form 4626) by 387 of these firms and recalculated the AMT based on the "pre-AMT" book income (i.e., the firm's position after reversing the net estimated discretionary accruals).[21] This innovative approach allowed them to identify not only firms that reported the BIA on their tax returns, but also firms that would have been affected by the BIA but managed earnings so that the BIA was no longer operative.

Boynton et al. partitioned their sample into 8 groups of firms (partitions 0 through 7) based on whether they paid the AMT, whether they were exposed to the AMT prior to making discretionary accruals, and whether their discretionary accruals were positive or negative ($2 \times 2 \times 2$). Each partition was further subdivided into two groups, based on whether they were limit firms (see the discussion in section 2 above, under the heading "The traditional argument"). AMT-NOL and AMT-FTC firms are labeled "L" firms, and the AMT-ITC firms are combined with the full-AMT firms and labeled "U" firms. Recall that limit (L) firms have the lowest incentives to reduce book income in 1987 since they face only a 1 percent immediate marginal tax rate on book income shifts.

Boynton et al. used the statistical comparisons across different partitions reported in their Table 4 as the basis for their discussion of the results. We find it more useful to examine the underlying *DAP* associated with each partition, reported in their Table 3.[22] Those results are summarized in our Table 3. Boynton et al. recognized the potential for error caused by focusing only on the 44 firms in partition 5 (all of which are in partition U5 since none of them are limit firms). Firms in this partition did not pay AMT, but would have been exposed to the AMT

if the negative estimated *DAP* had been removed from the amount reported as pre-tax book income. To mitigate this potential problem, Boynton et al. expanded the set of affected firms to include other partitions, and contrasted these groups of affected firms with various combinations of unaffected or less-affected firms. However, given the considerable number of firms in partition 5 and the large negative mean *DAP* of -11.21 percent (of total assets) for this partition, the mean *DAP* for these various sets of affected firms is influenced heavily by the inclusion of partition 5. Note that mean *DAP* as high as -11.21 percent implies considerable earnings management, since that level of *DAP* exceeds twice the normal level of after-tax earnings (the median ratio of after-tax earnings to total assets is approximately 5 percent for our sample). These comparisons suggest that except for the very largest firms (23 firms with assets greater than \$10 billion), "U" firms exhibit significant earnings management in 1987.[23]

Comment

Before discussing potential biases in the Boynton et al. study, we emphasize that we do not conduct any original analysis relating to this study (because we do not have access to the proprietary tax return data they examined); we simply provide alternative interpretations of their results. Since their measure of *DAP* does not isolate book-only accruals, efforts to shift taxable income that involve the collateral shifting of book income would also be included in *DAP*. The overall decline in regular tax rates from 1986 to 1987 creates incentives for all taxpayers to shift taxable income from 1986 to 1987 and, as predicted by the new argument, AMT firms face even greater incentives to make these shifts because of the larger decline in immediate tax rates. Therefore, *DAP* represents the net effect of efforts by BIA-affected firms to make book-only income-decreasing accruals in 1987 and efforts to shift taxable (and book) income in the opposite direction. As discussed below, there is clear evidence that *DAP* reflects the incentives created by the specific tax rate declines. The evidence regarding the former effect (book-only income-decreasing accruals) is mixed, however, and is discussed next.

A potential problem with Boynton et al.'s research design is that *DAP* was used both for determining which firms were exposed to the BIA (the independent variable) and for measuring firms' discretionary accrual responses to the BIA (the dependent variable).[24] Although this approach seems unusual, it raises no concerns, provided that *DAP* measures without error the discretionary accruals made by firms seeking to avoid the BIA effect. If, however, firms make discretionary accruals for reasons other than the BIA, or if discretionary accruals are measured with error, a bias is created in the research design in favor of finding a relation consistent with the traditional argument.

Unfortunately, neither condition is unlikely. As described in section 2, firms are likely to make accruals for both nontax and tax reasons, unrelated to the BIA. Further, Kang and Sivaramakrishnan (1995) and Gramlich (1992) discuss the sources of potential measurement error in the Jones 1991 model accruals used to estimate *DAP*.[25] To illustrate the nature of the experimental bias in favor of the traditional argument, consider firms that made no discretionary accruals in 1987 in

TABLE 3

Discretionary accruals for partitions of manufacturing and transportation firms based on AMT exposure (derived from Table 3 of Boynton et al. 1992)

Mean discretionary accruals (DAP), from industry-level Jones 1991 model estimates for the 387 firms in their sample filing Form 4626 in 1987, are reported below. Firms are first partitioned into 8 groups (0 through 7) based on whether or not they paid AMT in 1987, whether or not they would have been exposed to AMT if the estimated DAP had not been made, and the sign of DAP (positive or negative). Each partition is split again based on whether or not firms were in limit status (L and U). Limit firms had low incentives to manage book earnings to avoid the AMT, because of NOL carryovers and FTC credits. The following example describes how to interpret the table: the partition 0 consists of firms that did not pay the AMT in 1987, were not exposed to the AMT before making those discretionary accruals, and made positive accruals. The two subgroups U0 and L0 contain 136 and 10 firms, respectively, and mean discretionary accruals are equal to 6.35 percent (of total assets) for both subgroups combined.

	No AMT paid in 1987*				AMT paid in 1987†			
	Positive DAP		Negative DAP		Positive DAP		Negative DAP	
	Not in limit	In limit	Not in limit	In limit	Not in limit	In limit	Not in limit	In limit
Not exposed to AMT	Partition U0	Partition L0	Partition U1	Partition L1	Partition U2	Partition L2	Partition U3	Partition L3
	136 firms	10 firms	111 firms	1 firm	10 firms	5 firms	None	None
	DAP = 6.35%		DAP = −4.78%		DAP = 10.63%	DAP = 7.38%	—	—
Exposed to AMT	Partition U4	Partition L4	Partition U5	Partition L5	Partition U6	Partition L6	Partition U7	Partition L7
	None	None	44 firms	None	5 firms	27 firms	16 firms	22 firms
	—	—	DAP = −11.21%	—	DAP = 2.89%	DAP = 8.06%	DAP = −6.47%	DAP = −7.03%

Notes:

Mean DAP for all firms above = 0.04%.

