Modeling the Determinants of Meet-or-Beat Behavior in Distribution Discontinuity Tests

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Abstract
We develop new tests of distribution discontinuity conditional on multiple explanatory variables, which can be used to analyze meet-or-beat behavior around benchmarks. These tests combine Burgstahler and Dichev’s (1997) intuition on benchmark-driven earnings management with a flexible statistical model that addresses important limitations of the existing distribution discontinuity tests. Our conditional discontinuity method offers large improvements in test performance relative to both histogram-based tests of the existence of distribution discontinuity and logit-based tests of the determinants of distribution discontinuity, and it changes some of the major findings in the earnings discontinuity literature. Our method is robust and easy to implement in standard statistical software. Future research in many fields could benefit by adopting our distribution discontinuity tests.

Keywords: standardized difference test; performance benchmark; bright-line rule; smooth distribution; nonlinear interpolation; conditional distribution

JEL codes: M41, C20, C25

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1. Introduction

We propose new conditional distribution discontinuity tests for analyzing meet-or-beat behavior around a performance benchmark. Our tests embed Burgstahler and Dichev’s (1997) earnings distribution discontinuity logic in a flexible statistical model that can incorporate multiple explanatory variables. Our conditional discontinuity method addresses important limitations of current logit-based discontinuity tests, offers large improvements in Type-I error and statistical power, and changes major findings in the earnings discontinuity literature. Researchers can easily implement our tests with just one line of code using a custom Stata estimation command that we developed and made publicly available at http://astro.temple.edu/~dbyzalov/.

For expositional ease, we analyze the familiar context of earnings management. If some managers manipulate earnings to avoid reporting a loss, then the earnings distribution is discontinuous at the zero benchmark, with unusually few small losses and unusually many small profits. If some managers avoid earnings decreases, then a similar discontinuity arises for earnings changes. Burgstahler and Dichev (1997) document both discontinuities for U.S. public firms. Their earnings management tests rely on less restrictive assumptions than tests of abnormal accruals (e.g., Jones, 1991) and real activities management (e.g., Roychowdhury, 2006), thus yielding more credible inferences.1 The distribution discontinuity approach is widely used to detect earnings management (e.g., Degeorge et al., 1999; Matsumoto, 2002; Leuz et al., 2003; Barth et al., 2008), analyze properties of the “suspect” observations just above the zero earnings benchmark (e.g., Roychowdhury, 2006; Zang, 2012), and study manipulation of other metrics such as marathon running times (Allen et al., 2017) and reported tumor sizes (Samoylova et al., 2017).

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1 Durtschi and Easton (2005, 2009) and Beaver et al. (2007) argue that these earnings distribution discontinuities might not reflect earnings management, but Jorgensen et al. (2014), Burgstahler and Chuk (2015, 2017), and others refute their arguments.
Burgstahler and Dichev’s (1997) test is based on the empirical histogram of earnings and cannot easily incorporate multiple explanatory variables. To study the determinants of distribution discontinuity, researchers typically switch from histogram-based tests to a logit model for a meet-or-just-beat dummy (e.g., Frankel et al., 2002; Matsumoto, 2002; Cheng and Warfield, 2005). However, estimation for the meet-or-just-beat dummy (in logit or any similar model) yields faulty inferences about the determinants of meet-or-just-beat behavior. For example, if a variable $X$ affects the variance of pre-managed earnings, then the probability of unmanaged small profits varies with $X$. The small-profit dummy (i.e., the meet-or-just-beat dummy for the zero earnings benchmark) includes both managed and unmanaged small profits. Therefore, the small-profit probability varies with $X$, even if $X$ does not affect earnings management behavior. Thus, a researcher cannot infer the determinants of meet-or-just-beat behavior by only analyzing the meet-or-just-beat probability.

To overcome this identification problem, we use information on the shape of the earnings distribution, following Burgstahler and Dichev’s (1997) fundamental intuition, and we let the distribution vary with multiple explanatory variables $X$, using a flexible statistical model illustrated in Figure 1. We assume a smooth distribution of pre-managed earnings and a discontinuous effect of earnings management just below and above the zero benchmark (Panels A and B of Figure 1), where both the smooth component and the discontinuous component are conditioned on $X$. We use a flexible polynomial approximation to avoid restrictive functional form assumptions but require that all managed small losses are reported as small profits (Panel C). The smoothness assumption

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2 A researcher can compare histograms between two subsamples for a single explanatory variable (e.g., R&D-intensive versus non-R&D-intensive firms in Burgstahler and Chuk, 2017). However, to implement the test with multiple explanatory variables, one would need subsamples that capture the joint variation in these variables. This approach quickly becomes impractical. For example, if a sample is partitioned at the median of five explanatory variables, then the total number of subsamples is $2^5 = 32$. The tests will have low power due to small subsample size, and it will be difficult to draw any conclusions from 32 histograms.
conditional on $X$ identifies the pre-managed earnings distribution, while the missing small losses and the excess small profits conditional on $X$ identify earnings management behavior (Panel D). Distribution discontinuity is assessed using standard statistical tests for regression coefficients. We develop a simple two-stage estimation approach that uses ordinary least squares (OLS) in each stage, and package it as a custom Stata estimation command, which we used to generate all the two-stage estimates and graphs in the paper.

First, we validate our distribution discontinuity method in basic tests without explanatory variables. Our method successfully detects discontinuity for scaled earnings in U.S. Compustat data and is robust. In tests for 101 one-cent thresholds from $0$ to $1$ for EPS data, it reliably detects discontinuities at meet-or-just-beat benchmarks at multiples of 10 cents per share and reliably detects the absence of discontinuity at the remaining pseudo-benchmarks. In a simulated horse race against standard Burgstahler and Dichev (1997) tests, our method improves statistical power because it combines data just below and above zero into one estimate of earnings discontinuity (Panel C of Figure 1). Thus, our method can benefit researchers even in a basic test without explanatory variables, especially if the sample size and/or effect size are small.

Next, we assess our method’s performance in its primary use scenario, i.e., a distribution discontinuity test with explanatory variables. We simulate an explanatory variable $X$ that can affect both the pre-managed earnings distribution and the probability of earnings management (e.g., R&D intensive firms might have both more volatile pre-managed earnings and more frequent earnings management). The former effect defines the economic null hypothesis of no earnings management (Ball, 2013), and only the latter effect should be attributed to the alternative hypothesis of earnings management. We simulate earnings conditional on $X$ and estimate the effect of $X$ on earnings discontinuity using both the standard logit approach and our method. As expected,
the logit model for the small-profit dummy has excessive Type-I errors of 17.6–75.7% because it cannot separate the effect of \( X \) on the pre-managed earnings distribution from the effect of \( X \) on the earnings management probability (Panel D of Figure 1). In contrast, our method reliably separates the two effects, yielding both valid Type-I errors and greater statistical power than logit. In additional tests for earnings discontinuity determinants in U.S. Compustat data, the standard logit estimates contradict the theory, whereas our method performs as expected and is robust. Thus, in tests of earnings management determinants, our conditional discontinuity method offers a critical improvement over the standard logit approach.

Use of our method changes major prior findings. Using a generalized logit model for meeting or beating analyst forecasts, Barton and Simko (2002) find that firms with higher beginning-of-period net operating assets (NOA) are less likely to manage earnings, reflecting the role of the balance sheet as an earnings management constraint. However, this finding is an artifact of the logit model. When we control for the association between NOA and the pre-managed earnings distribution using our conditional discontinuity method, the effect of NOA on earnings discontinuity weakens considerably and becomes insignificant. Thus, the prior findings reflect variation in the pre-managed distribution rather than an actual earnings management effect.

Our method generalizes to other distribution discontinuity scenarios where a decision-maker has both an opportunity and an incentive to move from just below to just above a benchmark or vice versa, including Medicare reimbursements (Barnes and Harp, 2018), labor tax avoidance (Chetty et al., 2011), marathon running times (Allen et al., 2017), and reported tumor sizes at the eligibility thresholds for the liver transplant waiting list (Samoylova et al., 2017).

We develop the conditional discontinuity method in Section 2, present the main results in Section 3, re-visit prior findings in Section 4, and conclude in Section 5.
2. Statistical model of conditional earnings discontinuity at the zero benchmark

2.1. Main model components

We model the familiar context of earnings management to avoid reporting small losses, but the analysis generalizes to other distribution discontinuities. The main model components are summarized in Figure 1. Let $EARN^*$ be “pre-managed” earnings (Panel A of Figure 1), which are known to managers but unobservable to researchers, scaled as needed (e.g., on a per-share basis or divided by the lagged market value of equity). Pre-managed earnings follow a smooth distribution that can vary with explanatory variables $X = X_1 \ldots X_M$ (e.g., firm characteristics that affect the mean or variance of $EARN^*$). Formally,

$$EARN^* \sim f^*(EARN^*|X) \quad (1)$$

where $f^*(\cdot)$ is the probability density function. We omit the firm and year indexes for brevity.

Let $EARN$ be reported earnings, which is pre-managed earnings $EARN^*$ plus any earnings management to convert small losses into small profits. Panel B of Figure 1 illustrates the $EARN$ distribution conditional on $X$. When a firm has a pre-managed small loss in the interval $[-K^-, 0)$ just below zero, managers decide whether to manage earnings upwards to above zero. They can report either unmanaged earnings

$$\textit{unmanaged } EARN = EARN^* \quad (2a)$$

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3 For example, if earnings levels are replaced with earnings changes or consensus analyst forecast errors, then the same model describes earnings management to avoid earnings decreases (Burgstahler and Dichev, 1997) or to meet or beat analyst forecasts (DeGeorge et al., 1999), respectively. Discontinuities in debt covenant slack ratios (Dichev and Skinner, 2002), insurance loss reserve ratios (Gaver and Paterson, 2004), working capital ratios (Dyreng et al., 2017), reported hedge fund monthly returns (Bollen and Pool, 2009), Medicare reimbursements (Barnes and Harp, 2018), baseball batting averages (Pope and Winer, 1997), labor income (Chetty et al., 2011; Kleven and Waseem, 2013), size-contingent audit and disclosure requirements (Gao et al., 2009; Kausar et al., 2016; Bernard et al., 2018), marathon running times (Allen et al., 2017), reported statistical significance levels (Brodeur et al., 2016; Basu and Park, 2016), and perceived retail prices (e.g., Ginzberg, 1936; Stiving and Winer, 1997) are amenable to similar analysis. To apply the model to a given metric (e.g., reported liver tumor size around the 2 cm threshold for a patient’s inclusion on the liver transplant waiting list in Samoylova et al., 2017), the dependent variable should be defined as the deviation from the relevant benchmark, and it should be scaled appropriately (Burgstahler and Chuk, 2015).
or a managed small profit $EARN$, drawn from the conditional distribution

$$\text{managed } EARN \sim g(EARN|EARN^*, X)$$

in a narrow interval $[0, K^+]$ just above zero.\(^4\) This earnings management process causes a discontinuity at zero in the distribution of reported earnings $EARN$ (Burgstahler and Dichev, 1997). When pre-managed earnings are outside the small-loss interval, they are reported “as is”.\(^5\)

Managers choose whether to manage earnings based on the expected costs and benefits of reporting a small profit instead of a small loss. From a researcher’s perspective, this decision is probabilistic (Panel C of Figure 1). We assume that the earnings management probability can vary with both the size of the small loss $EARN^*$ (e.g., it is easier to conceal a loss of $1,000$ than $1,000,000$) and explanatory variables $X$ (e.g., earnings-based bonuses can increase the incentive to manage earnings). Formally,

$$\text{earnings management probability} = P(EARN^*, X)$$

for $EARN^*$ in the small-loss interval $[-K^-, 0)$. This probability is the main metric in many earnings management research studies. This research primarily focuses on how earnings management is affected by various economic and institutional factors, such as investor protection regulations (Leuz et al., 2003), accounting standards (Barth et al., 2008), reporting incentives (Healy, 1985; Burgstahler et al., 2006), and auditor incentives (Frankel et al., 2002; Ashbaugh et al., 2003). A researcher can include any such factors in the $X$ vector in our model.

For any $X$, the total mass of managed small profits (the shaded area to the right of zero in Panel

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\(^4\) In general, the width of the small-loss interval $K^-$ can differ from the width of the small-profit interval $K^+$. The former is affected by managers’ ability and incentives to convert pre-managed small losses of different magnitudes into small profits. The latter is affected by managers’ incentives to report an incrementally larger small profit and the precision with which they can manage earnings to the desired benchmark.

\(^5\) Other forms of earnings management such as earnings smoothing (e.g., Ronen and Sadan, 1981; Subramanyam, 1996; Leuz et al., 2003; Sivakumar and Waymire, 2003) or “big bath” (Healy, 1985) do not cause a discontinuity at zero earnings. We treat them as a component of pre-managed earnings and focus on earnings management at the zero benchmark.
C) must equal the total mass of missing small losses (the shaded area to the left of zero), because each managed small profit was originally an unreported pre-managed small loss. This logical restriction combines the information on small losses and small profits, yielding more efficient estimates of the earnings management process. In contrast, standard histogram-based tests separately compute a left standardized difference for small losses and a right standardized difference for small profits and do not combine them into a more efficient single estimate.