*   Mean DAP for all firms not paying AMT = −0.34%.

†   Mean DAP for all firms paying AMT = 1.38%.

response to the BIA. Applying the Boynton et al. model, however, these firms would be estimated to have made either negative or positive *DAP* in response to the BIA. For firms erroneously estimated to have made a negative *DAP*, the imputed pre-AMT book income is larger than the reported AMT book income. This error increases the excess of book income over taxable income and increases the likelihood that this firm will be identified as being exposed to the BIA. Boynton et al.'s approach would incorrectly include these firms in their partition 5.

Of the firms in the right half of Table 2 that paid the AMT in 1987 (their partitions 2, 6, and 7), 15 firms are categorized as not having been exposed to the BIA prior to making positive accruals (partition 2). These firms would not have paid the AMT if they had not made those positive accruals. While some firms may choose to pay the AMT rather than decrease book income in response to the BIA (for reasons discussed in section 2, under the heading "Review of reasons why managers might not reduce book income, despite the tax savings"), it is very unlikely that firms would intentionally undertake positive discretionary accruals to increase their AMT liability. Thus, we view the existence of firms in partition 2 as prima facie evidence that *DAP* measures with error the extent to which earnings were managed in response to the BIA. Moreover, if there are 15 firms in their sample that were misclassified as not being exposed to the BIA, but paid the AMT because they made positive accruals, it is reasonable to assume that there are 15 other firms that are misclassified as being exposed to the AMT, but successfully avoided the AMT by making negative accruals. That is, we estimate that about one-third of the 44 firms in partition 5 that are responsible for Boynton et al.'s conclusion were erroneously classified as making negative accruals in response to the AMT. Such misclassification creates a spurious relation between firm partitions (the independent variable) and firm responses (the dependent variable).

Other ways to identify affected firms that are not contaminated by the use of *DAP* do not provide evidence in support of the traditional argument. For example, the traditional argument would predict a negative mean *DAP* for the overall sample, provided that affected (unaffected) firms made negative (zero) *DAP* on average. Again, the results in Boynton et al.'s Table 3 indicate a slightly positive mean *DAP* overall (0.04 percent). Moreover, the 85 firms that paid the AMT should exhibit evidence of negative *DAP* in 1987 under the traditional argument. In fact, these firms had an average *positive DAP* of 1.38 percent for 1987.[26]

While these results do not support the traditional argument, they are consistent with the new argument. First, the positive mean *DAP* observed for the 85 firms paying the AMT is expected if AMT-affected firms shifted taxable (and book) income into 1987 to benefit from the decline in tax rates. Second, finding that a large proportion of the 85 were in limit positions (54 in the L2, L6, and L7 partitions) is also consistent with our argument that many affected firms have low incentives to make negative book-only accruals in response to the BIA because they face lower immediate marginal tax rates. Note that even the remaining non-limit firms (U2, U6, and U7) that had potentially greater incentives to decrease book income in 1987 exhibited positive accruals on average. This result is also consistent with our view that any book-only income-decreasing accruals made are

unlikely to be detected by the *DAP* measure because it would be swamped by the incentives to shift taxable (and book) income in the opposite direction.

To summarize, although Boynton et al. found evidence in their partition 5 results that supports the traditional argument, there is a caveat: their proxy for discretionary accruals likely measures accruals in response to the BIA with error, such that some firms that appeared to make negative accruals are misclassified. Singling out firms with large negative accruals biases the results in favor of the view that affected firms made negative accruals in response to the BIA. Given the lack of confirming evidence elsewhere in the sample and our reinterpretation of their results in support of the new argument, we believe the evidence of negative accruals observed for groups of affected firms that include partition 5 is not conclusive. Our investigation of the Boynton et al. study demonstrates how random error in a measure of discretionary accruals can create spurious results in earnings management research when both the independent and dependent variables are derived from that measure.

Dhaliwal and Wang 1992

Summary

Dhaliwal and Wang examined a sample of COMPUSTAT firms that reported positive book income from 1985 to 1987. They expected firms with 1986 effective tax rates (*ETR*) below 23 percent to be affected by the AMT (the test group), and firms with *ETR* greater than 23 percent to be unaffected (the control group), where *ETR* equals the ratio of taxes paid to pre-tax book income.[27] Discretionary accruals were measured by *CTMD*, the year-to-year change in the ratio of timing and permanent differences to pre-tax book income. Timing and permanent differences represent the difference between pre-tax book income and taxable income (see note * of Table 4 for details of how different variables are computed). This measure of earnings management, unlike the measures used in the previous two papers, has the desirable property of not being affected by efforts to manage both book and taxable income.

Dhaliwal and Wang found that *CTMD* exhibits a tendency to mean-revert, and that this tendency is related to the partitioning variable, *ETR*.[28] The mean-reversion effect is controlled by regressing *CTMD* on *ETR* in 1985 (a nonevent year) and using the estimated intercept and slope to compute adjusted *CTMD* measures for 1986 and 1987. On the basis of this adjusted *CTMD*, the test group exhibits a significant negative response for 1987, and a significant positive response for 1986. These results are consistent with the traditional argument because they imply that affected firms made accruals designed to increase (decrease) book income in 1986 (1987).

Comment

The main results of Dhaliwal and Wang are reported in panel A of Table 4. With considerable assistance from the authors, we were able to replicate to a substantial extent the sample, data analysis, and results (details available upon request). The

corresponding results for our sample are reported in panel B. Barring some exceptions (e.g., the control firms' adjusted response for 1987), the two panels are fairly similar. Importantly, the affected firms continue to exhibit significant changes in timing and permanent differences in the directions predicted by Dhaliwal and Wang, while the control firms do not.

Further analysis revealed that the Dhaliwal and Wang results are sensitive to the use of book income as a scaling variable when measuring $CTMD$, the response variable. While book income is a logical measure to use when scaling timing and permanent differences, mean book income (scaled by total assets) for affected firms decreased in 1986 and increased in 1987. We believe this change in book income caused the scaled $CTMD$ measure to increase in 1986 and decrease in 1987. That is, the significant results consistent with the traditional argument could be driven by coincidental changes in book income, the scaling variable, rather than by changes in permanent and timing differences, the variable of interest.