Degeorge et al. (1999) and Burgstahler and Eames (2003) present stylized theoretical models of earnings management that resemble our model (1)–(3), but they use these models only to motivate Burgstahler and Dichev (1997) histogram-based standardized difference tests. While these tests can detect an unconditional earnings discontinuity in a given sample, they cannot incorporate multiple determinants of earnings management, i.e., explanatory variables that affect the conditional earnings discontinuity.\(^6\) To analyze the determinants of conditional distribution discontinuity, researchers typically incorporate them as explanatory variables in a logit model for meeting or just beating an earnings benchmark (e.g., Frankel et al., 2002; Matsumoto, 2002; Cheng and Warfield, 2005; and many others). However, this logit model only captures the reduced-form probability of all small profits and cannot separate variation in the earnings discontinuity from variation in the smooth pre-managed earnings distribution (Panel D of Figure 1). This severely distorts inferences about the determinants of earnings discontinuity, as we show in Section 3.3. Thus, the logit approach should not be used for this analysis.

To overcome these limitations of current methods, we directly estimate model (1)–(3) as

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\(^6\) A researcher can use standard histogram-based tests in discrete partitions based on a single explanatory variable (e.g., Burgstahler and Chuk, 2017). If there is a large, statistically significant discontinuity in partition A but not in partition B, then one can draw informal inferences about the effect of the partitioning variable. However, if the discontinuity is statistically significant in both partitions, then it is difficult to tell whether the partitioning variable has an effect (e.g., Burgstahler and Dichev, 1997, p. 111). More important, this single-variable approach cannot measure the incremental effect of a new variable \(X_1\) after controlling for known determinants \(X_2 \ldots X_M\).
described next. The estimates of the conditional earnings management probability \( P(EARN^*, X) \) in (3) measure the impact of multiple explanatory variables \( X \) on the earnings discontinuity and let us test hypotheses about the determinants of earnings management.

2.2. Predicted distribution of reported earnings in the model

To estimate model (1)–(3), we fit the predicted conditional earnings distribution \( f(EARN|X) \) from the model to the data. The predicted earnings distribution is summarized in Figure 2, and the technical details are relegated to Online Supplement A.

Large losses below \(-K^-\) and large profits above \(K^+\) are reported “as is”, and these earnings values cannot be the result of earnings management at the zero benchmark. Therefore, reported earnings in these intervals follow the pre-managed distribution. Formally,

\[
f(EARN|X) = f^*(EARN|X) \quad \text{for } EARN < -K^- \text{ and } EARN \geq K^+
\]

where \( f(EARN|X) \) is the probability density function of reported earnings, and \( f^*(EARN|X) \) is the probability density function of pre-managed earnings (1), evaluated at \( EARN^* = EARN \).

Small losses in the interval \([-K^-, 0)\) are reported only when (a) there is a pre-managed small loss \( EARN^* = EARN \), and (b) this loss has not been managed upward. The distribution of reported small losses \( EARN \) is based on the intersection of these two conditions. Formally,

\[
f(EARN|X) = f^*(EARN|X) \times [1 - P(EARN, X)] \quad \text{for } EARN \in [-K^-, 0)
\]

where \( P(EARN, X) \) is the earnings management probability (3), evaluated at \( EARN^* = EARN \).

Small reported profits in the interval \([0, K^+)\) can arise in two ways. First, the firm could have a pre-managed small profit \( EARN^* = EARN \), reported “as is”. The associated density is \( f^*(EARN|X) \). Second, the firm could have a pre-managed small loss \( EARN^* \in [-K^-, 0) \) and convert it into the observed small profit \( EARN \) through earnings management. The distribution of reported earnings reflects the total of these two paths to a small profit. Formally,
\[
 f(EARN|X) = f^*(EARN|X) + G(EARN, X) \text{ for } EARN \in [0, K^+)
\]  

(4c)

where \(G(EARN, X)\) is the incremental density of managed small profits (i.e., the distance between the density of reported small profits and the density of pre-managed small profits in Figure 2).  

2.3. Empirical specification

We consider two specifications for the conditional earnings management probability \(P(EARN^*, X)\) and the incremental probability density function of managed small profits \(G(EARN, X)\). In Model I, \(P(EARN^*, X)\) and \(G(EARN, X)\) can vary with \(X\), but they are flat (for a given \(X\)) throughout the entire small-loss and small-profit intervals, as illustrated in Panel A of Figure 3. The earnings distribution is shifted downward equally for all small losses and is shifted upward equally for all small profits, where the size of the shifts is conditional on \(X\). This specification is motivated by the standard histogram-based tests, which treat the entire small-loss or small-profit interval uniformly as a single bin.

In Model II, \(P(EARN^*, X)\) and \(G(EARN, X)\) vary with \(X\), similar to Model I, and they also vary with the size of the small loss or profit, as illustrated in Panel B of Figure 3. The earnings management probability \(P(EARN^*, X)\) is largest when pre-managed loss \(EARN^*\) is just below zero, and it decreases with the size of the loss. This decrease captures the difficulty of concealing a larger loss through earnings management (e.g., Barton and Simko, 2002). Similarly, the density of managed small profits \(G(EARN, X)\) is largest when the managed profit \(EARN\) is just above zero, and it decreases with the size of the profit. This decrease reflects the difficulty of reporting a

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7 Formally, \(G(EARN, X) = \int_{-K}^{0} g(EARN|EARN^*, X)P(EARN^*, X)f^*(EARN^*|X)dEARN^*, \) where the first two terms represent the conditional density of the observed small profit \(EARN\) for a given value of \(EARN^*\), and the integral represents the expectation with respect to the unobserved random variable \(EARN^*\). This expression embeds the intuitive constraint from Panel C of Figure 1 that conditional on \(X\), the total mass of managed small profits must equal the total mass of missing small losses. Instead of repeatedly computing the integral during estimation, we directly specify the function \(G(EARN, X)\) as described in Section 2.4.
larger profit through earnings management and the smaller incremental benefit of beating the target by a large margin rather than meeting or just beating it.

Formally, the earnings management probability in the small-loss interval $[-K^-, 0)$ is

**Model I**

$$P(EARN^*, X) = \pi_0 + \pi_1X_1 + \cdots + \pi_MX_M \quad (5a)$$

**Model II**

$$P(EARN^*, X) = q(EARN^*) \times (\pi_0 + \pi_1X_1 + \cdots + \pi_MX_M) \quad (5b)$$

where $\pi_0 ... \pi_M$ are estimation parameters, and $q(EARN^*) = 2(1 - |EARN^*|/K^-)$ in Model II is a triangular interaction term that increases linearly from 0 at the lower bound $EARN^* = -K^-$ to 2 at the upper bound $EARN^* = 0$, such that it equals 1 on average in the small-loss interval.\(^8\)

The incremental density of managed small profits $G(EARN, X)$ in Models I and II does not incorporate any additional estimation parameters. It is computed through the restriction that all small losses that disappear due to earnings management must re-appear as small profits, as illustrated by the two shaded areas in Panel C of Figure 1.\(^9\)

In both models, the main construct to be estimated is the conditional earnings management probability $P(EARN, X)$. It is identified by the conditional earnings distribution for small losses and small profits, combined with the smoothness assumption that ties the distribution of pre-managed earnings in these narrow intervals to the distribution of observed earnings in the adjacent

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\(^8\) A more flexible specification such as $q(EARN^*) = 2(1 - \lambda|EARN^*|/K^-)$ with a slope coefficient $\lambda$ would complicate the estimation by introducing three-way interactions between the coefficients. Researchers are primarily interested in the effect of the explanatory variables $X$ on earnings management through the coefficients $\pi$, as opposed to the exact functional form of $q(z)$. Therefore, we choose a simple specification of $q(z)$ that can be combined with a richer specification of $\pi$.

\(^9\) Formally, let $M(X) = \int_{-K^+}^{0} P(EARN^*, X)f^*(EARN^*|X) dEARN^*$ be the total mass of missing small losses due to earnings management conditional on $X$ (the shaded area to the left of zero in Panel C of Figure 1). In Model I, $G(EARN, X) = (1/K^+) \times M(X)$, where the ratio $1/K^+$ adjusts the density for the width of the small-profit interval. In Model II, $G(EARN, X) = q^*(EARN) \times (1/K^+) \times M(X)$, where $q^*(EARN)$ is a triangular interaction term that decreases linearly from 2 at $EARN = 0$ to 0 at $EARN = K^+$, by analogy with $q(EARN^*)$ in (5b). For our functional forms, the integral in $M(X)$ can be computed analytically.
intervals (Figure 2). Therefore, we must accurately capture the earnings distribution in a relatively narrow region around the small-loss and small-profit intervals. In contrast, earnings that are far from zero do not help identify the earnings management probability. Further, these earnings values could potentially confound the estimates if the model’s earnings distribution is misspecified far from zero. To avoid this unnecessary complication, we estimate the models only for earnings near zero, as shown in Panel C of Figure 3. For example, when the small-loss and small-profit interval widths are set to $K^- = K^+ = 0.01$ (i.e., 1% of the lagged market value of equity), we restrict the estimation interval of earnings to $[-0.04, 0.04)$ and discard observations outside this interval. The results are robust to adjusting the width of the estimation interval.\textsuperscript{10}

The restricted estimation interval lets us model the conditional probability density function of pre-managed earnings (1) using a flexible polynomial approximation\textsuperscript{11}

\[ f^*(EARN^* = z|X) = \alpha_0(X) + \alpha_1(X) \times z + \alpha_2(X) \times z^2 + \cdots + \alpha_P(X) \times z^P \quad (6a) \]

where $P$ is the degree of the polynomial, and the polynomial coefficients $\alpha_p(X)$ can vary with $X$

\[ \alpha_p(X) = \alpha_{p,0} + \alpha_{p,1}X_1 + \cdots + \alpha_{p,M}X_M \quad (6b) \]

where $\alpha_{0,0} \ldots \alpha_{P,M}$ are estimation parameters.

2.4. Estimation

We examine two estimation approaches. First, we implement maximum likelihood (ML) estimation with a custom-written likelihood function that incorporates the restrictions from

\textsuperscript{10} This estimation approach involves selection on the dependent variable. It should never be used to estimate the conditional mean of earnings (e.g., in a regression for earnings) because the mean is determined by the entire earnings distribution. However, when the objective is to characterize the shape of a conditional distribution in a particular interval, this approach yields valid estimates, as we prove in Online Supplement B.

\textsuperscript{11} The polynomial specification is more flexible than standard parametric distributions such as Normal or Student’s $t$. It provides a simple, stable local approximation in our narrow estimation interval, but should not be used over much broader intervals because the polynomial terms explode for earnings far from zero.
equations (4a)–(6b).\textsuperscript{12} Online Supplement B provides the implementation details. ML estimation directly fits the conditional density function of earnings from the model to the data. It is asymptotically efficient (Wooldridge, 2002, Ch. 14), providing an upper bound of potential test performance, but is relatively difficult to use.

Our second estimation approach is slightly less efficient than ML, but it is easier to implement and is more computationally robust. We break down the estimation into two ordinary least squares (OLS) stages, where all the model restrictions are embedded in the construction of the explanatory variables in stage 2. This two-stage structure lets us use the simple OLS method to estimate a model with non-linear restrictions. Notably, because OLS directly computes the estimates using matrix algebra, it avoids the numerical complications that arise in iterative search-based methods such as ML or non-linear least squares. For example, because numerical search can converge to a local maximum (and there might be multiple local maxima), ML can yield different estimates depending on a researcher’s choice of initial values and optimization settings (e.g., Hessian updating method). In contrast, our two-stage approach (1) does not require a researcher to make these choices, and (2) directly computes the unique global maximum. Therefore, we recommend this estimation approach for future research.

To facilitate researchers’ use of our two-stage approach, we wrote a custom estimation command in Stata. This command automatically performs all of the data transformations and estimation steps described below, and the estimation output can be combined with standard Stata hypothesis tests. After downloading the ado file of our command from http://astro.temple.edu/~dbyzalov/, a researcher can obtain the estimates and the distribution

\textsuperscript{12} Chen et al. (2010) estimate a mixed-Normal model of earnings discontinuity using ML. However, they only measure the average unconditional discontinuity in the data and do not incorporate explanatory variables. Thus, they do not realize the primary benefit of this modeling approach.
graphs with just one line of code. The Appendix provides usage instructions, and table and figure notes provide examples of how we use this command to generate our estimates and graphs.

We describe the key estimation steps below and relegate the details to Online Supplement C. Because OLS cannot estimate a continuous density function, we discretize earnings into bins and work with this discrete distribution. For example, when the estimation interval for earnings (Panel C of Figure 3) is ±0.04 and the bin width is 0.0025, each firm-year observation in the estimation interval is converted into 32 \([=2 \times 0.04/0.0025]\) firm-year-bin observations with a dummy dependent variable that equals 1 if earnings are in the respective bin and 0 otherwise.

In the first stage, we estimate the pre-managed earnings distribution \((6a)\), using firm-year-bin data for all bins outside the small-loss and small-profit intervals. In a model without explanatory variables, stage 1 can be visualized as a polynomial that best fits the empirical histogram of earnings outside the small-loss and small-profit intervals (Panel A of Figure 4). When there are one or more explanatory variables, stage 1 is analogous to fitting earnings histograms for different values of \(X\). Equation \((6a)\) pools the distribution data conditional on \(X\), extracting useful information even from conditional histograms that are based on just a few observations. By excluding the small-loss and small-profit bins from the estimation sample in stage 1, we ensure that the estimates of the pre-managed earnings distribution are not influenced by meet-or-just-beat

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13 Because we use firm-year-bin data (and not histogram bin counts) in estimation, we can cluster the standard errors to account for cross-bin correlations within each firm-year, and we can incorporate explanatory variables. Labor economics researchers (e.g., Chetty et al., 2011; Kleven and Waseem, 2013; Kleven, 2016) examine discontinuities caused by kinks in the marginal tax rates for labor income. These papers fit a flexible polynomial to the empirical histogram of labor income, using data away from the kink, and then measure the difference between actual and predicted bin counts near the kink. However, similar to the Burgstahler and Dichev (1997) test, the empirical approach in these economics papers cannot incorporate multiple explanatory variables. Algebraically, our estimation approach resembles regression discontinuity methods (e.g., Wooldridge, 2002, Ch. 18), which use a flexible smooth function of a forcing variable and additional covariates in a narrow interval around the discontinuity combined with a step function at the discontinuity threshold. However, the regression discontinuity approach is used to study causal effects when a decision-maker cannot manipulate the forcing variable to be just below or just above the threshold, such that local variation around the threshold is exogenous (e.g., Atanasov and Black, 2016), whereas the distribution discontinuity approach is used to study a decision-maker who can successfully manipulate the outcome to move across the threshold.
behavior at zero. The estimated parameters are used to interpolate the pre-managed distribution in the small-loss and small-profit intervals, i.e., the economic null hypothesis, in the next step.

In the second stage, we estimate the earnings management probability (5a) in Model I or (5b) in Model II, using data for the small-loss and small-profit bins (Panel B of Figure 4). We regress deviations from the pre-managed earnings distribution on the product of (1) earnings management probability \( \pi_0 + \pi_1 X_1 + \cdots + \pi_M X_M \), which affects the height of the deviations, and (2) a synthetic explanatory variable shown in Panel C of Figure 4, which determines the shape of the deviations. This variable implements the logical restrictions of our models. First, it defines flat deviations for Model I and triangular deviations for Model II, following the model definitions in Figure 3. Second, the negative values of this variable for the small-loss bins are exactly offset by the positive values for the small-profit bins, thus implementing the restriction that excess small profits must be consistent with missing small losses (Panel C of Figure 1). Third, the amplitude of this variable is proportional to the pre-managed probabilities for the small-loss bins, thus implementing the logical restriction in (4b) that the density of managed small losses varies with the density of pre-managed small losses. This proportionality restriction is important because it lets us separate the effect of \( X \) on the earnings management probability from the effect of \( X \) on the pre-managed distribution.\(^{14}\)

Our two-stage estimation can be interpreted as Generalized Method of Moments (GMM) estimation, as shown in Online Supplement C. Therefore, our method has the standard GMM properties of consistency and asymptotic efficiency (e.g., Wooldridge, 2002, Ch. 14).

\(^{14}\) Suppose that a variable \( X \) does not affect the earnings management probability, but it increases the pre-managed earnings density in the small-loss interval (e.g., a higher \( X \) is associated with a taller, narrower pre-managed earnings distribution with a peak near zero). The density of managed small losses (i.e., the height of the shaded area to the left of zero in Panel C of Figure 1) in this scenario increases with \( X \), but it is strictly proportional to the density of pre-managed small losses conditional on \( X \). In contrast, when \( X \) affects the earnings management probability, it has an additional effect on the density of managed small losses that is not proportional to the pre-managed density. The synthetic explanatory variable absorbs the proportional effect, and therefore the coefficients \( \pi \) on \( X \) only capture the relevant incremental effect of \( X \) through the earnings management probability.
3. Empirical results

3.1. Sample selection

We use U.S. Compustat data for 1988–2015. Following Burgstahler and Dichev (1997), we discard financial firms and utilities (SIC codes 6000–6500 and 4400–4999, respectively) and require data on current net income (Compustat item NI) and lagged market value of common equity (PRCC_F×CSHO). To maintain a consistent sample in all of the main tests, we also require data on lagged total assets (item AT), lagged non-cash current assets and current liabilities (ACT–CHE and LCT–DLC, respectively), and lagged cost of goods sold (COGS) for the computation of the control variables. We require additional data for some tests. We measure earnings as net income scaled by the lagged market value of common equity. The dependent variable in our method must be scaled appropriately, following Burgstahler and Chuk’s (2015) recommendations for histogram-based discontinuity tests, even when the model incorporates flexible size controls.\(^{15}\)

Table 1 summarizes the sample construction. After imposing our main data requirements, the full sample comprises 152,586 firm-years. Most of our tests only use the subsample with scaled earnings in the \([-0.04, 0.04)\) interval, which comprises 34,483 firm-years.

3.2. Basic earnings discontinuity estimates without explanatory variables

To introduce our method, first we estimate Models I and II without any explanatory variables. These basic unconditional estimates parallel the standard Burgstahler and Dichev (1997) histogram-based tests. We let the data guide our choice of the polynomial approximation order, earnings management specification (i.e., Model I vs Model II), and small-loss and small-profit

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\(^{15}\) For example, size-related variation in earnings for Compustat firms spans several orders of magnitude. In a model for unscaled earnings, this variation will likely overwhelm any size controls, no matter how flexible, and \textit{a priori} the estimates lack any credibility. Because the small-loss and small-profit interval widths \(K^-\) and \(K^+\) in our method are restricted to be the same across all firm-years, a researcher should choose a scaling variable that is approximately proportional to the range of earnings that is likely affected by meet-or-just-beat behavior. After rescaling earnings, the scaling variable can also be included as a control to capture any residual effects.
interval widths $K^-$ and $K^+$. We choose a third-order polynomial ($P = 3$) for the probability density function (6a) because cubic terms are often significant and higher-order polynomial terms are consistently insignificant in untabulated tests, and we verify the approximation quality visually (Figure 5); the results are robust to alternative $P$. We check different combinations of interval widths $K^- = 0.005, 0.01, 0.015$ and $K^+ = 0.005, 0.01, 0.015$ and select the one with the highest log-likelihood (in ML) or adjusted $R^2$ (in two-stage estimation). The estimation parameters are the polynomial coefficients $\alpha_0 \ldots \alpha_3$ and the earnings management probability coefficient $\pi_0$.

Panel A of Table 2 presents the maximum likelihood (ML) estimates. ML is more numerically complex than our main two-stage estimation, but it helps us choose the empirical specification and assess the efficiency of the two-stage estimates. For brevity, we only tabulate the results with symmetric interval widths $K^- = K^+$.

In all cases, Model II fits the empirical earnings distribution better than Model I, as illustrated in Panel A of Figure 5, and the difference in log-likelihood is significant in the Vuong test.\(^{16}\) The small-loss and small-profit interval widths $K^- = 0.01$ and $K^+ = 0.01$ for Model II yield higher log-likelihood than all other values of $K^-$ and $K^+$ on our search grid, including untabulated asymmetric intervals $K^- \neq K^+$. Thus, the data favor Model II with $K^- = K^+ = 0.01$ (column 4). Per this specification, a smaller “small loss” is more likely to trigger earnings management than a bigger “small loss”, and a smaller “small profit” is more likely to be the result of earnings management than a bigger “small profit”.\(^{17}\)

We choose the default bin width for two-stage estimation as $2 \times IQR \times N^{-1/3}$, following Degeorge et al. (1999), where sample size $N = 34,483$ and interquartile range $IQR = 0.0386$

\(^{16}\) The Vuong (1989) test is defined only for ML. Dechow (1994) derives the Vuong test for OLS. However, her derivation hinges on the ML interpretation of OLS, which is valid only for i.i.d. Normal regression residuals and does not generalize to our two-stage estimation approach.

\(^{17}\) Consistent with our results, Figures 3 and 4 in Burgstahler and Dichev (1997) show that earnings discontinuity spans several bins and is largest in the bins just below and above zero. This pattern better fits Model II than Model I.
(untabulated). This formula yields a recommended bin width of 0.0024, which we round to 0.0025 for convenience. Panel B of Table 2 presents our main two-stage estimates. Consistent with the ML results in Panel A, Model II has higher adjusted $R^2$ than Model I as well as better visual fit (Panel B of Figure 5), and the interval widths $K^- = K^+ = 0.01$ in column 4 have higher adjusted $R^2$ than all other values of $K^-$ and $K^+$ (including untabulated $K^- \neq K^+$) on our search grid.\textsuperscript{18} The earnings management probability coefficient for this specification is $\pi_0 = 0.120$, i.e., on average, 12% of small losses in the $[-0.01, 0)$ interval are managed upward and reported as small profits, and it is statistically significant ($t = 10.49$). For comparison, in a Burgstahler and Dichev (1997) histogram-based test for the same sample, the standardized left difference is $-9.06$ and the standardized right difference is 7.23. The model restriction that all managed small losses must become small profits (Panel C of Figure 1) combines the data below and above zero into a single estimate $\pi_0$, resulting in a more powerful test with a higher test statistic.\textsuperscript{19}

Panel C of Table 2 examines alternative empirical settings for our two-stage estimates. First, we set the bin width to 0.005 following Burgstahler and Dichev (1997). The earnings management probability estimates remain similar (e.g., $\pi_0 = 0.118$, $t = 10.10$ for the main specification in column 4). We next use a finer earnings discretization with bin width set to 0.001, as illustrated in Panel C of Figure 5. Because our method pools information from all relevant bins, we can use these narrow bins without sacrificing test power. The earnings management probability estimate

\textsuperscript{18} The adjusted $R^2$ appears low (less than 1%) because the estimation uses dummy dependent variables. For example, when the conditional bin probability is 0.4, the bin-level dependent variable is 0 with probability 60% and 1 with probability 40%. Even when the model perfectly captures the true bin probability, actual values for each firm-year-bin observation deviate from the predicted value (0.4) by either $-0.4$ or 0.6, reducing the $R^2$. Additionally, Models I and II have identical predicted probabilities outside the small-loss and small-profit intervals, which shrinks the difference in adjusted $R^2$ between the two models. Therefore, even when one model fits the actual distribution considerably better than the other, as shown in Panel B of Figure 5, the difference in adjusted $R^2$ can be small.

\textsuperscript{19} Under the maintained assumption that the model is correctly specified, the ML estimates are asymptotically more efficient than the two-stage estimates. As expected, ML estimation in column 4 of Panel A yields a similar point estimate ($\pi_0 = 0.124$) with an even higher test statistic ($t = 12.27$).
increases slightly to $\pi_0 = 0.122 \ (t = 10.74)$. Notably, reducing the bin width to 0.001 improves the $t$-statistics in all columns, likely because the finer bin grid better captures the underlying continuous distribution. The results are also robust to varying the order of the polynomial approximation, the width of the estimation interval, and the degree of asymmetry between the small-loss and small-profit interval widths.\textsuperscript{20}

3.2.1. Type-I error and statistical power in tests without explanatory variables

First, following Burgstahler and Chuk (2015), we examine discontinuities in unscaled earnings per share (Compustat item EPSFX) at each one-cent threshold from $0.00$ to $1.00$. The thresholds at multiples of 10 cents are likely affected by meet-or-just-beat behavior (e.g., Thomas, 1989), while the remaining thresholds are “pseudo-targets” without clear meet-or-just-beat incentives (e.g., Ayers et al., 2006). They let us assess statistical power and Type-I error, respectively.

We use one-cent bins based on the resolution of the EPS data and use an estimation interval of ±5 cents around each threshold. To pinpoint the location of each discontinuity, we set the interval widths $K^-$ and $K^+$ to one cent (the results are robust to using two-cent intervals). Burgstahler and Chuk (2015) find that the EPS distribution for Compustat firms in the bottom share price quartile is very different from the other three quartiles, which can distort distribution discontinuity tests for the full sample. Therefore, we also examine a restricted sample without the bottom quartile.

Figure 6 presents the $t$-statistic on the earnings management probability coefficient $\pi_0$ at each threshold. Because upward earnings management is only consistent with positive $\pi_0$, we use one-

\textsuperscript{20} In both models, the probability estimate $\pi_0$ decreases with the width of the small-loss interval $K^-$, because the total incidence of earnings management is determined by (1) the probability that pre-managed earnings are in the small-loss interval, and (2) the probability of earnings management conditional on the small loss. The first number increases with the width of the small-loss interval; therefore, for the same total incidence of earnings management, the second number must decrease. Burgstahler and Dichev (1997, p. 108) note a similar tradeoff in their estimates of the prevalence of earnings management.
tailed tests. Out of the 11 thresholds at multiples of 10 cents, 10 are significant for the full Compustat sample in Panel A, and all 11 are significant for the restricted sample in Panel B. Among the 90 pseudo-thresholds without clear meet-or-just-beat incentives, only 6 (3) are significant in Panel A (B), in line with the nominal 5% significance level. Thus, our method successfully detects discontinuity where it is expected, showing high test power, and it successfully detects the absence of discontinuity where it is not expected, showing good Type-I error performance.

We next simulate a horse race between our method and standard Burgstahler and Dichev (1997) tests. The simulation protocol and detailed results are presented in Online Supplement D. While both methods have good Type-I errors, our method improves statistical power considerably. For example, in the simulation scenario with earnings management probability of 2.5% and sample size 30,000, the rejection rates improve from 59.3% (42.4%) in the left (right) standardized difference test to 71.0% for the main two-stage version of our method with bin width 0.0025. Further, our two-stage estimation approach is almost as efficient as ML (the asymptotically efficient upper bound of test performance), but without any of the numerical complications of ML. Thus, our method offers a sizable test power improvement at a low implementation cost.