The literature provides some guidance on the choice of deflators and the question of spurious results caused by deflators (e.g., Lev and Sunder 1979; Barth and Kallapur 1996). In essence, the deflator selected should change proportionately with the variable of interest, absent any incentives for managerial intervention. Timing and permanent differences, the variable of interest in Dhaliwal and Wang, are due to various differences in book and tax accounting that arise from a number of sources. Some of these differences relate to assets (such as the difference between book and tax depreciation), while others relate to revenues (such as differences between book and tax methods of revenue recognition). Thus, total assets and total sales are also reasonable candidates for deflating timing and permanent differences to control for variation in size across firms and over time. Although the relation derived by Dhaliwal and Wang was cast in terms of book income serving as a deflator, that relation can be logically recast with either total assets or sales as the deflator.[29]

Panels C and D of Table 4 provide the results obtained when the deflators are total assets and total sales, respectively. Note that the magnitudes of the means as well as the estimated slopes and intercepts in panels C and D are not comparable with those in panels A and B, because total assets and total sales are much greater than book income. However, the t-statistics and p-values are comparable across panels. The impact of changing the deflator is quite dramatic: the significant results observed in panels A and B for affected firms' adjusted $CTMD$ are no longer observed in panels C and D, and some mean values are of the wrong sign, relative to the traditional argument. In both panels C and D, the unadjusted $CTMD$ values are also not significant in the hypothesized directions. Note the lower t-statistics reported for the slopes and intercepts in panels C and D for the affected group, relative to those in panel B, indicating a decline in the extent of mean reversion when the deflator is total assets or total sales.[30]

To provide additional insights into the contradictory results observed in Table 4, we examined variation in book income, timing and permanent differences, total assets, and total sales (results available upon request). Our analysis indicates no evidence that the results in panels A and B are caused by changes in timing and

TABLE 4

Analysis of Dhaliwal and Wang 1992 results: Changes in timing and permanent differences (*CTMD*) around the Tax Reform Act of 1986

Panel A: Dhaliwal and Wang 1992 results, with *CTMD* deflated by book income*

	Affected firms ($ETR_{86} < 23$ percent)		Control firms ($ETR_{86} > 23$ percent)	
	1986	1987	1986	1987
Mean *CTMD*	0.121	−0.300	−0.235	0.074
t-statistic	3.52	−6.20	−4.06	1.16
(*p*-value)	(0.00)	(0.00)	(0.00)	(0.24)
Mean adj. *CTMD*	0.103	−0.219	−0.058	−0.016
t-statistic	3.31	−4.58	−1.30	−0.31
(*p*-value)	(0.00)	(0.00)	(0.19)	(0.75)
Intercept and slope for adjustments	$\hat{\alpha} = 0.045$, $\hat{\beta} = -0.81$ $t(\hat{\alpha}) = $ not available, $t(\hat{\beta}) = -2.7$		$\hat{\alpha} = 0.732$, $\hat{\beta} = -2.26$ $t(\hat{\alpha}) = $ not available, $t(\hat{\beta}) = -11.8$	

Panel B: Replication of Dhaliwal and Wang 1992 results, with *CTMD* deflated by book income*

	Affected firms ($ETR_{86} < 23$ percent)		Control firms ($ETR_{86} > 23$ percent)	
	1986	1987	1986	1987
Mean *CTMD*	0.172	−0.221	−0.215	0.082
t-statistic	3.34	−6.42	−3.76	1.31
(*p*-value)	(0.00)	(0.00)	(0.00)	(0.19)
Mean adj. *CTMD*	0.131	−0.209	−0.034	−0.051
t-statistic	2.56	−5.48	−0.87	−0.57
(*p*-value)	(0.01)	(0.00)	(0.39)	(0.56)
Intercept and slope for adjustments	$\hat{\alpha} = 0.049$, $\hat{\beta} = -0.32$ $t(\hat{\alpha}) = 1.79$, $t(\hat{\beta}) = -1.20$		$\hat{\alpha} = 0.737$, $\hat{\beta} = -2.24$ $t(\hat{\alpha}) = 9.93$, $t(\hat{\beta}) = -11.69$	

Panel C: Replication of Dhaliwal and Wang 1992 results, with *CTMD* deflated by total assets*

	Affected firms ($ETR_{86} < 23$ percent)		Control firms ($ETR_{86} > 23$ percent)	
	1986	1987	1986	1987
Mean *CTMD*	0.0022	−0.0040	−0.0185	0.0012
t-statistic	0.43	−0.95	−3.77	0.27
(*p*-value)	(0.66)	(0.34)	(0.00)	(0.78)
Mean adj. *CTMD*	−0.0037	0.0016	−0.0001	−0.0134
t-statistic	−0.72	0.38	−0.02	−1.31
(*p*-value)	(0.47)	(0.78)	(0.98)	(0.19)
Intercept and slope for adjustment	$\hat{\alpha} = 0.0061$, $\hat{\beta} = -0.0036$ $t(\hat{\alpha}) = 1.08$, $t(\hat{\beta}) = -0.07$		$\hat{\alpha} = 0.0546$, $\hat{\beta} = -0.1780$ $t(\hat{\alpha}) = 4.85$, $t(\hat{\beta}) = -6.13$	

(The table is continued on the next page.)

TABLE 4 (Continued)

Panel D: Replication of Dhaliwal and Wang 1992 results, with *CTMD* deflated by total sales[*]

	Affected firms ($ETR_{86} < 23$ percent)		Control firms ($ETR_{86} > 23$ percent)	
	1986	1987	1986	1987
Mean *CTMD*	0.0097	0.0026	−0.0100	−0.0006
t-statistic	1.72	0.36	−3.11	−0.22
(*p*-value)	(0.08)	(0.71)	(0.00)	(0.81)
Mean adj. *CTMD*	0.0035	0.0105	−0.0008	−0.0074
t-statistic	0.62	1.43	−0.25	−1.16
(*p*-value)	(0.53)	(0.15)	(0.79)	(0.24)
Intercept and slope for adjustments	$\hat{\alpha} = 0.057,\ \hat{\beta} = 0.0176$ $t(\hat{\alpha}) = 0.81,\ t(\hat{\beta}) = 0.26$		$\hat{\alpha} = 0.00366,\ \hat{\beta} = -0.1116$ $t(\hat{\alpha}) = 5.67,\ t(\hat{\beta}) = -6.72$	

Notes:

The samples utilize data drawn from December year-end COMPUSTAT firms, excluding
firms in the SIC 4xxx and 6xxx industry classifications. The number of firms in each
cell varies between 150 and 200 based on available data. As in Dhaliwal and Wang
1992, firms were also deleted based on the presence of unconsolidated subsidiaries,
which could create a potential difference between the book income reported in
financial statements and that used for computation of the book income adjustment.
(COMPUSTAT data items are shown in parentheses.)

[*]   *CTMD* is defined as the change in permanent and timing differences, scaled by book
income, total assets, or total sales, as indicated. Permanent and timing differences
equal the difference between pre-tax book income (sum of #18, #16, #49, and
#173) and taxable income (sum of #63 and #51, divided by the tax rate, equal to
46 percent). *ETR* is the effective tax rate, measured by the ratio of tax paid (#63)
to pre-tax book income. The intercept and slope ($\hat{\alpha}$) and ($\hat{\beta}$) are estimated from a
regression of *CTMD* on *ETR* for 1985. The adjusted *CTMD* values for 1986 and
1987 are then derived as follows:

$$\text{adjusted } CTMD_{i86} = CTMD_{i86} - (\hat{\alpha} + \hat{\beta} ETR_{i86}) \text{ and adjusted } CTMD_{i87}$$

$$= CTMD_{i87} + (\hat{\alpha} + \hat{\beta} ETR_{i87})$$

permanent differences, or that the results in panels C and D are caused by unusual
changes in total assets and total sales.[31] Thus, it appears that the results in panels A
and B are influenced by an unusual increase in book income that occurred between
1986 and 1987 for affected firms. Such an increase is not predicted by the tradi-
tional argument, but an increase in book income, to the extent that it also reflects
taxable income, is predicted by the new argument. Of course, economic factors
unrelated to the new argument may also be responsible for the observed patterns in

book income. Regardless of why book income changed for affected firms, the relevant finding is that the result is not observed with other deflators.

Dhaliwal and Wang conducted two additional sensitivity analyses that relate to the choice of deflator. In their footnote 15 they reported that their results remained *unchanged* when they replaced the book income deflator with total assets. As reported in Table 4, our investigation does not support that finding, since the results change substantially when the book income deflator is replaced by total assets. Second, they reported in their Tables 6 and 7 that their results changed substantially when they scaled timing and permanent differences by total assets (*TPDA*) and estimated firm-specific Jones 1991 model regressions over prior estimation periods to control for contemporaneous levels of property, plant, and equipment (*PP&E*) and changes in sales. They ascribed this change in results to the controls for sales changes and *PP&E*, rather than to the use of total assets as a deflator. Their overall conclusion that affected firms manipulated timing and permanent differences discounts this contradictory result because they argued that the fit of the Jones model is mediocre and structural changes are likely to have occurred during the estimation period as well as between the estimation period and the test period.

In summary, for the sample of firms identified by Dhaliwal and Wang as affected by the AMT, our evidence shows that the deflator, book income, declined on average in 1986 and increased in 1987. This increase appears unrelated to changes in firm size as measured by either total assets or total sales. Although inconsistent with the traditional argument that affected firms would reduce book income in 1987, this increase in book income is consistent with the new argument that affected firms would shift taxable income from 1986 to 1987 to benefit from a steep decline in immediate marginal tax rates. Examining alternative deflators and time-series variation in book income and timing and permanent differences (the variable of interest) suggests that the Dhaliwal and Wang results may have been caused by unforeseen changes in the deflator they used. These results highlight the importance of investigating the extent to which the observed results are sensitive to the scaling variable used.

Manzon 1992

Summary

Manzon (1992) identified 151 firms on the National Automated Accounting Retrieval System (NAARS) data base that disclosed in footnotes to their 1987 financial statements that they were subject to the AMT. He categorized these firms into three groups based on their estimated immediate marginal tax rates for book income: (a) equal to 7.5 percent (AMT-ITC firms) or 10 percent (full-AMT firms), (b) equal to 1 percent (AMT-NOL and FTC firms), and (c) equal to 0 percent (firms with book income less than tentative minimum taxable income). He then examined the expense for depreciation, depletion, and amortization (hereafter referred to as "amortization") to compare the differential extent of earnings management across these groups. Manzon's measure of earnings management, like Dhaliwal and Wang's, focuses on efforts to influence book income without affect-

ing taxable income — precisely the kind of accrual predicted by the traditional argument. Manzon concluded that firms with the highest immediate marginal tax rates (7.5 and 10 percent), and thus the greatest incentives to decrease book earnings in response to the BIA, reported greater relative amortization in 1987 than did firms with lower marginal tax rates (1 and 0 percent).

Comment

Although we were able to replicate much of Manzon's results, we have reservations about their interpretation. A representative set of results from the paper is reported in panel A of Table 5. Manzon expected all AMT firms to increase amortization (decrease book income) during 1987 and predicted that this increase would be greater for firms with higher immediate marginal tax rates. In fact, the results indicate that all groups of AMT firms *decreased* amortization in 1987 on average, thereby *increasing* book income in that year. Manzon focused, however, on the comparative results: firms with higher immediate marginal tax rates (those in the 7.5 percent and 10 percent groups) decreased amortization by a smaller amount (−0.07 percent of total assets) than did firms with lower immediate marginal tax rates (−1.59 and −0.94 percent for firms with marginal rates of 1 and 0 percent). In essence, his conclusion hinges on two presumptions: (a) that amortization for AMT payers would have decreased in 1987 in the absence of the BIA, and (b) that the firms with low immediate marginal tax rates provide the appropriate control against which the "most likely" high marginal rate firms should be compared.