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21 The left (right) standardized difference test detects 9 (10) significant discontinuities out of 11 in the full sample, and 8 (9) out of 11 in the restricted sample, indicating slightly lower test power. Consistent with Burgstahler and Chuk’s (2015) findings, the discontinuity at the zero EPS benchmark in our full sample is insignificant, and the reason is that firms in the bottom share price quartile have a large concentration of EPS values just below zero.

22 When we use a finer bin width 0.001, test power is slightly higher but the estimation is more time-consuming for reasons explained in the Appendix. In additional simulations, we examine whether the cubic polynomial approximation in our main tests has an advantage over a simpler linear interpolation. Linear interpolation over the main estimation interval \([-0.04, 0.04]\) is vulnerable to distribution non-linearities and often has excessive Type-I errors. Linear interpolation over a narrower interval such as \([-0.015, 0.015]\) has acceptable Type-I errors but has lower power because it uses less data. In all cases, the polynomial approximation performs at least as well as linear interpolation, and it often performs considerably better (e.g., when the discontinuity is near the peak of an asymmetric distribution with high curvature), without any additional implementation costs. In additional tests, we combine the cubic polynomial approximation with dummies for each of the small-loss and small-profit bins, following Chetty et al. (2011). Statistical power decreases considerably because this approach does not impose the restriction that the missing small losses must be consistent with the excess small profits (Panel C of Figure 1).
Therefore, it can benefit researchers even in basic tests without explanatory variables, especially when the sample size or effect size is small.

3.2.2. Examples of unreasonable empirical design choices for our tests

The choice of the estimation interval and polynomial approximation order require researcher judgment. The estimation interval should contain sufficient data to reliably estimate the smooth pre-managed earnings distribution near the zero benchmark, and be sufficiently compact to allow accurate polynomial approximation of the local earnings density function. The polynomial order should be sufficiently high to provide a reasonably accurate approximation, but it should be sufficiently low (e.g., 3 or 4, definitely not 10 or 15) to provide a smooth approximation and avoid overfitting (Leamer, 1983). Figure 7 illustrates violations of these guidelines. In Panel A, the estimation interval $[-0.015, 0.015)$ is only slightly wider than the small-loss and small-profit intervals. The pre-managed earnings distribution is identified by data only in the two narrow intervals $[-0.015, -0.01)$ and $[0.01, 0.015)$, which might lead to unreliable estimates. In Panel B, the estimation interval $[-0.2, 0.2)$ is too wide. The estimation procedure tries to fit the earnings distribution over the entire interval $[-0.2, 0.2)$ using a simple polynomial function (6a) and cannot accurately fit the distribution in the important earnings range near zero. Therefore, the estimates are likely unreliable. In Panel C, we use a 15-th order polynomial. The polynomial overfits the estimation sample in stage 1 and provides an $a$ priori unusable interpolation in stage 2. These unreasonable specifications are easy to detect visually (option graph in our Stata command).
3.3. Earnings discontinuity estimates with explanatory variables: Standard logit approach versus our method

3.3.1. What is wrong with logit for the meet-or-just-beat dummy?

Barton and Simko (2002), Frankel et al. (2002), Matsumoto (2002), Ashbaugh et al. (2003), Cheng and Warfield (2005), Jiang et al. (2010), and others study potential determinants of earnings management by incorporating them as explanatory variables in a logit model with a dummy dependent variable for meeting or beating an earnings benchmark, such as the small-profit dummy in our context. 23 This definition of the dependent variable compresses the earnings distribution into a binary distribution for the small-profit dummy, discards all information about the shape of the original distribution, and lumps together pre-managed and managed small profits.

This crude information structure confounds the inferences drawn from the small-profit dummy (in logit or any similar model). Suppose that a variable $X$ (e.g., R&D intensity) affects both the distribution of pre-managed earnings (e.g., R&D intensive firms have more volatile earnings) and earnings management incentives. The first channel affects the probability of pre-managed small profits and the second channel affects the probability of managed small profits. Estimation for the small-profit dummy correctly measures the total effect of $X$ on the probability of all small profits (the right graph in Panel D of Figure 1), but it cannot isolate the net effect of $X$ on the probability of managed small profits. In other words, the meet-or-just-beat probability profile does not contain sufficient information to estimate the determinants of meet-or-just-beat behavior.

In contrast, our method combines information on small losses and profits with information on the shape of the earnings distribution outside the small-loss and small-profit intervals. This

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23 Some authors (e.g., Barton and Simko, 2002; Matsumoto, 2002; Jiang et al., 2010) define the dependent variable as “meeting or beating” the benchmark by any amount, while others (e.g., Frankel et al., 2002; Cheng and Warfield, 2005) restrict attention to “meeting or just beating” the benchmark. The “meet or beat” definition is less appropriate because it can be confounded by economically irrelevant distribution changes far from the benchmark.
additional information lets us isolate the net effect of $X$ on the probability of managed small losses and profits (the left graph in Panel D of Figure 1), producing credible inferences on the determinants of meet-or-just-beat behavior.

We validate these arguments using simulations in the next subsection.

3.3.2. Simulation evidence: Logit for the small-profit dummy versus our method

For each observation, we generate an explanatory variable $X$ that equals 0 or 1 with probability 0.5 each (the results generalize to a continuous $X$ and multiple $X$s in untabulated tests). $X$ affects the shape of the pre-managed earnings distribution (6a) as shown in Figure 8, and it can also affect the earnings management probability (5b). We simulate $X$ and earnings conditional on $X$ in 1,000 samples with 5,000 or 30,000 observations per sample. In each sample, we test whether $X$ affects the small-profit probability in the logit model and the earnings management probability in our main Model II.24 Table 3 presents the simulated rejection rates.

The proportion of small pre-managed profits varies from 12.1% for $X = 0$ to 11.1% for $X = 1$ (Figure 8). This variation in the pre-managed distribution confounds the interpretation of the logit estimates. Even when $X$ does not affect the earnings management probability (columns 1 and 4 in Table 3), logit detects a significant effect of $X$ in 17.6% of the simulations for $N = 5,000$ and 75.7% of the simulations for $N = 30,000$. Thus, a significant effect of $X$ in the logit model does

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24 In general, the alternative hypothesis for $X$ can include both positive and negative coefficient values. Therefore, we use two-tailed tests. The intercept in the logit model cannot be interpreted as a measure of earnings discontinuity. Therefore, we only focus on the effect of $X$. The small-profit dummy in the logit model equals 1 when simulated earnings are between 0 and 1.0% of the lagged market value of equity, for consistency with the true earnings management process in the simulated data. Our simulation protocol restricts simulated earnings to the interval $[-0.04, 0.04]$. Prior research often estimates logit models for earnings discontinuity using samples that span the entire earnings distribution (e.g., Frankel et al., 2002; Matsumoto, 2002). However, this empirical design is both unnecessary, because observations far from zero do not provide any information on earnings discontinuity at zero, and dangerous, because the logit estimates can be confounded by model misspecification for earnings far from zero. By restricting the estimation sample for the logit model to our main estimation interval $[-0.04, 0.04]$, we give logit the best chance to be competitive. In untabulated tests for an unrestricted range of simulated earnings, the logit model suffers from additional biases when $X$ affects the proportion of earnings that fall within the $[-0.04, 0.04]$ interval.
not indicate that $X$ affects earnings management. In contrast, our method yields valid type-I errors.

When $X$ affects the earnings management probability (columns 2–3 and 5–6), the logit model fails to detect this effect. For example, in columns 3 and 6, $X = 1$ increases the earnings management probability by 10 percentage points relative to $X = 0$. For $N = 5,000$, the logit model detects this effect in only 4.9% of simulated samples, versus 41.5–46.2% for our Model II (column 3). Even for $N = 30,000$, the logit rejection rate is only 5.2%, versus 98.9–99.6% in Model II (column 6). Thus, an insignificant effect of $X$ in the logit model for the small-profit dummy does not indicate that $X$ is irrelevant for earnings management.

In summary, the meet-or-just-beat dummy does not contain sufficient information to separate the effect of $X$ on meet-or-just-beat behavior from the effect of $X$ on the pre-managed earnings distribution. Therefore, researchers should never use estimates for the meet-or-just-beat dummy (in logit or any similar model) to characterize the determinants of meet-or-just-beat behavior.

3.3.3. Empirical estimates of distribution discontinuity determinants: Logit for the small-profit dummy versus our method

To compare the empirical performance of the standard logit approach with our conditional discontinuity method with explanatory variables, we measure the impact of several major determinants of earnings discontinuity from prior research. Following Burgstahler and Dichev

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25 The rejection rate in the logit model decreases when $X$ has a stronger positive effect on the earnings management probability. The reason is that this positive effect is offset by a larger negative confounding effect of $X$ from Figure 8, such that the total effect of $X$ on the small-profit probability is negative. In untabulated additional simulations, we remove the confounding effect of $X$ (i.e., $X$ does not affect the simulated pre-managed distribution). The logit model in this best-case scenario has acceptable Type-I errors, as expected. However, it has considerably lower statistical power than our method because it reduces earnings information to a crude dummy variable for small profits. In another untabulated simulation, $X$ does not affect the true earnings management probability for small losses, but it increases the true range of managed small losses and profits from ±0.005 for $X=0$ to ±0.01 for $X=1$, such that the total incidence of earnings management increases with $X$. Although our estimation method assumes constant small-loss and small-profit interval widths $K^-$ and $K^+$ for computational tractability, it can approximate this effect of $X$ in the reduced form by letting the measured earnings management probability vary with $X$ over a fixed small-loss interval. As expected, our method successfully detects the effect of $X$ on the total incidence of earnings management in this simulation.
(1997), we use current asset intensity and current liability intensity as proxies for a firm’s ability to manage earnings through working capital manipulation. Following Burgstahler and Chuk (2017), we use COGS intensity and R&D intensity as proxies for implicit claims by stakeholders such as customers, employees, and suppliers. These implicit claims could create contracting incentives for earnings management. We convert the explanatory variables into tercile dummies for consistency with Burgstahler and Chuk (2017). Because earnings management can affect concurrent assets, liabilities, COGS, and R&D expense, we lag the explanatory variables. A priori, we do not expect to overturn the fundamental intuition on these variables; instead, our objective is to empirically validate our method by confirming that it produces the expected inferences for these known discontinuity determinants, and to contrast the results with the parallel logit estimates.

The estimates are presented in Table 4. Column 1 presents the standard logit estimates for the small-profit dummy in the full sample. The measured effects of current liability intensity and COGS intensity are significantly negative, both of which contradict the theory (Burgstahler and Dichev, 1997; Burgstahler and Chuk, 2017). Column 2 restricts the logit sample to scaled earnings in the ±0.04 interval for consistency with our main method. The estimates change and no longer contradict the theory. These results suggest that, in addition to the fundamental identification problem for the meet-or-just-beat dummy from subsection 3.3.1, the standard logit estimates for the full sample are also confounded by irrelevant observations with earnings far from zero.\(^{26}\)

Column 3 presents the conditional distribution discontinuity estimates for our main Model II. The coefficients are consistent with the theory (Burgstahler and Dichev, 1997; Burgstahler and

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\(^{26}\) The conditional probability of a small profit \( \Pr(EARN \in [0,K^+]|X) \) in the full sample can be decomposed as \( \Pr(EARN \in [0,K^+]|EARN \in [-0.04,0.04),X) \times \Pr(EARN \in [-0.04,0.04]|X) \), where the first term is the probability of a small profit for observations in the restricted subsample and the second term is the probability of inclusion in the restricted subsample. In untabulated analysis, \( \Pr(EARN \in [-0.04,0.04]|X) \) varies significantly with the control variables. This indicates that the estimates for the full sample are influenced by the irrelevant parts of the earnings distribution outside the \([-0.04,0.04]\) interval.
Chuk, 2017), i.e., our method performs as expected in estimation with multiple explanatory variables. The estimates differ qualitatively from the corresponding logit estimates in column 2 (e.g., the coefficient on CL3 changes from significant to insignificant, and the coefficient on COGS3 changes from insignificant to significant), and they are more empirically credible than the logit estimates given the simulation evidence in subsection 3.3.2.

The remaining columns in Table 4 examine the sensitivity of our estimates to empirical design choices. The estimates are generally robust to alternative definitions of the small-loss and small profit intervals in columns 4–5, different estimation intervals in columns 6–7, different bin widths in columns 8–9, the use of continuous explanatory variables instead of tercile dummies in column 10, and ML estimation in column 11. Thus, our method performs as expected in estimation for alternative empirical settings.27

4. Application: The balance sheet as an earnings management constraint

Using a generalized logit model, Barton and Simko (2002) find a negative association between beginning-of-period net operating assets (NOA) and the probability of meeting or beating the consensus analyst EPS forecast.28 They attribute this result to prior net asset overstatement constraining current earnings management. However, as we show in Section 3.3, the logit model cannot distinguish variation in the pre-managed earnings distribution from variation in the earnings discontinuity, which confounds inferences in the logit model. Therefore, we re-examine the

27 Burgstahler and Chuk (2015, Figures 2, 4, and 5) find that both the pre-managed earnings distribution and the distribution discontinuity vary considerably across quartiles of lagged share price and lagged market value of equity. As an additional validation check, we incorporate these quartile dummies as explanatory variables in our method. As expected, our method successfully reproduces Burgstahler and Chuk’s results (untabulated).
28 Barton and Simko examine multiple EPS surprise thresholds from –5 cents to +5 cents. They use a logit specification for the probability of meeting or beating each threshold per their equation (2), and they estimate the model jointly for multiple thresholds using generalized ordered logit estimation. This joint estimation is necessary only if a researcher wants to conduct hypothesis tests across different thresholds; further, it might be less robust than standard logit estimation because the inferences for the economically important zero threshold could be confounded by model misspecification for other thresholds. Therefore, we use a standard logit model for the zero threshold in our replication.
relation between NOA and earnings management using our conditional discontinuity method.

Our sample and variable definitions replicate Barton and Simko (2002). We combine quarterly U.S. Compustat data with IBES analyst forecast data during 1993–1999 and discard financial firms and utilities (SIC codes 6000–6999 and 4400–4999). The final sample comprises 48,054 firm-quarter observations. The variable definitions are summarized in the notes to Table 5.

The dependent variables in all models are based on the EPS surprise, computed as actual EPS for the quarter minus the most recent consensus mean EPS forecast for the quarter and rounded to the nearest penny. Barton and Simko define the dummy dependent variable in the logit model as “meet or beat” the benchmark (i.e., EPS surprise ≥ 0); however, this definition could be confounded by large positive EPS surprises, which are unlikely to arise from benchmark-driven earnings management. Therefore, we also examine a “meet or just beat” dummy for EPS surprises of 0 and 1 cents following Cheng and Warfield (2005).

To adapt our Models I and II to analyst forecast data, we replace scaled earnings with EPS surprises and use 1-cent bins based on the resolution of the EPS surprise data. We interpret reported EPS surprises of 0 and 1 cents as potential outcomes of earnings management, following Cheng and Warfield (2005), and interpret pre-managed EPS surprises of -1 and -2 cents (i.e., the same number of bins for symmetry) as potential triggers of earnings management. We restrict the estimation sample for Models I and II to EPS surprises within the [-5 cents, 5 cents) interval, such that the pre-managed EPS surprise distribution is estimated based on 3 bins below zero and 3 bins

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29 Firms and analysts typically do not include fractions of a cent in their reported EPS and EPS forecasts (Das and Zhang, 2003; Dechow and You, 2012). Similar to DeGeorge et al. (1999) and Cheong and Thomas (2011), we find that the dispersion of IBES EPS surprises does not vary in proportion to scale (e.g., in all four quartiles of lagged share price, approximately two-thirds of all EPS surprises are in the ±5 cent interval), and therefore unscaled EPS surprise is more appropriate than EPS surprise scaled by share price. This conclusion does not generalize to EPS for all Compustat firms (Burgstahler and Chuk, 2015).
above zero (counting only the bins unaffected by earnings management).30

Columns 1–4 of Table 5 present the logit estimates. Column 1 replicates Barton and Simko’s (2002) estimates. Higher beginning-of-period NOA significantly reduces the probability of meeting or beating the forecast (the coefficient on NOA is \(-0.027, t = -7.84\)), consistent with Barton and Simko’s estimates (\(-0.031, t = -4.98\) in their Table 5). Column 2 redefines the dependent variable as “meet or just beat” the EPS forecast. The coefficient on NOA shrinks by one-third to \(-0.017\) but remains significant (\(t = -4.58\)). We next restrict the sample to EPS surprises in the \([-5\) cents, 5 cents\) interval. Because benchmark-driven earnings management converts small negative surprises into small non-negative surprises but does not affect large surprises, the logit model is estimated more reliably on this restricted sample. For both “meet or beat” in column 3 and “meet or just beat” in column 4, the logit coefficient on NOA remains negative and significant (\(-0.034, t = -7.75\) and \(-0.013, t = -3.00\), respectively).

Columns 5–9 present the estimates for our Models I and II. In untabulated exploratory analysis across various specifications for the EPS surprise data, Model I has consistently higher adjusted \(R^2\) than Model II, and the adjusted \(R^2\) for Model I is maximized when we use a fourth-order polynomial. Therefore, we use Model I with \(P = 4\) as our main specification for EPS surprises.

In column 5, the explanatory variables affect the earnings discontinuity but do not affect the pre-managed distribution. Thus, similar to the logit model, this restricted version of Model I attributes all changes in the EPS surprise distribution to earnings management. Beginning-of-period NOA has a significant negative effect on earnings management (\(-0.013, t = -3.15\), consistent with Barton and Simko (2002). In the full model in column 6, the explanatory variables

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30 To maintain consistent notation with Sections 2 and 3, the \([-5\) cents, 5 cents\) estimation interval is defined as a half-open interval, where the lower bound (-5 cents) is included and the upper bound (+5 cents) is excluded. Under this definition, the estimation sample includes five “miss” bins from -5 to -1 cents and five “meet or beat” bins from 0 to 4 cents, thus preserving symmetry in bins. The results are robust to including the 5-cent bin in the sample.
affect both the earnings discontinuity and the pre-managed distribution, thus letting us separate the earnings management effect from the confounding variation in the pre-managed distribution. The effect of NOA on earnings discontinuity weakens from $-0.013$ in column 5 to $-0.007$ in column 6 and is insignificant at the 10% level ($t = -1.45$). This insignificant result is robust to alternative empirical specifications in columns 7–9. Notably, NOA has a significant effect on the pre-managed distribution through the coefficients $\alpha_{0,NOA} \ldots \alpha_{4,NOA}$ ($F = 9.92 \ldots 11.84$ in columns 6–9). The economic null hypothesis of no earnings management (Ball, 2013) in our model incorporates this effect. In contrast, the simpler models in columns 1–5 implicitly assume that NOA does not affect the distribution when the null hypothesis of no earnings management is true. Therefore, the effect of NOA on the pre-managed distribution in these models is mistakenly attributed to the alternative hypothesis of earnings management. By disentangling these two effects, our full conditional discontinuity model in columns 6–9 leads to qualitatively different conclusions about the effect of beginning-of-period NOA on earnings management.

5. Conclusion

We propose conditional distribution discontinuity tests for studying meet-or-just-beat behavior that overcome important limitations of the existing tests. A standard Burgstahler and Dichev (1997) histogram-based test for benchmark-driven earnings management (and other forms of meet-or-just-beat behavior) cannot incorporate multiple explanatory variables. Therefore, to study the

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31 To directly show this effect, we estimate untabulated logit models for the probability of each EPS surprise in the ±5 cent interval, using the full list of explanatory variables from Table 5. While logit cannot separate the effect of $X$ on the pre-managed distribution from the effect of $X$ on distribution discontinuity (Section 3.3), it correctly measures the sum of these two effects. The effect of NOA is largest for EPS surprises far from zero ($-5$ cents, $-4$ cents, and $+3$ cents). Because these large surprises are unlikely to be affected by meet-or-just-beat behavior at the zero-cent benchmark, the results indicate that NOA affects the pre-managed distribution. In contrast, we do not find any unusual patterns in the effect of NOA on EPS surprises near zero. To confirm the robustness of our results to Burgstahler and Chuk’s (2015) scale concerns for EPS, we repeat this reduced-form logit analysis and our main analysis from Table 5 after discarding firms in the bottom quartile of lagged share price. The results continue to hold (untabulated).
determinants of earnings management, researchers typically use a logit model for the meet-or-just-beat dummy. However, this standard empirical approach cannot distinguish variation in earnings discontinuity from variation in the pre-managed earnings distribution, which can lead to faulty inferences about the determinants of earnings management. We develop a flexible statistical model that can incorporate multiple explanatory variables and that can separate these variables’ effect on distribution discontinuity from their effect on the pre-managed distribution. We propose a simple two-stage estimation procedure that is almost as efficient as maximum likelihood and is much easier to use. While we present the analysis in the context of earnings management, our method can be applied to any distribution discontinuity around a salient performance benchmark with incentives to move from just below to just above the benchmark or vice versa.

Simulation analysis shows that our conditional discontinuity method considerably outperforms the standard tests in terms of both Type-I error and statistical power. The widely used logit estimates for the meet-or-just-beat dummy suffer from large Type-I errors. They should never be used to study the determinants of meet-or-just-beat behavior. In contrast, our method yields reliable inferences on these determinants because we specifically model how they affect the smooth pre-managed distribution under the economic null hypothesis of no earnings management (Ball, 2013).

Our method changes some major findings in prior accounting research. For example, we show that Barton and Simko’s (2002) findings on the relation between beginning-of-period net operating assets and earnings management reflect variation in the pre-managed earnings distribution rather than an actual earnings management effect.
Our method is robust and can be implemented easily using the Stata estimation command that we made publicly available. Researchers who seek to model distribution discontinuities, whether in accounting or other fields, can benefit from our method.
References


Gelman, A., Stern, H., 2006. The difference between “significant” and “not significant” is not itself statistically significant. The American Statistician, 60(4), 328–331.


Panel A: Pre-managed earnings distribution

Panel B: Earnings management at the zero benchmark, holding $X$ constant

Panel C: Statistical properties of the earnings management process, holding $X$ constant

Panel D: Our method versus logit for small-profit dummy

Fig 1. Model summary
Fig 2. Predicted distribution of reported earnings in the model conditional on $X$.
Panel A: Model I — flat increments due to earnings management, holding X constant

Panel B: Model II — triangular increments due to earnings management, holding X constant

Panel C: Polynomial approximation in a narrow estimation interval for scaled earnings

Fig 3. Empirical implementation
Panel A: Stage 1 — estimation of the pre-managed earnings distribution

Panel B: Stage 2 — estimation of the earnings management process

Panel C: Structure of the synthetic explanatory variable in stage 2

**Fig 4.** Our two-stage estimation approach, illustrated in a model without explanatory variables

The bars represent the empirical histogram of earnings. In a specification without explanatory variables, our estimation for the firm-year-bin data can be visualized as fitting the model to the histogram bins. The first stage in Panel A estimates the pre-managed earnings distribution by fitting a polynomial to the bins outside the small-loss and small-profit intervals. The second stage in Panel B estimates the incremental effect of earnings management, relative to the pre-managed distribution from stage 1 (the dashed line), using small-loss and small-profit bins. The deviations from the pre-managed distribution are regressed on the synthetic explanatory variable from Panel C multiplied by the earnings management probability. The estimation equations from Panels A and B and the synthetic variable from Panel C are formally defined in Online Supplement C.
**Panel A:** Maximum likelihood estimates from columns 3–4 in Panel A of Table 2

**Panel B:** Two-stage estimates from columns 3–4 in Panel B of Table 2 (bin width = 0.0025)

**Panel C:** Two-stage estimates with a finer earnings discretization (bin width = 0.001)

**Fig 5.** Predicted earnings distribution based on the estimates in Table 2 and the empirical earnings histogram for comparison

In Panel A, the histogram bin width does not affect the ML estimates. We do not provide a code fragment for Panel A because it uses our custom ML code, which we do not package as a general-purpose estimation tool for future research. In Panels B and C, the two-stage estimates are based on the same bin grid as the corresponding histogram. The estimation command used in the code fragments for Panels B and C is described in the Appendix.
Panel A: Full Compustat sample

Panel B: Restricted Compustat sample without the bottom quartile of lagged share price

Fig 6. T-statistic for the earnings management probability coefficient in our method at each EPS threshold from $0.00 to $1.00, using unscaled EPS data

The code fragment for each test is
kinkyX diffEPS, binwidth(0.01) est_bins(5) em_bins(1) em_type(i) degree(3) cluster(gvkey)
where diffEPS is actual EPS (Compustat item EPSFX) in dollars and cents minus the corresponding threshold, and the estimation command tests for a discontinuity at zero for diffEPS.

The significant negative t-statistics at pseudo-thresholds one cent below and one cent above multiples of 10 cents are a normal consequence of applying the test to the wrong side of the actual discontinuity in the data. For example, when the threshold in estimation is set at 21 cents, our method expects a dip in the distribution just below the 21-cent threshold (i.e., at 20 cents), but instead it detects a spike due to the actual discontinuity at the 20-cent threshold. Therefore, it is important to use the one-tailed test for positive values.
Panel A: The estimation interval is too narrow. The estimates of the pre-managed earnings distribution are based on a limited range of data and might be unreliable.

```
kinkyX NI, binwidth(0.0025) est_bins(6) em_bins(4) em_type(ii) degree(3) cluster(gvkey) graph(Fig 7_panelA)
```

Panel B: The estimation interval is too wide. The cubic polynomial cannot accurately capture the pre-managed earnings distribution.

```
kinkyX NI, binwidth(0.0025) est_bins(80) em_bins(4) em_type(ii) degree(3) cluster(gvkey) graph(Fig 7_panelB)
```

Panel C: The polynomial order is too high ($P = 15$), leading to overfitting in the estimation sample for stage 1 and bad interpolation in stage 2.

```
kinkyX NI, binwidth(0.0025) est_bins(16) em_bins(4) em_type(ii) degree(15) cluster(gvkey) graph(Fig 7_panelC)
```

Fig 7. Examples of questionable empirical design choices that should be avoided
Pr(small profit)=12.1%  
Pr(small profit)=11.1%

**Fig 8.** The distribution of pre-managed earnings conditional on $X$ in the simulation in Table 3
<table>
<thead>
<tr>
<th>Step</th>
<th>Number of firm-year observations in the sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full annual U.S. Compustat sample during 1988–2015</td>
<td>323,829</td>
</tr>
<tr>
<td>Discard financial firms and utilities</td>
<td>276,723</td>
</tr>
<tr>
<td>Discard observations with missing data on net income or market value of equity</td>
<td>197,399</td>
</tr>
<tr>
<td>Discard observations with insufficient data to construct the main variables</td>
<td>152,586</td>
</tr>
<tr>
<td>Restrict the sample to scaled earnings in the [−0.04, 0.04) interval</td>
<td>34,483</td>
</tr>
</tbody>
</table>
Table 2. Basic estimates without explanatory variables

Panel A: Maximum likelihood estimates as a benchmark for asymptotically efficient estimates
The estimation interval is [−0.04, 0.04), cubic polynomial.