   We have three concerns about the link between Manzon's results and conclusion. First, we believe that the level of amortization, unlike other items of revenue and expense, should be fairly stationary over time and is unlikely to exhibit a general decrease in 1987. In unreported analyses (available upon request), we examined the economy-wide distribution of amortization for all firms on COMPUSTAT, and found no evidence of shifts in any year during the period 1985–88.

   Second, we are concerned that the low marginal tax rate group may not be an appropriate control group, because the financial ratios for those firms exhibit unusual patterns in the years leading up to the tax event. As shown in panel B of Table 5, they exhibit much lower growth (often negative) in total assets and sales, relative to high tax rate firms, between 1982 and 1986. In 1987, however, there is a sudden change and they appear quite similar to high tax rate firms along these dimensions. Similar patterns are observed for profitability measures (results available upon request). These time-series shifts may complicate comparisons of treatment and control firms. Specifically, Manzon adjusted reported amortization levels to control for the level of prior year's depreciation and acquisitions of property, plant, and equipment, and estimated this regression jointly on treatment and control firms (see the estimated relation in Table 5). Given the differences we observe in patterns for growth and profitability for the two groups of firms, it may be appropriate to use separate models to adjust reported amortization levels (details of empirical analysis available upon request).

   Finally, we believe that managing amortization is an awkward way to alter book income for BIA purposes. To manage earnings using amortization, firms

TABLE 5

Analysis of the results from Table 5 of Manzon 1992

Panel A: Unexpected amortization*

	Immediate marginal tax rate		
	7.5% and 10%	1%	0%
Likelihood of responding to BIA by managing book income	Most likely to respond to the BIA	Less likely to respond to the BIA	Unlikely to respond to the BIA
Number of firms in group	58	24	69
Unexpected $\left(\dfrac{amort_{87}}{asset_{87}}\right)$	−0.07%	−1.59%	−0.94%
Unexpected $\left(\dfrac{amort_{86}}{asset_{86}}\right)$	−0.77%	0.22%	0.57%

Panel B: Time series of annual changes in total assets and sales for affected and control firms

Variable analyzed	Immediate marginal tax rate	1983	1984	1985	1986	1987	1988
Median change in total assets	7.5 & 10%	14.89%	12.08%	11.20%	5.41%	7.07%	7.88%
	0 & 1%	3.10%	−2.05%	−2.29%	−1.32%	4.16%	6.49%
Median change in sales	7.5 & 10%	10.79%	11.33%	4.85%	1.90%	12.39%	11.67%
	0 & 1%	2.20%	5.27%	−5.45%	0.99%	10.35%	10.19%

Notes:

The relation used to estimate unexpected amortization is obtained from Table 3 of Manzon 1992:

$$\text{Unexpected}\left(\frac{amort_t}{asset_t}\right) = \left(\frac{amort_t}{asset_t}\right) - \left[0.01643 + 0.76339\left(\frac{amort_{t-1}}{asset_{t-1}}\right) - 0.00495\,ACQ_t\right]$$

The variables above are defined as follows (with COMPUSTAT data items shown in parentheses):

$amort_t$ = depreciation, depletion, and amortization from income statement in year t (#14),

$assets_t$ = total assets at end of year t (#6), and

ACQ_t = net acquisitions of long-lived assets in year t (#128), scaled by total assets in year t.

*   The signs of the unexpected amortization values in the bottom two rows are reversed relative to those in Table 5 of Manzon 1992. Manzon defines unexpected amortization as the difference between expected amortization and actual amortization. We use the more conventional definition of unexpected amortization: the difference between actual and expected amortization.

would need to change amortization methods, estimates, or asset group classifica-
tions in order to achieve the increased expense in 1987, and then three years later
(when the BIA is no longer binding) return to the level of depreciation that was
optimal prior to the BIA. Our examination of the 1987 annual report footnotes of
firms in Manzon's sample revealed that few firms disclosed changes in method or
estimates. Of firms that did disclose such changes, fewer than half resulted in
increased amortization. Overall, there is no evidence in the footnotes suggesting that
the high tax rate group made systematic changes designed to increase amortization.

Unlike the typical case, where firms might not wish to reveal that earnings
were managed, here there are clear incentives for firms to highlight the fact that the
income reductions were designed to reduce taxes and are not symptoms of a
decrease in profitability. Specifically, stock market participants might react nega-
tively to the earnings decline if the amortization increase is perceived as a permanent
effect because it is buried in Cost of Goods Sold and/or Selling, General, and
Administrative expenses.[32] (See Weiss 1999 for a discussion of these incentives
and supporting evidence.) Given this background, the absence of disclosures indi-
cating changes in amortization raises concerns about Manzon's interpretation of
his results.

Note that Manzon identified affected firms based on self-reported admissions
of having paid the AMT, unlike the other papers examined here, which used an ex
ante or expected likelihood of being exposed to the AMT. As argued by Manzon
and Plesko 1996, this ex post procedure could create biases that *reduce* the likeli-
hood of observing income-decreasing accruals in 1987. Specifically, some affected
firms might be able to avoid the AMT altogether and would appear as unaffected
firms using the ex post measure, and some firms paying the AMT might not dis-
close this position in their annual reports.[33]

Overall, we are less confident than Manzon that the smaller amortization
decrease observed for AMT payers with high immediate marginal tax rates, rela-
tive to AMT payers with low immediate marginal tax rates, should be interpreted
as evidence that these firms *increased* amortization to decrease book income in
1987 in response to the BIA. Potential difficulties associated with adjusting
reported earnings through changes in amortization, in combination with the
absence of evidence that amortization methods and estimates were changed to
increase amortization, raise concerns about this interpretation. Also, we are con-
cerned that the amortization changes for the low tax rate AMT firms, which are
crucial to the relative comparison underlying his results, may not represent the
appropriate control. More generally, the implications of our analysis are that con-
trol samples deserve careful scrutiny, because the comparative results observed
could be driven by unexpected control sample characteristics.

Wang 1994

Summary
Wang (1994) identified 56 firms from the NAARS data base (excluding financial
firms and utilities) that disclosed both positive adjusted book income and deferred

tax items relating to reserves and allowances in their 1987 financial statement tax footnotes. Deferred tax items relating to reserves and allowances were divided by the statutory tax rate to calculate the timing differences. The dependent variable, changes in accounting estimates (*CHGACC*), was measured by timing differences related to changes in accounting estimates, deflated by adjusted book income. Adjusted book income was measured by adding back to net book income the effects of total federal and foreign taxes, discontinued operations, extraordinary items, and the cumulative effect of changes in accounting method. Wang showed that *CHGACC* is significantly negative (positive) in 1987 (1986); those results are reported in panel A of Table 6. He repeated the same analysis for control firms matched on the basis of industry and size, and found no such patterns in *CHGACC*.

Comment

We attempted to construct an identical sample of affected firms, to determine whether Wang's results were sensitive to book income, the chosen deflator. This effort was motivated by our findings relating to the Dhaliwal and Wang paper. We followed the sample selection steps (1) to (4) described in Wang but were unable to replicate closely Wang's sample and results (contrast panels A and B of Table 6).[34] However, the results for our sample are in the same direction as (albeit less significant than) those reported by Wang. We then repeated the analysis using two deflators other than book income: sales and total assets.[35] The results of this analysis, reported in panels C and D of Table 6, are not consistent with the earlier evidence that income-decreasing accruals were made in 1987: the timing differences are now positive, though insignificant. This finding suggests that Wang's results could be sensitive to the choice of deflator. We should emphasize, however, that there is less basis for this concern, relative to our concerns regarding the Dhaliwal and Wang study, since we were not able to replicate the results of the original study as closely.

Summary

In each of the five studies, we show why the results might be influenced by unintentional biases in the experimental procedures that cause the reported results to support the traditional argument. While some of the biases are less likely to be observed than others, they are all examples of biases that could affect earnings management research, and many other empirical studies as well, especially when the results are consistent with strong prior beliefs held by the researcher. We offer possible checks that such studies should routinely consider to confirm that their results are robust.

Regarding the question of earnings management in response to the BIA, we believe this paper provides sufficient reason to reopen that discussion. Beyond our reexaminations reported above, we are also influenced by our repeated inability to find clear evidence of earnings management in response to the BIA in a number of other analyses that we conducted on different samples using different measures of discretionary accruals and different ways of identifying affected firms.[36] If, as the previous studies suggest, many firms were affected by the BIA and they undertook

TABLE 6

Replication of Wang 1994 study (57 NAARS firms, excluding non-December year-ends and SIC industry groups 4xxx and 6xxx)

Panel A provides the results reported in Table 5 of Wang 1994, obtained for Wang's sample of firms affected by the BIA. The variable analyzed in Wang's paper is the timing difference (between taxable and book income) relating to various reserves and allowances (causing deferred taxes) reported in the tax footnotes, scaled by adjusted book income. *Timing differences* are computed as the deferred tax amounts divided by the appropriate statutory tax rate for that firm-year. *Book income* (adjusted) is pre-tax book income before discontinued operations and extraordinary items. *Timing differences* are proxies for the changes in accruals undertaken by affected firms to minimize the impact of the BIA. They are computed for 1986 and 1987 for a sample of 56 NAARS firms disclosing AMT exposure.

Panel B provides the results of our attempts to replicate the Wang study; panels C and D provide the results of analyzing the same variable with two alternative scaling variables: *Sales* and *Total assets*.

Statistic	A Timing differences Book income		B Timing differences Book income		C Timing differences Sales		D Timing differences Total assets	
	1986	1987	1986	1987	1986	1987	1986	1987
Median	0.129	−0.217	0	−0.007	0	0.002	0	0.003
Mean	0.253	−0.432	0.363	−0.431	0.003	0.002	0.006	0.001
Std. deviation	0.89	0.95	2.02	1.54	0.03	0.02	0.03	0.03
t-statistic	2.05	−3.28	1.35	−2.12	0.67	0.68	1.84	0.20
p-value	0.05	0.01	0.18	0.04	0.5	0.5	0.07	0.83
n	56		57		57		57	

Notes:

Variables are defined as follows (COMPUSTAT data items are shown in parentheses):

Book income = income before extraordinary items (#18) + tax expense (#16) − state taxes (#173),
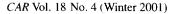
Sales = total sales (#12), and

Total assets = total assets (#6).

substantial amounts of earnings management in response, it is puzzling that such evidence is not robust to alternative research designs.

4. Conclusion

The book income adjustment was viewed as creating a strong and direct tax-based incentive for a substantial number of firms to make income-decreasing accruals in 1987. We believe the strength of this traditional argument caused researchers to accept confirming evidence more easily than disconfirming evidence.

Our examination of the logical underpinnings of the traditional argument uncovers reasons why the incentives to manage earnings may have been overstated

in prior research. Also, we offer a new argument derived from incentives for affected firms to increase taxable (and book) income to take advantage of a precipitous decline in tax rates that occurred in 1987. Not only is it harder to detect evidence of negative book-only accruals for these firms, but some firms are no longer exposed to the BIA if sufficient taxable (and book) income is shifted into 1987.

Our examination of empirical issues for the five prior studies offers the following findings that pertain to the design and implementation of earnings management studies. The Gramlich 1991 study alerted us to mechanical relationships in the data that are caused by the selection of the independent variable. The Boynton et al. 1992 study highlighted the potential bias that can result when discretionary accruals, measured with error, are used in the construction of both the partitioning (independent) variable and the response (dependent) variable. Our analysis of the Dhaliwal and Wang 1992 and Wang 1994 studies suggests that the choice of scaling variable can influence the results, and the Manzon 1992 research highlights the need to check whether the results are sensitive to the choice of control groups. While our investigation has focused on earnings management studies, the issues we consider are also likely to be relevant in other genres of empirical research.

The overall lack of substantial evidence in support of BIA-induced earnings management can be interpreted in a number of ways. It is possible that the AMT savings were not large enough to encourage managers to make adjustments to book income. It is also possible that the various nontax costs mentioned were high enough to deter earnings management, or affected firms found other ways to mitigate the AMT impact. A very different possible explanation for our lack of significant results is the low power associated with extant techniques to detect earnings management. Overall, our analysis indicates that it remains premature to conclude that the BIA caused affected firms to make income-decreasing accruals in 1987.