<table>
<thead>
<tr>
<th></th>
<th>Small-loss and small-profit interval width is</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$K^- = K^+ = 0.005$</td>
</tr>
<tr>
<td></td>
<td>Model I</td>
</tr>
<tr>
<td>Polynomial</td>
<td></td>
</tr>
<tr>
<td>coefficients</td>
<td></td>
</tr>
<tr>
<td>$\alpha_0^a$</td>
<td>1.00</td>
</tr>
<tr>
<td></td>
<td>(1.00)</td>
</tr>
<tr>
<td>$\alpha_1$</td>
<td>14.446***</td>
</tr>
<tr>
<td></td>
<td>(23.15)</td>
</tr>
<tr>
<td></td>
<td>(15.06)</td>
</tr>
<tr>
<td>$\alpha_3/100^b$</td>
<td>6.058</td>
</tr>
<tr>
<td></td>
<td>(0.97)</td>
</tr>
</tbody>
</table>

Earnings management probability for small-loss observations:

<p>| | | | | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$\pi_0$</td>
<td>0.173***</td>
<td>0.163***</td>
<td>0.128***</td>
<td>0.124***</td>
<td>0.105***</td>
<td>0.102***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(10.54)</td>
<td>(11.96)</td>
<td>(10.43)</td>
<td>(12.27)</td>
<td>(9.59)</td>
<td>(11.84)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>lnL</td>
<td>88,900.56</td>
<td>88,914.15</td>
<td>88,899.35</td>
<td>88,919.00</td>
<td>88,890.59</td>
<td>88,914.06</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Vuong Z</td>
<td>2.38**</td>
<td>3.31***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Panel B: Main two-stage estimates
The estimation interval is [−0.04, 0.04), cubic polynomial, bin width is 0.0025.

<table>
<thead>
<tr>
<th></th>
<th>Small-loss and small-profit interval width is</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$K^- = K^+ = 0.005$</td>
</tr>
<tr>
<td></td>
<td>Model I</td>
</tr>
<tr>
<td>Polynomial</td>
<td></td>
</tr>
<tr>
<td>coefficients</td>
<td></td>
</tr>
<tr>
<td>$\alpha_0$</td>
<td>0.028***</td>
</tr>
<tr>
<td></td>
<td>(101.69)</td>
</tr>
<tr>
<td>$\alpha_1$</td>
<td>0.417***</td>
</tr>
<tr>
<td>$\alpha_2/10^b$</td>
<td>0.596***</td>
</tr>
<tr>
<td></td>
<td>(12.95)</td>
</tr>
<tr>
<td>$\alpha_3/100^b$</td>
<td>0.052</td>
</tr>
<tr>
<td></td>
<td>(0.27)</td>
</tr>
</tbody>
</table>

Earnings management probability for small-loss observations:

<p>| | | | | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$\pi_0$</td>
<td>0.170***</td>
<td>0.161***</td>
<td>0.124***</td>
<td>0.120***</td>
<td>0.088***</td>
<td>0.093***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(9.49)</td>
<td>(10.14)</td>
<td>(9.19)</td>
<td>(10.49)</td>
<td>(7.35)</td>
<td>(9.69)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>adj. R² (%)</td>
<td>0.352</td>
<td>0.353</td>
<td>0.352</td>
<td>0.354</td>
<td>0.349</td>
<td>0.353</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(1) kinkyX Ni, binwidth(0.0025) est_bins(16) em_bins(2) em_type(ii) degree(3) cluster(gvkey)
(2) kinkyX Ni, binwidth(0.0025) est_bins(16) em_bins(2) em_type(ii) degree(3) cluster(gvkey)
(3) kinkyX Ni, binwidth(0.0025) est_bins(16) em_bins(4) em_type(i) degree(3) cluster(gvkey)
(4) kinkyX Ni, binwidth(0.0025) est_bins(16) em_bins(4) em_type(i) degree(3) cluster(gvkey)
(5) kinkyX Ni, binwidth(0.0025) est_bins(16) em_bins(6) em_type(i) degree(3) cluster(gvkey)
(6) kinkyX Ni, binwidth(0.0025) est_bins(16) em_bins(6) em_type(ii) degree(3) cluster(gvkey)
Panel C: Robustness checks — earnings management probability coefficient $\pi_0$ for alternative empirical definitions in two-stage estimation

Unless stated otherwise, the estimation interval is $[-0.04, 0.04]$, cubic polynomial, bin width is 0.0025.

<table>
<thead>
<tr>
<th>Bin width (option binwidth)</th>
<th>$K^- = K^+ = 0.005$</th>
<th>$K^- = K^+ = 0.01$</th>
<th>$K^- = K^+ = 0.015$</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.005</td>
<td>Model I (1)</td>
<td>Model I (2)</td>
<td>Model I (3)</td>
</tr>
<tr>
<td></td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>0.170***</td>
<td>0.170***</td>
<td>0.124***</td>
<td>0.118***</td>
</tr>
<tr>
<td>(9.47)</td>
<td>(9.47)</td>
<td>(9.19)</td>
<td>(10.01)</td>
</tr>
<tr>
<td>0.025</td>
<td>0.170***</td>
<td>0.124***</td>
<td>0.120***</td>
</tr>
<tr>
<td>(9.49)</td>
<td>(10.14)</td>
<td>(9.19)</td>
<td>(10.49)</td>
</tr>
<tr>
<td>0.001</td>
<td>0.170***</td>
<td>0.124***</td>
<td>0.122***</td>
</tr>
<tr>
<td>(9.51)</td>
<td>(10.42)</td>
<td>(9.23)</td>
<td>(10.74)</td>
</tr>
</tbody>
</table>

Polynomial degree for the pre-managed distribution (option degree)

<table>
<thead>
<tr>
<th>Polynomial degree</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.169***</td>
<td>0.161***</td>
<td>0.161***</td>
<td>0.163***</td>
</tr>
<tr>
<td>(9.49)</td>
<td>(10.13)</td>
<td>(9.90)</td>
<td>(10.42)</td>
</tr>
<tr>
<td>0.170***</td>
<td>0.124***</td>
<td>0.120***</td>
<td>0.126***</td>
</tr>
<tr>
<td>(9.49)</td>
<td>(10.14)</td>
<td>(9.19)</td>
<td>(10.49)</td>
</tr>
<tr>
<td>0.172***</td>
<td>0.126***</td>
<td>0.123***</td>
<td>0.128***</td>
</tr>
<tr>
<td>(9.46)</td>
<td>(10.10)</td>
<td>(9.15)</td>
<td>(10.37)</td>
</tr>
</tbody>
</table>

Estimation interval width (option est_bins)

<table>
<thead>
<tr>
<th>Estimation interval width</th>
<th>0.03</th>
<th>0.04</th>
<th>0.05</th>
<th>0.06</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.168***</td>
<td>0.160***</td>
<td>0.123***</td>
<td>0.120***</td>
<td>0.123***</td>
</tr>
<tr>
<td>(9.31)</td>
<td>(10.01)</td>
<td>(8.77)</td>
<td>(10.23)</td>
<td>(6.33)</td>
</tr>
<tr>
<td>0.170***</td>
<td>0.124***</td>
<td>0.120***</td>
<td>0.124***</td>
<td>0.118***</td>
</tr>
<tr>
<td>(9.49)</td>
<td>(10.14)</td>
<td>(9.19)</td>
<td>(10.49)</td>
<td>(7.35)</td>
</tr>
<tr>
<td>0.166***</td>
<td>0.118***</td>
<td>0.115***</td>
<td>0.118***</td>
<td>0.115***</td>
</tr>
<tr>
<td>(9.44)</td>
<td>(10.11)</td>
<td>(9.00)</td>
<td>(10.38)</td>
<td>(7.16)</td>
</tr>
<tr>
<td>0.159***</td>
<td>0.110***</td>
<td>0.109***</td>
<td>0.110***</td>
<td>0.109***</td>
</tr>
<tr>
<td>(9.38)</td>
<td>(10.07)</td>
<td>(8.78)</td>
<td>(10.25)</td>
<td>(6.33)</td>
</tr>
</tbody>
</table>

Small-profit interval width $K^+$ (option em_bins), for a given small-profit interval width $K^-$ from column heading

<table>
<thead>
<tr>
<th>Small-profit interval width</th>
<th>0.005</th>
<th>0.01</th>
<th>0.015</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.170***</td>
<td>0.161***</td>
<td>0.185***</td>
<td>0.187***</td>
</tr>
<tr>
<td>(9.49)</td>
<td>(10.14)</td>
<td>(8.84)</td>
<td>(8.56)</td>
</tr>
<tr>
<td>0.190***</td>
<td>0.124***</td>
<td>0.125***</td>
<td>0.118***</td>
</tr>
<tr>
<td>(9.66)</td>
<td>(10.30)</td>
<td>(9.19)</td>
<td>(8.56)</td>
</tr>
</tbody>
</table>

As an illustration, the code for several representative rows in column (4) is:

- bin width 0.001: kinkyX NI, binwidth(0.001) est_bins(40) em_bins(10) em_type(ii) degree(3) cluster(gvkey)
- degree 4: kinkyX NI, binwidth(0.0025) est_bins(16) em_bins(4) em_type(ii) degree(4) cluster(gvkey)
- estimation interval 0.06: kinkyX NI, binwidth(0.0025) est_bins(24) em_bins(4) em_type(ii) degree(3) cluster(gvkey)
- $K^+ = 0.005$: kinkyX NI, binwidth(0.0025) est_bins(16) em_bins(2) em_binsMinus(4) em_type(ii) degree(3) cluster(gvkey)

The table presents the estimates of Models I and II for alternative empirical specifications. The dependent variable $EARN$ is net income (NI) scaled by the lagged market value of equity (PRCC F×CSHO). *, **, and *** indicate significance at the 1%, 5%, and 10% level, respectively, in two-tailed tests. Panel A presents the maximum likelihood (ML) estimates. The $t$-statistics in parentheses in Panel A use the standard ML computation, which does not incorporate clustering. Panels B and C present the estimates for our two-stage estimation approach. The $t$-statistics in Panels B and C are clustered by firm and are adjusted for the first-stage estimation noise as described in Online Supplement C. Unless stated otherwise, the estimation sample comprises $N = 34,483$ firm-year observations with scaled earnings in the $[-0.04, 0.04]$ interval. The number of firm-year-bin observations in two-stage estimation is larger (e.g., for the empirical definitions in Panel B, there are $2 \times 0.04/0.0025 = 32$ bin-level observations per firm.
year). Because the two-stage estimates are clustered by firm, the larger sample size at the firm-year-bin level does not artificially inflate the \( t \)-statistics.

\(^a\) The intercept \( \alpha_0 \) in ML estimation is set to 1 and the density function is rescaled as explained in Online Supplement B. This rescaling is necessary only for the ML estimates in Panel A. We do not (and should not) use it in the two-stage estimation in Panels B and C. Because of this scaling difference, the polynomial coefficients are not comparable between ML and two-stage estimates.

\(^b\) The polynomial coefficients \( \alpha_2 \) on \( EARN^2 \) and \( \alpha_3 \) on \( EARN^3 \) are orders of magnitude larger than the other coefficients because \( EARN^2 \) and \( EARN^3 \) are very small numbers (e.g., for the main estimation interval width of 0.04, \( EARN^2 \) is less than \( 0.04^2 = 0.0016 \), and \( EARN^3 \) is less than \( 0.04^3 = 0.000064 \) in absolute value). We rescale them to ensure the stability of numerical optimization in ML estimation and we present the rescaled estimates \( \alpha_2/10 \) and \( \alpha_3/100 \) in the table.