Endnotes

1. De Long and Bradford-Lang (1992) find that unrejected null hypotheses are very infrequent in economics journal papers except in cases where the null hypotheses are patently false. They suspect that this is because journals are less likely to publish "no-results" papers that fail to reject the null hypothesis. As a result, authors consciously or unconsciously mine data by considering alternative experimental procedures, and the potential for bias occurs when the choices made are based on the results they produce. De Long and Bradford-Lang conclude: "hypothesis tests should concentrate on implications that are robust to minor changes in specification" (1272).

2. Summary statements excerpted from the conclusions of each of these papers follow: "The results are consistent with firms adjusting accruals to minimize their tax bills" (Gramlich 1991, 54); "The results indicate that firms managed depreciation accruals to minimize the AMT cost" (Manzon 1992, 89); "The results indicate that firms that are likely to be affected by the AMT manipulated timing and permanent differences in 1986 and 1987 in response to the book income adjustment provision" (Dhaliwal and Wang 1992, 26); "We find evidence that firms potentially exposed to the AMT undertook income-reducing accruals in 1987 when compared to firms not so exposed" (Boynton et al. 1992, 148); "The analyses in general indicate that the AMT firms

changed accounting practices in response to the book income adjustment" (Wang 1994, 506). A subset of affected firms, consisting of "limit" firms with considerably lower incentives to manage earnings, do not exhibit negative accruals in 1987 and is used as a control group in some studies.

3. We thank an anonymous referee for pointing out this implication.

4. During the tax years 1987 through 1989, only AMT arising from certain deferral adjustments could generate AMT credit; permanent or "exclusion" preferences were not eligible to create AMT credits (section 53(d)(1)(B)(i)). For this purpose, the only exclusion preferences that did not generate AMT credit were tax-exempt interest and excess percentage depletion. (See title VII, part 7, section 464, of the General Explanation of the Tax Reform Act of 1986, P.L. no. 99-514, 99th Congress.) As a deferral adjustment, all AMT related to the BIA created AMT credit.

5. Han and Wang (1996) examine the accrual behavior of oil firms during the 1990 Persian Gulf War to document evidence that political costs can influence reported accruals.

6. Rather than reduce reported earnings with discretionary accruals, it is also possible that managers could undertake a host of alternative responses that mitigate the AMT. They could alter their financing and investment decisions to reduce the effect of the preferences and adjustments that are added back to taxable income when determining the AMT. For example, they could reduce investment in tax-preferred items such as oil and gas developments and lease rather than buy depreciable assets (e.g., Lyon 1997; Kramer, McDonald, and Kramer 1993). Other possible responses include merging with firms that have appropriate tax attributes, and altering ownership in subsidiaries selectively above (below) the 80 percent level at which tax returns are consolidated to include (exclude) negative (positive) book incomes.

7. The intuition for this result is as follows. Although the dollar amount of the BIA (the difference between book and taxable income) remains unchanged when both book and taxable income are shifted, the importance of the BIA declines, relative to the level of taxable income. (The BIA in columns 8 and 11 of Table 1 are equal, but the taxable income in column 8 is less than that in column 11.)

8. Other unreported analyses, which consider features such as the impact of limits on the use of investment credits and NOL deductions for AMT purposes, are available upon request from the authors.

9. We were unable to replicate exactly the results in Gramlich 1991 because we could not access the CD-ROM COMPUSTAT edition he used to generate his sample. We were, however, able to replicate the general tenor of his results and the mechanical effect using our sample of COMPUSTAT firms and his variables.

10. We elected to combine the second, third, and fourth quartiles in the unaffected group, rather than use just the fourth quartile (as in Gramlich 1991), because we found that the fourth quartile exhibited accruals that were in between the first quartile and the second and third quartiles. That is, including the second and third quartiles in the unaffected group increases the likelihood of rejecting the null hypothesis. Another difference between our TP/TE measure and Gramlich's is that he required positive pre-tax income in each year between 1984 and 1986, whereas we require that the three-year sum be positive. We also require that the three-year sum of tax expense, the denominator in

TP/TE, be positive. As a sensitivity test, we computed the statistics using Gramlich's partitioning variable TP/TE and the results obtained are similar to those presented in Table 2.

11. Jones (1991) discusses the assumptions underlying the model and the trade-off between considering a more complex model and losing observations due to missing data. This is a two-stage approach: it fits (1) without the dummy variables over an estimation period (1972–84) and then computes prediction errors for each year in the four-year period between 1985 and 1988. Using dummy variables produces results identical to computing prediction errors, and yet offers several benefits, including ease of interpretation and a convenient way to compute standardized prediction errors (see Thompson 1985).

12. In a minor departure from the Jones 1991 model, we use the depreciation/depletion/amortization amount from the funds flow statement instead of the income statement, because it is more comprehensive. The two amortization measures are, however, empirically very close to each other and our results are not affected by this choice.

13. To increase the reliability of the estimated parameters, we deleted firms with fewer than 10 years of nonmissing data between 1972 and 1984. Note that the models used to estimate discretionary accruals typically cause certain industries to be underrepresented.

14. Although we report two-tailed α-levels for all tests to maintain consistency with the prior literature, these α-levels should be halved (the differences would become more significant), since one-tailed tests are more appropriate in this case.

15. Similar mechanical effects were observed for two other simulated tax events: two years prior to the actual event (TP/TE based on 1982–84), and one year after the actual event (TP/TE based on 1985–87).

16. Under the heading "Boynton et al. 1992" in section 3, we show the potential for misclassification based on their ex ante approach. While this potential for misclassification could affect the validity of their suggested TP/TE measure, we believe that it is reasonable to consider their suggested TP/TE measure in sensitivity analyses, since their ability to examine tax returns provides a valuable assessment of alternative TP/TE measures.

17. Since the 1987 mean and median accruals for affected firms are *greater* than those for unaffected firms, we do not examine the significance of this difference, nor do we examine whether this difference is significantly different from that for simulated affected and unaffected firms.

18. We thank an anonymous referee for this suggestion.

19. Note that the mean-reversion effect we describe here relates to discretionary accruals from the Jones 1991 model, which differs from the mean-reversion effect that Dhaliwal and Wang (1992) observe in their measure of book-only accruals. Also, the dummy event approach that we use to control for mean reversion is different from the approach that Dhaliwal and Wang employ, which requires the estimation of a regression of book-only accruals on effective tax rates over a nonevent period.

20. That is, rather than estimate separate Jones 1991 models for each firm using a long time series, they pool five years of data for all firms in each industry group and estimate a single model for each industry. Their approach removes the need for a

sufficiently long time series of data for all sample firms, but it suppresses within-industry variation. In concept, their industry-specific model lies in between the firm-specific Jones 1991 model and the economy-wide K-S model.

21. While Boynton et al.'s sample consisted of 414 firms, we ignore the 27 firms in their partitions 8 and 9 for which AMT exposure could not be obtained because these firms did not file Form 4626 with their 1987 tax returns.

22. Since *DAP* removes the nondiscretionary component of accruals, mean *DAP* values for each partition provide stand-alone measures of earnings management; in other words, *DAP* can be compared to zero and therefore no comparisons with control groups are necessary. This assumption is the basis for Boynton et al.'s approach to estimate ex ante exposure to the BIA by removing *DAP* from the book income amount reported on Form 4626. We are puzzled by the assertion in Dhaliwal 2001 that Boynton et al.'s approach of fitting the Jones 1991 models over a prior estimation period using a larger set of firms than their final sample precludes comparisons of *DAP* with zero. The Jones model should be estimated over a nonevent period, and over as many firms as possible in each industry (given Boynton et al.'s maintained assumption that industry-level Jones model estimates are superior to firm-level estimates).

23. Boynton et al. (1992) reported in their Table 5 that the differences between the 9 affected firms (partitions 2, 5, 6, and 7) and the 14 unaffected firms were not significant for the largest group. This lack of significance could, however, have been driven by the small sample sizes and the very few firms from partition 5 in the affected group.

24. Gramlich (1992) discusses this and other features of the Boynton et al. 1992 study.

25. One indication of measurement error in *DAP* is that in 21 of the 36 industry models described in Table 1 of Boynton et al. 1992, the coefficient on the variable gross property, plant, and equipment is *positive*, suggesting that greater investment in property, plant, and equipment leads to *lower* depreciation charges.

26. The average accruals for different combinations of partitions are a weighted average of the component partition averages. They are obtained by multiplying the averages for each partition by the number of firms in that partition and then dividing the total by the total number of firms in the combination of partitions.

27. To get the 23 percent upper bound for average tax rates for affected firms, Dhaliwal and Wang considered the case when adjustments and preferences in 1987 equal the difference between book and taxable income (and BIA = 0). The point at which the regular tax equals the tentative minimum tax is when 40 percent of taxable income equals 20 percent of [taxable income plus (book income minus taxable income)]. Rearranging terms, Dhaliwal and Wang got taxable income = 0.5*book income. This relation implies that the *ETR* equals 20 percent in 1987 when the regular tax rate was 40 percent, which implies an *ETR* of 23 percent in 1986 when the regular tax was 46 percent.

28. This is the finding that has implications for the discussion above under the heading "Gramlich 1991" regarding the mean-reversion effect that we claim contaminates the Gramlich results.

29. Under the simplifying assumptions of the Dhaliwal and Wang model, reducing the 1987 ratio of timing and permanent differences to book income by making negative book-only accruals reduces exposure to the BIA. If the 1986 level of this ratio is a

reasonable proxy for the 1987 level of the ratio before earnings management, the change in the ratio from 1986 to 1987 measures the attempts to reduce exposure to the BIA. While violations of the simplifying assumptions and/or problems with using the 1986 ratio as a proxy for the unmanaged 1987 ratio might reduce the usefulness of the change in the ratio as a response variable, the change in timing and permanent differences remains a useful measure of book-only accruals. That is the empirical context in which we suggest that Dhaliwal and Wang were effectively using book income as a deflator to control for changes in scale between 1986 and 1987. If so, it is reasonable to examine whether the result is robust to alternative deflators, especially given the incentives for affected firms to shift taxable (and book) income from 1986 to 1987.

30. That is, adjusting for mean reversion, using procedures similar to the Dhaliwal and Wang approach of first estimating the relation between $CTMD$ and ETR in a nonevent year, is less important when scaling variables other than book income are used.

31. For example, while the median ratio of pre-tax book income to total assets for affected firms increased from 8.1 percent in 1986 to 9.2 percent in 1987, it remained relatively unchanged for control firms (13.7 percent in 1986 and 14.0 percent in 1987). The median percent change in book income from 1986 to 1987 for affected firms is 26.2 percent, whereas it is 15.0 percent for control firms. In contrast, the median percent change in total assets from 1986 to 1987 is 11.7 percent for affected firms and 11.6 percent for control firms.

32. The stock market is less likely to react negatively if firms highlight the cosmetic nature of the income decrease (caused by a more aggressive amortization policy) without admitting explicitly that it was designed to minimize the AMT.

33. Note the difference between identifying firms paying the AMT using tax return data, as in Boynton et al. 1992, and using annual report disclosures, as in Manzon 1992. The former approach is likely to be more comprehensive than the latter because it is not likely that all firms that pay the AMT will indicate this in their annual report.

34. Wang was unable to identify the names of firms in his sample, because some of the documentation relating to his paper was misplaced during a move he undertook subsequent to publication of his paper.

35. As discussed under the heading "Dhaliwal and Wang 1992", we examined percent changes in the different deflators, and again uncovered a similar, but less significant, effect: there was an unusual book income increase during 1987 for the affected sample, whereas there was no unusual increase observed for total assets or sales.

36. For example, a simple nonparametric approach to detect whether the BIA caused earnings management, without actually identifying affected firms, is to examine the shape of the distribution of various measures of discretionary accruals. If a substantial fraction of firms (say 5 or 10 percent) made income-decreasing (income-increasing) accruals in 1987 (1986), the tails of the overall distribution would be skewed to the left (right) in 1987 (1986). The results of an examination of five measures of accruals indicate no evidence of such a shift.

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