\(^c\) A positive Vuong \( Z \)-statistic indicates that Model II performs better than Model I. The Vuong test is defined only for ML estimates. Because our two-stage estimates do not have a maximum likelihood interpretation, we cannot use Vuong tests in Panels B and C.
### Table 3. Simulated Type-I error and statistical power in tests of distribution discontinuity determinants: Logit model for small-profit dummy versus our method

<table>
<thead>
<tr>
<th></th>
<th>Simulated sample comprises 5,000 observations in the interval [-0.04, 0.04)</th>
<th>Simulated sample comprises 30,000 observations in the interval [-0.04, 0.04)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$X=1$ increases the earnings management probability by</td>
<td>0%</td>
<td>5%</td>
</tr>
<tr>
<td>Logit</td>
<td>17.6</td>
<td>7.3</td>
</tr>
</tbody>
</table>

Significance test for the effect of $X$ on the earnings management probability in our main specification (Model II with $K^-=K^+=0.01$)

<table>
<thead>
<tr>
<th>Bin width</th>
<th>0.005</th>
<th>0.0025</th>
<th>0.001</th>
<th>ML estimation</th>
</tr>
</thead>
<tbody>
<tr>
<td>0%</td>
<td>5.1</td>
<td>4.4</td>
<td>4.4</td>
<td>4.6</td>
</tr>
<tr>
<td>5%</td>
<td>14.1</td>
<td>14.7</td>
<td>14.4</td>
<td>15.1</td>
</tr>
<tr>
<td>10%</td>
<td>41.5</td>
<td>44.0</td>
<td>44.3</td>
<td>46.2</td>
</tr>
</tbody>
</table>

Main two-stage estimation with

- bin width = 0.005 5.1 14.1 41.5 5.0 53.8 98.9
- bin width = 0.0025 4.4 14.7 44.0 4.4 57.6 99.6
- bin width = 0.001 4.4 14.4 44.3 5.3 57.9 99.5
- ML estimation 4.6 15.1 46.2 5.4 58.8 99.6

The table presents the rejection rates in two-tailed tests of the slope coefficient on $X$ in the logit model and the earnings management probability coefficient $\pi_1$ on $X$ in Model II with a 5% nominal significance level in 1,000 simulated samples. The pre-managed distribution parameters $\alpha_{p,0}$ for $X=0$ are based on the estimates in column 4 of Panel A in Table 2, and $X=1$ changes the pre-managed earnings distribution as shown in Figure 8 (the corresponding parameters are $\alpha_{0,1}^{true} = \alpha_{4,1}^{true} = 0$, $\alpha_{2,1}^{true} = 2$, $\alpha_{3,1}^{true} = 0.5$). The earnings management probability for $X=0$ is $\pi_0^{true} = 0$ for simplicity. The true earnings management process follows Model II. For the Type-I errors in columns 1 and 4, the 95% confidence interval is 3.6% to 6.4%. The code fragment for the two-stage estimation is:

```plaintext
kinkyX simNI X, binwidth(0.005) est_bins(8) em_bins(2) em_type(ii) degree(3) cluster(gvkey)
kinkyX simNI X, binwidth(0.0025) est_bins(16) em_bins(4) em_type(ii) degree(3) cluster(gvkey)
kinkyX simNI X, binwidth(0.001) est_bins(40) em_bins(10) em_type(ii) degree(3) cluster(gvkey)
```
<table>
<thead>
<tr>
<th>Pred. sign</th>
<th>Standard logit estimates</th>
<th>Logit for earnings within ±0.04</th>
<th>Main Model II</th>
<th>Small loss/profit interval</th>
<th>Estimation interval</th>
<th>Bin width</th>
<th>Bin width</th>
<th>Continuous X</th>
<th>ML estimates</th>
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<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
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<tr>
<td>Intercept</td>
<td>-3.254**</td>
<td>-2.015**</td>
<td>-0.024</td>
<td>-0.005</td>
<td>-0.029</td>
<td>-0.016</td>
<td>-0.041*</td>
<td>-0.027</td>
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<tr>
<td></td>
<td>(-102.56)</td>
<td>(-60.78)</td>
<td>(-0.98)</td>
<td>(-0.14)</td>
<td>(-1.42)</td>
<td>(-0.67)</td>
<td>(-1.81)</td>
<td>(-1.09)</td>
<td>(-0.73)</td>
</tr>
<tr>
<td>CA2</td>
<td>+ 0.080*</td>
<td>0.061</td>
<td>0.092***</td>
<td>0.092**</td>
<td>0.092***</td>
<td>0.080**</td>
<td>0.083***</td>
<td>0.104***</td>
<td>0.088***</td>
</tr>
<tr>
<td></td>
<td>(1.89)</td>
<td>(1.32)</td>
<td>(3.00)</td>
<td>(2.13)</td>
<td>(3.56)</td>
<td>(2.53)</td>
<td>(2.92)</td>
<td>(3.27)</td>
<td>(2.86)</td>
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<tr>
<td>CA3</td>
<td>+ 0.156***</td>
<td>0.215***</td>
<td>0.105**</td>
<td>0.072</td>
<td>0.111**</td>
<td>0.108**</td>
<td>0.117***</td>
<td>0.108**</td>
<td>0.107**</td>
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<tr>
<td></td>
<td>(2.67)</td>
<td>(3.32)</td>
<td>(2.35)</td>
<td>(1.14)</td>
<td>(2.99)</td>
<td>(2.29)</td>
<td>(2.70)</td>
<td>(2.32)</td>
<td>(2.41)</td>
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<tr>
<td>CL2</td>
<td>+ -0.301***</td>
<td>0.072</td>
<td>0.054*</td>
<td>0.112***</td>
<td>0.026</td>
<td>0.051*</td>
<td>0.082***</td>
<td>0.044</td>
<td>0.054*</td>
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<td></td>
<td>(-7.29)</td>
<td>(1.62)</td>
<td>(1.78)</td>
<td>(2.68)</td>
<td>(1.02)</td>
<td>(1.66)</td>
<td>(2.91)</td>
<td>(1.40)</td>
<td>(1.80)</td>
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<tr>
<td>CL3</td>
<td>+ -0.685***</td>
<td>0.146**</td>
<td>0.039</td>
<td>0.075</td>
<td>0.013</td>
<td>0.043</td>
<td>0.069*</td>
<td>0.037</td>
<td>0.035</td>
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<td></td>
<td>(-12.17)</td>
<td>(2.34)</td>
<td>(0.89)</td>
<td>(1.21)</td>
<td>(0.36)</td>
<td>(0.93)</td>
<td>(1.65)</td>
<td>(0.82)</td>
<td>(0.80)</td>
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<tr>
<td>COGS2</td>
<td>+ -0.051</td>
<td>-0.047</td>
<td>0.042</td>
<td>0.072*</td>
<td>0.029</td>
<td>0.042</td>
<td>0.036</td>
<td>0.034</td>
<td>0.038</td>
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<tr>
<td></td>
<td>(-1.30)</td>
<td>(-1.14)</td>
<td>(1.51)</td>
<td>(1.84)</td>
<td>(1.23)</td>
<td>(1.46)</td>
<td>(1.40)</td>
<td>(1.18)</td>
<td>(1.35)</td>
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<tr>
<td>COGS3</td>
<td>+ -0.111***</td>
<td>-0.032</td>
<td>0.095***</td>
<td>0.109**</td>
<td>0.088***</td>
<td>0.080**</td>
<td>0.067**</td>
<td>0.094**</td>
<td>0.095***</td>
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<tr>
<td></td>
<td>(-2.57)</td>
<td>(-0.67)</td>
<td>(2.67)</td>
<td>(2.67)</td>
<td>(2.89)</td>
<td>(2.20)</td>
<td>(1.98)</td>
<td>(2.57)</td>
<td>(2.67)</td>
</tr>
<tr>
<td>RD2</td>
<td>+ 0.079*</td>
<td>-0.006</td>
<td>0.042</td>
<td>0.032</td>
<td>0.017</td>
<td>0.044</td>
<td>0.041</td>
<td>0.046</td>
<td>0.046</td>
</tr>
<tr>
<td></td>
<td>(1.64)</td>
<td>(-0.11)</td>
<td>(1.14)</td>
<td>(0.63)</td>
<td>(0.57)</td>
<td>(1.15)</td>
<td>(1.18)</td>
<td>(1.21)</td>
<td>(1.25)</td>
</tr>
<tr>
<td>RD3</td>
<td>+ 0.133***</td>
<td>0.115**</td>
<td>0.060**</td>
<td>0.038</td>
<td>0.059**</td>
<td>0.061**</td>
<td>0.072**</td>
<td>0.067**</td>
<td>0.059**</td>
</tr>
<tr>
<td></td>
<td>(4.00)</td>
<td>(3.25)</td>
<td>(2.44)</td>
<td>(1.11)</td>
<td>(2.85)</td>
<td>(2.39)</td>
<td>(3.10)</td>
<td>(2.65)</td>
<td>(2.41)</td>
</tr>
<tr>
<td>CA</td>
<td>+</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>CL</td>
<td>+</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>COGS</td>
<td>+</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>RD</td>
<td>+</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\alpha_0 \ldots \alpha_{p,M})</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>(\text{adj. R}^2) (%)</td>
<td>-20,505.23</td>
<td>-13,526.29</td>
<td>0.474</td>
<td>0.472</td>
<td>0.471</td>
<td>0.386 a</td>
<td>0.597 a</td>
<td>0.977 a</td>
<td>0.186 a</td>
</tr>
<tr>
<td>lnL</td>
<td>152,586</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>24,685</td>
<td>55,761</td>
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<tr>
<td>N</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
<td>34,483</td>
</tr>
</tbody>
</table>

The table presents estimates of the logit model for the small-profit dummy (columns 1–2) and our Model II with explanatory variables \(X\) (columns 3–11) for alternative empirical definitions. The logit estimates in column 1 use the full sample following the standard approach in the literature, while the logit estimates in column 2 use the restricted sample with scaled earnings in the ±0.04 interval for comparability with our main Model II estimates in column 3. For brevity, in columns 3–11 we only tabulate the coefficients \(\pi\) that determine the earnings management probability as a function of \(X\); each variable in \(X\) also affects the untabulated polynomial coefficients \(\alpha_0(X) \ldots \alpha_p(X)\) in the probability density function (6a) of pre-managed earnings.
\textit{EARN} is net income (NI) scaled by the lagged market value of equity (PRCC\_F$\times$CSHO). Following Burgstahler and Dichev (1997), \textit{CA} is the ratio of non-cash current assets (ACT–CHE) to the market value of equity (PRCC\_F$\times$CSHO), and \textit{CL} is the ratio of current liabilities (LCT–DLC) to the market value of equity. Following Burgstahler and Chuk (2017), \textit{COGS} is the ratio of cost of goods sold (COGS) to total assets (AT), and \textit{RD} is the ratio of R&D expense (XRD) to total assets. We replace missing R&D with zero following Hirshleifer et al. (2012). \textit{CA}_2, \textit{CA}_3, \textit{CL}_2, \textit{CL}_3, \textit{COGS}_2, \textit{COGS}_3, \textit{RD}_2, \text{and} \textit{RD}_3 \text{are dummy variables for the second and third terciles of} \textit{CA}, \textit{CL}, \textit{COGS,} \text{and} \textit{RD}, \text{respectively.} \text{53\% of observations have zero or missing R&D; to avoid an arbitrary division of these observations between the bottom and middle terciles, we assign all of them to the bottom “tercile” of R&D and shrink the middle tercile. All explanatory variables are lagged.}

The code fragment for column 3 is \texttt{kinkyX NI CA2 CA3 CL2 CL3 COGS2 COGS3 RD2 RD3, binwidth(0.0025) est_bins(16) em_bins(4) em_type(ii) degree(3) cluster(gvkey)}. The dummy variables for the base tercile are omitted to avoid perfect multicollinearity. Columns 4 and 5 use the option \texttt{em_bins(2)} and \texttt{em_bins(6)}, respectively. Columns 6 and 7 use the option \texttt{est_bins(12)} and \texttt{est_bins(24)}, respectively. Column 8 uses the options \texttt{binwidth(0.005) est_bins(8) em_bins(2)}. Column 9 uses the options \texttt{binwidth(0.001) est_bins(40) em_bins(10)}. Column 10 replaces the tercile dummies with continuous explanatory variables. We do not package the custom ML code for column 11 as a general-purpose estimation tool for future research.

\textsuperscript{a} The adjusted $R^2$s in columns 6–9 are not comparable to the other columns because of differences in sample size and/or bin dummy definitions.
Table 5. The relation between beginning-of-period net operating assets (NOA) and meeting or beating analyst forecasts

<table>
<thead>
<tr>
<th>Logit for the full sample</th>
<th>Logit for subsample with ( EPS ) ( \in [-5c, 5c] )</th>
<th>Restricted Model I</th>
<th>Main estimates for Model I</th>
<th>Robustness checks</th>
<th>( EPS ) ( \in [-4c, 4c] )</th>
</tr>
</thead>
<tbody>
<tr>
<td>meet or beat</td>
<td>meet or just beat by 1c</td>
<td>meet or beat</td>
<td>meet or just beat by 1c</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-1.550***</td>
<td>-1.483***</td>
<td>-0.863***</td>
<td>-0.824***</td>
<td>-0.260***</td>
</tr>
<tr>
<td>( NOA )</td>
<td>-0.027***</td>
<td>-0.017***</td>
<td>-0.033***</td>
<td>-0.013***</td>
<td>-0.013***</td>
</tr>
<tr>
<td>( PB )</td>
<td>-7.84***</td>
<td>-4.58***</td>
<td>-7.75***</td>
<td>-3.00***</td>
<td>-3.15***</td>
</tr>
<tr>
<td>( BIG5 )</td>
<td>-1.155***</td>
<td>0.608***</td>
<td>-0.917***</td>
<td>0.322***</td>
<td>-0.046</td>
</tr>
<tr>
<td>( LTGN_RISK )</td>
<td>0.058**</td>
<td>-0.012</td>
<td>0.069*</td>
<td>0.021</td>
<td>0.011</td>
</tr>
<tr>
<td>( PB )</td>
<td>0.034***</td>
<td>0.030***</td>
<td>0.027***</td>
<td>0.011***</td>
<td>0.013***</td>
</tr>
<tr>
<td>( ANALYSTS )</td>
<td>0.058***</td>
<td>0.058***</td>
<td>0.044***</td>
<td>0.025</td>
<td>0.016</td>
</tr>
<tr>
<td>( PREV_MB )</td>
<td>-3.65***</td>
<td>0.527***</td>
<td>0.776***</td>
<td>0.392***</td>
<td>0.410***</td>
</tr>
<tr>
<td>( CV_FORECAST )</td>
<td>-0.235***</td>
<td>-0.082**</td>
<td>-0.367***</td>
<td>-0.184***</td>
<td>-0.199***</td>
</tr>
<tr>
<td>( DOWN_REV )</td>
<td>-0.346***</td>
<td>0.138***</td>
<td>-0.359***</td>
<td>0.025</td>
<td>-0.106***</td>
</tr>
<tr>
<td>( SALES_GRW )</td>
<td>-1.537***</td>
<td>0.611***</td>
<td>0.461***</td>
<td>0.794***</td>
<td>0.124***</td>
</tr>
<tr>
<td>( ROE )</td>
<td>0.528***</td>
<td>0.611***</td>
<td>0.461***</td>
<td>0.794***</td>
<td>0.124***</td>
</tr>
<tr>
<td>( AROE )</td>
<td>-0.346***</td>
<td>0.138***</td>
<td>-0.359***</td>
<td>0.025</td>
<td>-0.106***</td>
</tr>
<tr>
<td>( MKT_CAP )</td>
<td>0.157***</td>
<td>-0.034***</td>
<td>0.117***</td>
<td>-0.008</td>
<td>0.021*</td>
</tr>
</tbody>
</table>
| \( 

Coefficients \( \alpha \) in the pre-managed distribution

\( \alpha_{0,NOA} \) | 0.001 | 0.001 | 0.001 | 0.001 |
| \( \alpha_{1,NOA} \) | (0.59) | (0.59) | (0.76) | (0.58) |

\( \alpha_{0,NOA} \) | -0.112*** | -0.112*** | -0.130*** | -0.150*** |
<p>| ( \alpha_{1,NOA} ) | (-5.35) | (-5.35) | (-6.53) | (-4.11) |</p>
<table>
<thead>
<tr>
<th></th>
<th>Logit for the full sample</th>
<th>Logit for subsample with $\text{EPS ~surprise} \in [-5c, 5c)$</th>
<th>Restricted Model I</th>
<th>Main estimates for Model I</th>
<th>Robustness checks</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>meet or beat</td>
<td>meet or just beat by 1c</td>
<td>meet or beat</td>
<td>meet or just beat by 1c</td>
<td>meet vs miss by 1c</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>$\alpha_{2,NOA}$</td>
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<tr>
<td>other $\alpha_{p,j}$</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$F$-statistic for $\alpha_{0,NOA} \ldots \alpha_{p,NOA}$</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>lnL</td>
<td>-28,377.16</td>
<td>-27,934.61</td>
<td>-18,807.09</td>
<td>-21,943.10</td>
<td>9.92***</td>
</tr>
<tr>
<td>adj. $R^2$ (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>48,054</td>
<td>48,054</td>
<td>32,707</td>
<td>32,707</td>
<td>32,707</td>
</tr>
</tbody>
</table>

The table presents the estimates for the logit model and our Models I and II. In untabulated exploratory analysis across various specifications for EPS surprise data, Model I has consistently higher adjusted $R^2$ than Model II, which leads us to use Model I as the main specification for EPS surprises. The sample and variable definitions follow Barton and Simko (2002). The sample is the intersection of quarterly Compustat and IBES data during 1993–1999. The dependent variables are based on the EPS surprise, computed as actual EPS minus the latest consensus mean EPS forecast for the quarter, both rounded to the nearest penny. Both actual EPS and consensus EPS forecast are from split-unadjusted data. The dependent variable “meet or beat” in columns 1 and 3 equals one for non-negative EPS surprises of any size and equals zero otherwise. The dependent variable “meet or just beat” in columns 2 and 4 equals one for EPS surprises of 0 and 1 cents and equals zero otherwise. The dependent variable for Models I and II in columns 5–9 is the EPS surprise in cents. In columns 5–7 and 9, we interpret EPS surprises of 0 and 1 cents (-1 and -2 cents) as possible outcomes (triggers) of earnings management in Models I and II. In column 8, only EPS surprises of 0 cents (-1 cent) are interpreted as possible outcomes (triggers) of earnings management. Unless stated otherwise, the estimation sample for Models I and II is restricted to EPS surprises in the $(-5 \text{ cents}, 5 \text{ cents})$ interval. NOA is the ratio of net operating assets (CEQQ−CHEQ+DLCQ+DLTTQ) to sales (SALEQ) at the beginning of the quarter. SHARES is total shares outstanding (CSHOQ). BIG5 is a dummy variable for a Big-5 auditor (AU=1,4,5,6,7). PB is the market-to-book ratio (CEQQ/[PRCCQ*CSHOQ]). LTGN_RISK is a dummy variable for high litigation risk industries (SIC codes 2833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, 8731–8734). ANALYSTS is the number of analysts (NUMEST). PREV_MB is a dummy for meeting or beating the consensus forecast in the prior quarter. CV_FORECAST is the coefficient of variation in the analysts’ most recent forecasts for the quarter. DOWN_REV is the number of downward forecast revisions (total NUMDOWN during the quarter). SALES_GRW is sales (SALEQ) growth relative to quarter $t$–4. ROE is return on equity (NIQ/CEQQ). $\Delta$ROE is change in ROE relative to quarter $t$–4. MKT_CAP is the natural logarithm of the market value of equity (PRCCQ*CSHOQ). The code fragment for column 6 is kinkyX surprise Xvars, binwidth(0.01) est_bins(5) em_bins(2) em_type(i) degree(4) cluster(gvkey), where Xvars is the full list of explanatory variables in Table 5. Columns 7–9 use options em_type(ii), em_bins(1), and est_bins(4), respectively. Column 5 uses a custom modification of our estimation command.
Appendix. Usage instructions for our estimation command

The general syntax is

\[ \text{kinkyX depVar [explVars] [if], binwidth(#) est_bins(#) em_bins(#) [em_binsMinus(#)] em_type(#)} \]

\[ \text{degree(#) cluster(varname) [ smooth(name) kinky(name) sloppy predict(name) graph(name) graphtitle(string) ] } \]

where the command name and parameter names are case-sensitive, square brackets indicate optional parameters, \# indicates a number, and

depVar = Dependent variable (e.g., scaled net income or EPS surprise). It must be scaled appropriately (Burgstahler and Chuk, 2015), even when the model includes size controls.
explVars = Explanatory variables (optional).
if = Standard Stata sample selection conditions (optional). These condition are applied in addition to the estimation interval restriction shown in Panel C of Figure 3.
binwidth(#) = Bin width for the discretization of earnings.
est_bins(#) = Number of estimation bins on each side of zero.
em_bins(#) = Number of small-profit bins; if the option em_binsMinus is not specified, then em_bins also defines the number of small-loss bins. Because em_bins (and em_binsMinus) is the same across all firm-years, it is important to choose a scaling variable that is approximately proportional to the range of earnings likely affected by earnings management, so that the constant interval width assumption is reasonable for the scaled dependent variable.
em_binsMinus(#) = Number of small-loss bins (optional, only used to define asymmetric small-loss and small-profit intervals, i.e., \( K^- \neq K^+ \)). If em_binsMinus is not specified, then em_bins defines symmetric small-loss and small-profit intervals.
em_type(string) = Specify em_type(i) for Model I or em_type(ii) for Model II.\(^{32}\)
degree(#) = Degree of the polynomial.
cluster(varname) = Cluster variable for the standard errors (e.g., a firm index or an industry index). This is a required parameter because the computation of two-stage standard errors always includes clustering of bin-level observations (at least at the firm-year level) to capture cross-bin correlations within each firm-year.
smooth(name) = Stem for the variable names associated with the smooth polynomial terms of the pre-managed distribution (optional). E.g., if the model includes explanatory variables x1–x5 and has the options degree(3) and smooth(sm), then the polynomial variables and the associated coefficients in the estimation output will be named sm0...sm3 (stand-alone polynomial terms), and sm0_x1...sm0_x5,..., sm3_x1...sm3_x5 (interactions with the x-s). Any existing variables with these names will be overwritten. The default stem is “smooth”. This option is useful for creating more interpretable or more compact variable names.
kinky(name) = Stem for the variable names associated with the discontinuity coefficients (optional). E.g., if the model includes explanatory variables x1–x5 and has the option kinky(k), then the discontinuity variables and the associated coefficients in the estimation output will be named k, k_x1...k_x5. Any existing variables with these names will be overwritten. The

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32 For backward compatibility, this option can also be set to em_type(0) for Model I or em_type(1) for Model II.
default stem is “kinky”. This option is useful for creating more interpretable or more compact variable names.
sloppy = Use a quick-and-dirty single-stage standard error computation instead of the econometrically correct, but slower, two-stage standard errors. This option can be useful in exploratory analysis, because single-stage standard errors tend to be reasonably close to the correct two-stage standard errors, but it should never be used for the final reported estimates.
predict(name) = Create a new variable with the predicted probability of the observed earnings bin for each firm-year.
graph(name) = Plot a predicted-vs-actual distribution graph (optional). This option is relevant only in models without explanatory variables. A non-empty graph name must be specified (e.g., use graph(Fig1_PanelA) not graph()). It will overwrite any existing graph of the same name. To better represent the underlying continuous distribution, this option uses the original continuous values of the dependent variable depVar (rather than the discrete bin midpoints) for the graph. If the dependent variable is discrete (e.g., EPS surprise in cents), redefine it as the midpoint of the respective bin to correctly plot the predicted values for each bin.
graphtitle(string) = Graph title (optional).

To get started, download kinkyX.ado and save it either in the Stata directory for user-installed third-party ado files (e.g., C:\ado\plus per standard Stata directory definitions for Windows) or in the code directory for your current project. In the latter case, you should cd to the relevant directory (e.g., cd c:\MyCurrentProject\code) before calling kinkyX to let Stata find the ado file.

Essential usage tips:
• If the command crashes or is stopped in the middle of execution for any reason, the data may include multiple copies of the original observations. The reason is that each observation in the sample is converted into multiple bin-level observations during estimation, and the original data are restored only when the command successfully reaches the end of estimation. Reload the original sample before proceeding.
• In case of perfect multicollinearity (e.g., if the model includes a full set of year dummies in addition to the intercept), the command does not automatically discard redundant explanatory variables. Perfect multicollinearity will trigger conformability errors. To resolve such errors, remove the redundant collinear variables from the model.
• For discrete dependent variables that are exactly at the lower bound of the bin intervals, you should add a small positive value (e.g., 0.000001) to the variable to ensure that it is always assigned to the correct bin. For example, because floating-point computer variables have finite precision, an observation with a theoretical EPS value of $0.01 can have a slightly smaller actual value such as 0.0099999999. Because 0.01 is in the (0.01, 0.02) interval, it is assigned to the 1-cent bin, using standard Burgstahler and Dichev (1997) bin interval definitions. In contrast, because 0.0099999999 is in the [0, 0.01) interval, it is assigned to the 0-cent bin. Adding a small positive number such as 0.000001 resolves this problem because values such as 0.0099999999+0.000001 are guaranteed to be in the correct bin interval (0.01, 0.02).33 Alternatively, the dependent variable can be redefined as the midpoint of the respective bin.

33 This issue is not specific to our command, and researchers should be aware of it whenever they use floating-point computations. We do not automatically adjust 0.0099999999 to 0.01 because such an adjustment is not always reasonable for continuous dependent variables (e.g., $999,999.99 is not necessarily equivalent to $1,000,000).
To analyze a discontinuity at a benchmark other than zero, define the dependent variable as the deviation from the benchmark and scale it appropriately. If the research context involves incentives to move from just above to just below the benchmark (e.g., to avoid a higher labor income tax bracket or more stringent audit requirements above a size threshold), invert the sign of the dependent variable, such that managed values just below the benchmark correspond to the interval just above zero for the inverted dependent variable in estimation.

Scaled earnings $EARN$ in our main empirical examples is a small number in the ±0.04 interval, and the higher-order polynomial terms are very small numbers (e.g., $EARN^2$ is less than $0.04^2 = 0.0016$, and $EARN^3$ is less than $0.04^3 = 0.000064$ in absolute value). These very small values for the polynomial terms are likely to be accompanied by very large polynomial coefficients. To improve the readability of the coefficients, the estimation procedure rescales the polynomial terms $EARN^p$ in both estimation stages as $EARN^p \times 10^{p-1}$. The reported coefficients correspond to these rescaled variables.

In models with a large number of explanatory variables, the computation of two-stage standard errors can be time-consuming because the computational burden for major code parts is proportional to the number of variables squared. The option `sloppy` can significantly speed up the exploratory analysis, but it should not be used for the final reported estimates.

Specifications with a large number of bins can considerably slow down the estimation. For example, if the original sample comprises 100,000 firm-year observations with earnings in the estimation interval, and the estimation interval is discretized into 200 bins, then the number of firm-year-bin observations in estimation is $100,000 \times 200 = 20,000,000$. Some of the estimation steps (e.g., sorting) can become very slow for such a large dataset, and the estimation speed can deteriorate further if the data size exhausts the available random access memory (RAM). To conserve RAM, discard unused variables before estimation. If this does not speed up the estimation to an acceptable level, then use a less detailed bin grid with fewer bins.